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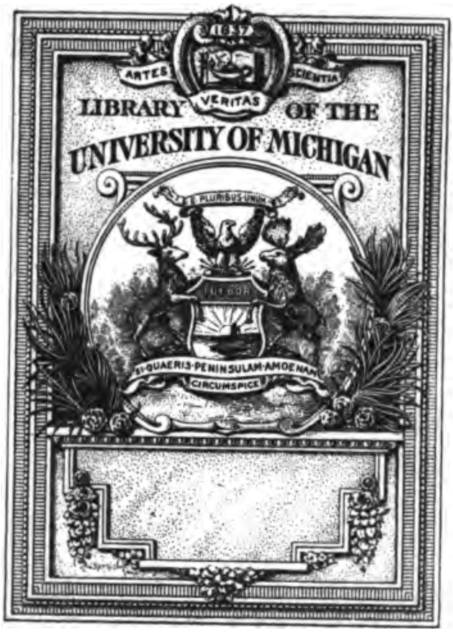

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THE

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MEDICO-CHIRURGICAL

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1859.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

Course of Lectures on the Physiology and Pathology of the Central Nervous System, delivered before the Royal College of Surgeons of England, in May, 1858, *by* E. BROWN-SÉQUARD, M.D. Illustrated by numerous Engravings, representing the principal Experiments and Pathological Cases. (From the 'Lancet,' 1858–59).

IN a Lecture delivered by Schiller at Jena in 1789, on the study of Universal History, the poet draws a striking contrast between the empiric "or trader in science," and the "real philosopher" or lover of wisdom; and in no respect is that contrast more remarkable or more true, than as to the reception which each gives to new discoveries. Of the former he remarks, "Every extension of the boundaries of the science by which he earns his bread is regarded by him with anxiety, since it occasions him fresh labour, or renders his former labours useless; every important innovation or discovery alarms him, for it breaks down those old school formulæ which he had taken so much pains to acquire: it endangers the entire produce of the toil and trouble of his whole previous life." On the other hand, "new discoveries in the field of his activity, which depress the trader in science, enrapture the philosopher. Perhaps they fill a chasm which the growth of his ideas had rendered more wide and unseemly, or they place the last stone, the only one wanting to the completion of the structure of his ideas. But even should they shiver it into ruins,—should a new series of ideas, a new aspect of Nature, a newly-discovered law in the physical world, overthrow the whole fabric of his knowledge,—*he has always loved Truth better than his system,* and gladly will he exchange an old and defective form for a new and fairer one."

We have thought it not inappropriate to call the attention of our readers to the noble sentiment we have italicized, by way of preface to the enquiry through which we purpose to conduct them, as to the merits of the most important among the numerous sets of researches carried on by one of the most distinguished experimental physiologists of our time,—namely, those investigations into the Physiology and Pathology of the Nervous System, of which M. Brown-Séquard gave an account (with experimental illustrations) in a course of lectures delivered by him last summer at St. Bartholomew's Hospital, and afterwards repeated at the Royal College of Surgeons, by the special request of its Council. The slight notice which we have already given of the results of some of these investigations (vol. xvii.) by no means relieves us from the responsibility of now critically examining into the value of the entire series; and we are sure that no one is more desirous than M. Brown-Séquard himself, that their merits should be tested by a careful scrutiny into the validity of his methods, the accuracy of his statements, and the justice of his deductions. If they prove able to bear such an ordeal, they are fairly entitled to take rank as physiological verities, notwithstanding their contrariety to much that has been previously received as true. We are not among those who are disposed to stand by a doctrine, merely because it is currently accepted, or because it has been taught by men whose authority we hold in the highest respect. Such reasons for upholding it are good against the slight assaults of such opponents as are themselves deserving of but little credit; but when a formal contradiction is given even to our most cherished beliefs, by men whose patient search for truth, and whose capacity for its discovery, command our highest respect, we do not see that any other honest course is left to us, than that of candidly weighing the new facts and arguments against the old, assigning to each its worth according to the best judgment we can form, and striking the balance without more regard to our previous conviction than is found to be justified by the stability of the base on which it rests.

Now, in endeavouring to form this kind of estimate of the merits of M. Brown-Séquard's doctrines, as compared with those which have been currently accepted in Physiology, we must confess *in limine* that we are not able to speak from personal experience as to more than a small part of his experimental results. But we feel scarcely less readiness to credit those which we have not ourselves witnessed, than to trust our own observation as to those which we have; so great is our confidence in M. Brown-Séquard's aptitude for observation, which allows no phenomenon to escape him, in the exactness with which he not merely performs his operations but subsequently determines the precise nature of the lesion he has inflicted, and in the scrupulous truthfulness with which he records every fact which bears upon the question at issue, whether or not it is favourable to his own view of the case. This confidence is founded in part on the careful watch we have ourselves kept on M. Brown-Séquard's scientific course, from the time when his researches first began to attract attention about twelve years ago, and on the estimate we have formed of his character from the personal acquaintance which it was our good fortune to commence with him not long afterwards; and in part on the fact that several of his capital experiments have been performed before a Commission of the Société de Biologie, consisting of MM. Cl. Bernard, Bouley, P. Broca, Giraldès, Goubaux, and Vulpian, whose report drawn up by M. Paul Broca, bears the fullest testimony to the accuracy with which he has described both the operations and their results.

The Course of Lectures which M. Brown-Séquard has published in the columns of the 'Lancet,' is much fuller in detail than that which he actually delivered at the College, especially in regard to the evidence supplied by Pathological observation; still on many points it is far from being complete; and we think that we shall do most justice both to him and to our readers, by limiting our discussion of his doctrines to the following questions, which we shall take up in succession:—

1. The relative functions of the Anterior and Posterior Roots of the Spinal Nerves.
2. The channel through which Sensory Impressions are conveyed from the Spinal Nerves to the Encephalon.
3. The Decussation of the Conductors of Sensory Impressions in the Spinal Cord itself.
4. The channel through which Motor power is conveyed from the Encephalon to the Spinal Nerves.
5. The motor action of the Sympathetic system on the walls of the blood vessels.
6. The action of the Nervous System (especially, though not exclusively, its Sympathetic division) upon the Organic functions of Nutrition and Secretion.

I. The first question to which we have to apply ourselves,—that of the *Relative Functions of the Anterior and Posterior Roots of the Spinal Nerves,*—is one which, although commonly supposed to have been conclusively settled by Sir C. Bell and his immediate followers, still presents certain points of difficulty. And it is obvious that until these shall have been elucidated, no conclusions regarding the functions of the different columns of the Spinal Cord can be satisfactorily drawn either from experiment or from pathological observation. It was early observed by Magendie, that although pain is obviously the principal result of irritation of the posterior roots, and muscular contraction the principal result of irritation of the anterior roots, yet that local movements are induced by irritation of the posterior roots, and that pain is obviously excited by irritation of the anterior; and hence he was for a time erroneously led to the conclusion that each set of roots is subservient to both functions, the posterior being chiefly but not exclusively sensory, and the anterior chiefly but not exclusively motor. Further enquiry, however, has served to demonstrate the erroneous nature of this conclusion; the movements excited by irritation of the posterior roots, and the pain induced by irritation of the anterior, being clearly due in each case to the participation of the nerve-roots not thus experimented on. For if, before irritating the posterior roots, the operator divide the anterior, he can excite no movement by irritation of the former; and if, instead of irritating the posterior roots whilst connected with the spinal cord, he divide those roots, he finds that motion can only be excited by irritation of their proximal segment, no irritation of their distal segment having the least power of calling it forth. On the other hand, if, before irritating the anterior roots, the operator divide the posterior, he finds that irritation of the anterior no longer gives signs of pain; and if, instead of irritating the anterior roots whilst in connexion with the spinal cord, he divide those roots, he finds that signs of pain can only be excited by irritating their distal segment, no such indications being called forth by irritation of their proximal segment. It is obvious from these facts, that the movements excited by irritation of the posterior nerve roots are in reality *reflex* actions; the irritation being first propagated by the afferent fibres to the *central* organs, and being thence reflected through the motor to the muscles. And it would at first seem equally obvious, that the sense of pain excited by irritation of the anterior roots is the result of the transmission of the effect of the irritation to the peripheral organs, through which it is reflected back, as it were, to the central.

Such was the explanation of the phenomenon that was originally adopted by Magendie, who designated his form of nervous activity as "recurrent sensibility." Owing, however, to the obvious difficulty attending Magendie's interpretation, another explanation proposed by Kronenberg and Pappenheim has met with much acceptance—namely, that the sensitiveness of the anterior roots is due to their containing fibres which pass back into them from the posterior at the point of junction of the two sets of roots, so that when they are irritated, the sensory current passes peripherally no further than that junction, being then transmitted direct to the spinal cord through the posterior roots. But if this were the case, it is obvious that such transmission of the sensory current would not be interrupted by section of the trunk of the nerve *beyond* the junction of the two roots; whilst the fact has been demonstrated by the experiments of Magendie, Cl. Bernard, Volkman, Schiff, and Brown-Séquard himself, that the pain-inducing effect of the irritation proceeds as far as the peripheric extremity of the nerve-fibres, no signs of pain being given except when the continuity is complete between the part of the root or trunk irritated and the muscles which that trunk supplies. Further, it appears from these experiments, that the amount of pain induced is proportional to the force of the muscular contraction called forth; and thus it seems obvious that the effect of the irritation is really transmitted along the *motor* fibres; and that the excitement of a current in the sensory fibres is consecutive upon the action of the muscles to which they are distributed.

A very ingenious mode of accounting for this excitement is proposed by M. Brown-Séquard. Every one is now acquainted with the phenomenon of "induced contraction," first discovered by Matteucci, in which the contraction of one muscle calls forth the contraction of a second whose *motor* nerve has been dissected out and laid upon the first; and it is certain that, whether the excitation of the nerve-current be due, as maintained by Matteucci, to a sort of galvanic discharge in the contracting muscle, or, as asserted by Du Bois-Reymond, to a diminution of the ordinary muscular current at the moment of contraction, it results from *some* change in the electric state of the muscle. To this change M. Brown-Séquard considers that we may attribute the excitement of that current in the *afferent* nerves of the muscle itself, which makes us conscious of the state of its contraction, and thus affords the sensations by which our voluntary efforts are guided; whilst it is by a more intense excitement of the same kind, that the pain of *cramp* is produced. This pain, according to him, depends upon the degree of resistance which is opposed to the contraction; and he adduces the following well-known physiological and pathological phenomena in support of his doctrine:—the uterine pains are far more severe when the parturient efforts are antagonized by the rigidity of the os uteri or of the external parts, than they are when the contractions are effectual in overcoming the resistance; in cases of anal fissure, the pain due to the spasm of the sphincter is increased when there is a resistance to the contraction, and ceases when that resistance is destroyed by section of the sphincter; and in cases of painful contracture (by which we suppose is meant permanent contraction) of other muscles, the pain increases when the muscles are elongated, but disappears entirely or almost entirely when the resistance is completely or almost completely destroyed by division of the tendon. Two other facts occur to us, which harmonize well with this view. The experiments of M. Du Bois-Reymond, as we can ourselves testify, clearly show that the amount of electric disturbance depends, not on the amount of muscular contraction produced, but on the amount of contractile effort put forth; this effort being facilitated by using means to resist contraction, as by grasping in the hand a full-sized stick. So, again, it is well known that the reflex expulsive efforts of parturition, defecation, urination, &c., become more violent the more they are resisted; the explanation of which seems partly (at any rate) to be in the intensification of the contraction by the additional stimulation conveyed to the spinal cord through the afferent nerves of the contracting muscle itself. And we may further notice, that when the action of any set of muscles is ordinarily dependent upon guiding sensations originating *out of* themselves, but from defect of these is brought to depend upon sensations originating *in* themselves, those sensations are painful. Of this any of our readers may convince himself by attempting to rotate his eye-balls when he has completely excluded the light by closing the lids and covering them with his hand. And it is also a matter of experience, that when the deaf-mute is taught to speak, it is in the first instance at the expense of considerable uneasiness in the larynx; this uneasiness, in the well-known case of Dr. Kitto, having amounted to a degree of pain sufficient to induce him to abandon all attempts to speak for several years.

Whether or not M. Brown-Séquard's explanation of the phenomenon known as "recurrent sensibility" be correct, it may be taken as proved by experimental inquiry that the recurrence is only apparent, and that the pain occasioned by irritation of the anterior roots of the spinal nerves is really *felt* through the posterior, and is of the nature of the pain of cramp, being dependent on the muscular contraction which that irritation calls forth. Hence the phenomenon of "recurrent sensibility" does not furnish any more real objection to Sir C. Bell's doctrine of the distinctness of the motor and sensory nerves, than does the phenomenon of reflex movement; the excitation of either set of nerves producing a certain excitation of the other, in the one case through the peripheral organs, in the other through the central. There are certain results of experiments on frogs, however, which it seems difficult to explain except upon the hypothesis that some, at least, of the nerve-fibres which minister to the muscular sensations that serve for the guidance of movements, pass to the spinal cord through the anterior roots; but we are not sure that they necessarily require this inference, as we are disposed to think that the conditions of reflex and of voluntary movement as to this particular are very different, and that the actions cited by M. Brown-Séquard as having been performed after all the posterior roots of the spinal nerves had been cut, do not necessarily imply the existence of guiding sensations. At any rate, the harmonious results of the best-conducted experiments on birds and mammals appear fully to justify the conclusion, that the transmission of impressions which occasion *pain* takes place through the posterior roots alone.

II. From the study of the relative endowments of the anterior and posterior roots of the spinal nerves, M. Brown-Séquard naturally passes to that of the relative functions of the several columns of the Spinal Cord; and he makes it his first business to ascertain *the channel through which Sensory impressions are transmitted to the encephalon.* Every one who is acquainted with the history of Sir C. Bell's inquiries, must be aware, that when he had established the relative functions of the anterior and posterior roots of the spinal nerves, he at first attributed the corresponding functions to the anterior and posterior columns of Spinal Cord; regarding the latter as the conductors of sensory impressions upwards from the nerve-roots to the encephalon, and the former as the conductors of motor impulses downwards from the encephalon to the nerve-roots. But it is equally well known amongst his followers in this country, that he did not continue to entertain this idea; but that he was led, both by anatomical enquiry and by pathological observation, to consider the lateral columns as the parts of the cord chiefly if not entirely concerned in the transmission of sensory impressions, and partly also in that of motor impulses. Of late, however, the experiments of M. Longet, who began by opposing the doctrines of Sir C. Bell, and ended by becoming more Bellian than Sir Charles himself, have been generally accepted, both in France and England, as justifying his reversion to Bell's former doctrine, notwithstanding the difficulty of reconciling it with pathological phenomena.

The experiments of M. Brown-Séquard, however, have led him to very different conclusions; which our readers will be better prepared to appreciate, if we first remind them of what careful anatomical research has within the last few years ascertained, as to the course of the bundles of nerve-fibres that are the continuations of the posterior roots, after their entrance into the spinal cord. None of the inquirers who have devoted themselves to this difficult investigation have done so much to elucidate the structure of the cord as Mr. Lockhart Clarke;* and what others have been able to demonstrate harmonizes fully with his results. Of those fibres of the posterior roots of either side which are brought into view by a *transverse* section, some appear directly continuous with the fibrous strands of the posterior column, and others with those of the lateral column of the same side; whilst others at once enter the posterior cornu of the grey substance of the cord. Of the latter some may be traced through the grey substance towards the anterior cornu of the same side; and appear to be continuous (whether they are so directly, or through the intervention of vesicular substance, is not yet certain) with motor fibres proceeding from it into the anterior roots; but the larger proportion seem to lose themselves in the grey substance. Of these it may be pretty certainly affirmed that some come into connexion with the cells of the vesicular matter; whilst others pass out into the lateral and posterior columns, a considerable proportion passing across the commissure, and, after passing through the grey substance of the other side, emerging into its posterior and lateral columns, some fibres appearing also to pass towards the anterior cornu of that side. When the course of the fibres of the posterior roots is brought into view by *longitudinal* sections of the spinal cord, it becomes apparent that some of the fibres which enter the posterior columns pass in the first instance *upwards*, and others *downwards*, in those columns; but that in neither case do they pursue this course far, their direction being soon changed to the transverse, so that they enter the posterior part of the grey substance a little above or a little below the fibres which passed into it horizontally from the nerve-roots. To the accuracy of Mr. Lockhart Clarke's statements on these points, we can bear testimony from our own examination of his preparations; and their very remarkable accordance with the conclusions drawn by M. Brown-Séquard from his experimental inquiries, is not a little confirmatory of our belief in the latter, especially when it is considered that the greater part of these results were obtained without any knowledge of what anatomical investigation could reveal, M. Brown-Séquard's researches having been carried on contemporaneously with those of Mr. Lockhart Clark.

In the attempt to determine experimentally what is the part of the spinal cord which serves to convey sensory impressions, it is of great importance to observe certain precautions, the neglect of which is very likely to lead to erroneous conclusions. The first thing to be guarded against, is the disturbance of the functions of the spinal cord which often results from the operation of laying it bare; diminution or even loss of both sensibility and of the power of voluntary motion in the posterior limbs being so frequent a result of the opening of the spinal canal, that some physiologists regard it as inevitable. M. Brown-Séquard, however, has found that the spinal cord may be laid open for a considerable part of its length, without any apparent diminution of the sensibility or of power of motion (except such as results from the injury done to the muscles of the spine), if the operation be performed quickly, if pain be prevented by the exhibition of chloroform, and if any considerable loss of blood be avoided. It is of course essential that the persistence of the normal sensibility should be fully substantiated in the posterier extremities of an animal thus treated, before the effects of division of any part of its spinal cord are tested experimentally. Again, it is important to bear in mind that the powers of *receiving* and of *conducting* the impressions which give rise to sensations, do not by any means correspond with one another; so that a deficiency in the former is no disproof of the existence of the latter; or, in common language, a part may not be in itself sensible, although it may be the medium of transmission for the sensory impressions made on some other part. Of this fact the following curious example is given by M. Brown-Séquard:—A large root of the sensory division of the fifth pair (the most impressionable nerve in the body) passes down the medulla oblongata between the anterior pyramid and the corpus restiforme, towards the nib of the *calamus scriptorius;* it has been shown by Magendie that the division of this root by a transverse section of one half of the medulla oblongata occasions loss of sensibility of the face, so that this root obviously serves as the channel of sensory impressions; and yet if a large pin be inserted into the medulla oblongata in such a manner as to penetrate it, there is no sign of pain. Hence it is obvious that the absence of signs of pain, when any particular portions of the cord are irritated, does not justify the inference that those portions are destitute of the power of conducting sensory impressions. Thirdly, in making use of excitation, especially galvanism, as the means of determining the functions of the several parts of the cord, unless this be done in such a manner as to secure the limitation of the excitement, no trustworthy results can be attained. Now practically it is found to be extremely difficult, if not impossible, to effect this limitation; the application of galvanism, for instance, to either of the columns of the cord being almost certain to involve the contiguous roots, to an extent which there is no means of determining. And even if this difficulty can be excluded, the experimental use of such excitation must be extremely limited, since (as just shown) it only serves to indicate what parts of the cord are impressionable, and can give no valid information as to their relative conducting power.

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* Philosophical Transactions, 1851 and 1858.

Hence the method of experimenting by division of the several parts of the cord is the one which is for most purposes to be preferred; but it is requisite to use the greatest caution both in observing its results, and in drawing inferences from them. This method has to be put in practice in two modes; first, that part alone of the cord is divided whose functions we wish to determine, and the results of that division are tested; second, all the other parts of the cord are divided save that one, and the results of that division also are observed. Now, of course, the results of these two operations *should* be antagonistic and at the same time harmonious; that they seem to be otherwise may be due, on the one hand, to an imperfect performance of the experiment, or, on the other, to an error in our observation or our interpretation of the phenomena. In making experiments of this kind, it is especially necessary to determine with precision the exact limits of the lesion inflicted. The most dexterous operator may find himself in error on this point, the section being often proved by subsequent examination to have been either more or less extensive than he had supposed it to be. And there can be little doubt that the want of such exact determination has been the cause of many of the contradictions between the results of operations apparently but not really the same. The more carefully this determination is made in each instance (by a subsequent examination of the spinal cord after it has been hardened by temporary maceration in alcohol), the more satisfactory do the results of section-experiments prove to be; since it is found that where the lesions are the same, their results are the same, and that perplexing differences in results of experiments supposed to be identical are explicable by differences in the lesions really inflicted.—In the care which he takes to avoid the foregoing and other sources of fallacy, we regard M. Brown-Séquard as pre-eminent amongst the experimental physiologists who have devoted themselves to this investigation; and are disposed to accept his results as, if not containing the whole truth, the nearest approximations to it which have yet been obtained.

When both the *posterior* columns of the spinal cord are divided, but the remainder of the cord is left entire, not only is there no diminution of sensibility in the parts below the section, but there is an absolute increase, which is sometimes extremely remarkable. This result shows itself whether the section be made in the lumbar, dorsal, or cervical region; it is equally manifested after section of the restiform bodies, and in a less degree after a transverse incision in the cerebellum, in the processus a cerebello ad testes, or in the corpora quadrigemina. Putting aside for the present the phenomena of hyperæsthesia, and looking to the fact that there is no diminution in the sensibility of the parts below the section, it appears certain that the posterior columns are not essential to the conveyance of sensory impressions from the nerve-roots to the encephalon, but that those impressions can pass by some other channel. On the other hand, if the whole spinal cord except the posterior columns be divided, sensibility is lost in all the parts below the section except those at a short distance from it; though, if even only a very small part of the central grey substance remain undivided, a proportional measure of sensibility is still to be observed. The cause of the persistence of sensibility in the parts immediately below the section seems to lie in the fact, that the portion of the posterior roots which enters the posterior columns of the cord is not interrupted by the section of its remaining substance; but that the fibres of this portion do not proceed far in the posterior columns, seems proved by the circumstance that sensibility persists for only a short distance below the section, the root-fibres of the parts further removed from it having already quitted the posterior columns for the grey substance, and the transmission of their sensory impressions being thus interrupted by the section of the latter. A result harmonizing with this is obtained from experiments of another kind. If the posterior columns alone be divided, indications of sensibility may be obtained by irritation of the upper cut surface, involving the nerve-roots which pass into it. These indications are not diminished by complete division of the same columns at some distance above; but if one section of these columns be made after another, nearer and nearer to the irritated surface, there is to be observed, first a diminution, and (as the distance is reduced) a gradual extinction of the sensibility; whilst if a section of the remainder of the cord be made at some distance above the irritated section of the posterior columns, there is a like cessation of the indications of suffering upon the application of the irritation. From these facts it seems to be a fair inference, that some (at least) of the conductors of sensory impressions pass upwards for a short distance in the posterior columns; but that, instead of proceeding in the same course, they then quit those columns for some other part of the cord. But further, it was long since observed by M. Brown-Séquard that the lower or caudal surface of the section of the posterior columns is not less impressible than the upper or cephalic surface; and by pursuing the same method of experimenting, he has shown that this result is due to the passage downwards for a short distance, in the posterior columns, of a portion of the conductors of sensitive impressions; these conductors, however, soon altering their course, and passing first into the central substance of the cord, and then in it upwards or towards the brain. These results, it will now be seen, harmonize so remarkably with the facts determined by anatomical research, as to leave no reasonable doubt of the correctness of the conclusion,—that of the conductors of sensory impressions contained in the posterior roots of the spinal nerves, a part go upwards (or towards the encephalon) for a time in the posterior columns, and that another part bend downwards (or away from the encephalon) in the same columns; but that in each case, the sensory fibres soon quit the posterior columns to enter some other portion of the spinal cord.

We have now to inquire what can be safely inferred from experiment as to the relative share taken by the anterior columns, the lateral columns, and the central grey substance, in the transmission of sensory impressions to the encephalon. With regard to the *lateral* columns, the result seems to correspond with those obtained in experimenting upon the posterior; for whilst section of the lateral columns alone does not produce any diminution, but rather an increase of sensibility in the parts below, section of all other parts than the lateral columns extinguishes the sensibility of all the parts below, save in the immediate neighbourhood of the section. Hence it seems, in the first place, to be a justifiable inference, that the lateral columns are not continuous conductors of sensory impressions from the nerve-roots to the encephalon; whilst in the second, it may be presumed, from the persistence of sensibility in the parts a little below the section, that some of the fibres of the posterior roots pass along them for a time before entering the central substance; and this last inference is borne out by the results of experiments analogous to those just cited, which indicate that these conductors pass, as in posterior columns, downwards as well as upwards, though for no great distance.—In regard to the *anterior* columns, however, the case is somewhat different; for whilst it is not found that section of them produces any notable reduction in the power of transmitting sensory impressions, it appears that some degree of that power remains when the entire spinal cord has been divided with the exception of the anterior columns. By Calmeil and Nonat it has been thought that these columns have a large share in this function; but according to M. Brown-Séquard, it is only when some portion of the central grey substance has remained undivided, that sensibility manifests itself immediately or shortly after the operation; the sensibility being, in his experience, altogether extinguished for a time, when the division of the central grey substance has been complete, and only reappearing in a very imperfect degree after the lapse of some hours.—If, then, neither the posterior nor the lateral columns serve for the transmission of sensory impressions from the nerve-roots to the encephalon, and the anterior columns have but a slight participation in this function, it would seem to be established, by the method of exclusion, that the *central grey substance* must be the principal channel for such transmission. We must own, however, that this inference does not seem to us so free from objection, as to be entitled to claim for itself acceptance without more support from experiment than we find adduced in M. Brown-Séquard's lectures. To make the proof complete, it should be shown, in the first place, that section of the grey substance alone, without the division of either the anterior, the lateral, or the posterior columns, almost entirely annihilates the sensibility of the parts behind the section; and conversely, that section of *all* those columns *at once*, the grey substance alone being left, has little effect in diminishing the sensibility of the parts behind. The first kind of proof it might not be possible completely to attain, since the whole of the grey substance of the cord could scarcely be divided without carrying the section through one or other of the columns; yet still some nearer approximation to it might be effected than we gather from M. Brown-Séquard's statements that he has made; and some more satisfactory explanations seems to us to be required, of the results obtained by Magendie, Sarlandière, Longet, and others, who found that the passage of a stylet down the central parts of the cord does not produce any marked effect either on sensibility or on power of movement. The second is surely attainable; and we gather from experiments cited elsewhere, that M. Brown-Séquard has firmly established the fact that the continuity of even a small part of the grey matter, the whole remainder of the cord being divided by transverse section, serves for the transmission of sensory impressions. Still this evidence by no means satisfies us that the grey matter is the channel through which these impressions are conducted all the way to the encephalon; more especially since anatomical considerations seem plainly to indicate that, after passing through the grey substance, a large part of the fibres proceeding from the posterior roots emerge again into the fibrous columns of their own or the opposite side of the cord.

Supposing, however, M. Brown-Séquard's position to be conclusively established, it must not be thence inferred that the conduction of sensory impressions takes place by *vesicular* matter, to the exclusion of the *tubular*. For the grey substance of the spinal cord is by no means composed of the former only; being penetrated not merely in the transverse but in the longitudinal direction, by numerous fasciculi of nerve-fibres. These longitudinal fasciculi are probably composed in part of those fibres which appear in transverse sections of the cord to lose themselves in the central grey substance; whilst it would seem likely that they also include a portion at least of those fibres which pass into the grey substance, as already explained, after running for some little distance either up or down the posterior and lateral columns. Still, as M. Brown-Séquard justly remarks, the whole aggregate of these fibrous strands is too small to admit of our supposing that they are the exclusive channels of the transmission of sensory impressions as well as of moter impulses; and since it has been clearly shown that nerve-cells communicate with one another by filamentous prolongations analogous to those by which they connect themselves with nerve-tubes, there seems no *à priori* reason against the belief that the vesicular substance may share with the tubular in conducting power.

In his published lectures, M. Brown-Séquard enters into a very elaborate examination of the evidence furnished by Pathological observation in regard to the conduction of sensory impressions along the spinal cord, which want of time prevented him from discussing orally. Of this evidence, some appears contrary not only to his theory, but to every other; such, for instance, as that of cases in which voluntary motion and sensation were in great degree preserved, notwithstanding what appeared to be the disorganization of the entire substance of the cord. Few, as he justly remarks, who are acquainted with the present state of neurology, will now be disposed to accept either the hypothesis of Magendie that nervous transmission was effected through the membranes of the cord, or that of Hutin, who supposed that it took place through the serous fluid which replaced the cord; the more probable explanation of such anomalies being obviously that a great part of the alteration in the structure of the cord had taken place in these cases subsequently to the last examination of the patient, very likely in some of them after death. The rapid occurrence of post-mortem change in the nervous substance, especially in warm weather, and when the tissue has already undergone partial disorganization, is known to every pathologist; and its possibility must be always kept in view in the interpretation of phenomena of this class. It must be admitted that recourse to such an explanation affords an easy solution of many difficulties, a sort of cutting of the Gordian knot, which becomes very convenient when the recorded facts are inconsistent with hypothesis. But as it is obvious from the cases just referred to, that its use *must* be occasionally admitted, we cannot consistently decline to accord it in other instances; and the question therefore stands thus:—what hypothesis of the relative functions of different parts of the cord is *most* accordant with pathological phenomena, and leaves *least* to be explained away by this or any other kind of special pleading?

In interpreting the affections of sensibility which depend on lesions of the spinal cord, we are warned by M. Brown-Séquard to keep in mind the two following positions, which he has experimentally determined:—1st, that the smallest portion of the conducting zone of the cord contains elements capable of conveying sensory impressions from all the parts below it, so that partial division of the cord, instead (like partial division of a nerve) of extinguishing the sensibility of certain regions, leaving others but little or not at all affected, diminishes the sensibility of the whole region posterior to it, without destroying the sensibility of any one part; this diminution increasing with the extension of the division, but not increasing to complete anæsthesia until the whole of the conducting zone has been cut through: and 2nd, that whilst simple section of the posterior columns induces hyperæsthesia, extensive alteration of those columns is not attended with this result, the hyperæsthesia being antagonized by the partial anæsthesia produced by the destruction of that portion of the posterior roots of the spinal nerves which at first enters into the composition of the posterior columns. It is also important to bear in mind that sensibility often persists after a structural alteration sufficient to destroy the power of voluntary movement; apparently because the sensorium is much more readily acted on by feeble impressions than is the motor apparatus. And thus it happens that complete anæsthesia from lesions of the spine is a rare occurrence. Now in regard to the functions of the posterior columns, we believe that M. Brown-Sequard is justified in asserting that no case has yet been adduced in which there has been any considerable diminution of sensibility during life, and in which after death there has been found a lesion of the posterior columns strictly limited to them, and involving neither the posterior roots of the spinal nerves nor the central grey substance; whilst, on the other hand, there are many cases in which a notable diminution of sensibility accompanied disorganization of the anterior and central portions of the cord, the posterior columns remaining entire. Moreover, when the posterior columns have been alone affected in one spot or for a short distance (as by an incised wound, or by the pressure of a tumour or of a piece of bone), hyperæsthesia has been an almost constant symptom; although a more extensive lesion does not produce any increase, but rather a diminution, of sensibility. In cases of alteration extending through a great length of the posterior columns, there is a notable diminution in the power of standing and of walking; and when the affection has lasted long, there may be a complete loss of those powers without any considerable impairment of the sensibility of the lower limbs.* The causes of this weakness are said by M. Brown-Sequard to be:—1st. That the posterior columns are the principal channels for the excitations which produce reflex movements, so that when they are altered there is a great diminution of those movements; and as these are absolutely necessary for the actions of standing or walking, it follows that the power of performing these actions must be diminished by an alteration of the posterior columns. 2nd. That after a time, when an alteration exists in these columns, the amount of power of action in the other parts of the spinal cord diminishes, as a consequence (we suppose M. Brown-Séquard to mean) of their diminished exercise. The following seems to us the most important case on record of the persistence of sensibility, notwithstanding the disorganization, in a certain portion of the cord, of all its fibrous columns, the central grey substance alone remaining; it is recorded by M. Laboulbène:—

> "A man, aged forty-four, after having had cramps, formication, weakness in the lower limbs, and paralysis in the upper limbs, for a long period, was admitted at the Charité. Sensibility to pinching, pricking, touching, tickling, the feeling of cold, and to muscular spasm excited by galvanism, existed everywhere. On the evening of November 1st the patient was able to walk with some aid. Sensibility remained everywhere to the last moment before his death on November 3rd.
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> "*Autopsy.*—Encephalon normal. There is an induration of the spinal cord from its upper extremity to the third dorsal vertebra, and from the sixth dorsal vertebra to the lower extremity. The tissue of the cord in those parts being cut was shining, looking like porcelain, hard and difficult to be crushed. The grey matter was also a little harder than normally, but of its usual colour. The anterior and posterior roots seemed normal. In the space between the third and sixth dorsal vertebræ the cord was softened, pultaceous, resembling a whitish or slightly rose-coloured pulp (*bouillie*), punctuated in some places. Put in water, many parts became disintegrated and formed a kind of emulsion. This alteration existed only in the white substance; the grey, on the contrary, seemed to have preserved its normal consistence. The microscope showed that the grey matter in both the softened and the indurated parts contained normal cells and fibres and normal bloodvessels; while the white substance in the softened region contained but rare fibres, which were altered, containing an oily matter and granulations. There was also a quantity of granulated corpuscles of inflammation, with many capillaries, oily drops, and amorphous matter. In the indurated white substance there was less alteration, and the fibres were normal and numerous."†

M. Brown-Séquard also enters at considerable length into the pathological evidence which may be opposed to the doctrine that the encephalic prolongations of the posterior columns constitute the special channels of common sensibility; but we do not think it requisite to dwell upon this, since this doctrine has never been advocated by any British physiologist of repute; and it was, in fact, the obvious anatomical connexion of the posterior columns with the cerebellum, through the restiform bodies, whilst the sensory tract of the medulla oblongata (shown to be such by the origins of sensory nerves), passes on in close apposition with the motor in the crura cerebri, that led Sir C. Bell to modify his original idea of the sensory function of those columns.

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* Of this we have a striking example in the well-known case recorded by Mr. Stanley (Medico-Chirurgical Transactions, 1840), the symptoms of which had led to the confidant expectation that the anterior portion of the cord was diseased, but in which the disease was found after death to involve only the posterior columns.

† Mémoires de la Société de Biologie. 1855.

III.—Of all the results of M. Brown-Séquard's experimental researches on the nervous system, the most original and satisfactory appear to us to be those which appear conclusively to establish the *Decussation of the Conductors of Sensory Impressions in the Spinal Cord itself*, at a very short distance from their entrance into it. So far as we are aware, this had never been previously suggested even as a possible hypothesis; certainly it had never been propounded as a deduction either from experimental enquiries or from pathological observation. It had been universally admitted that from the medulla oblongata downwards, each lateral half of the cord ministers to the sensibility as well as to the motility of its own side of the body; and as the phenomena of ordinary cerebral paralysis clearly indicate that the conductors of sensory impressions, like those of motor influence, cross from one side to the other in their passage between the sensorimotor ganglia and the periphery of the body, it was currently supposed that a decussation of the sensory tract must take place either in or above the medulla oblongata. By Sir C. Bell it was affirmed that this decussation takes place in a part of the floor of the fourth ventricle, above and near the crossing of the anterior pyramids; but the appearances on which he founded this assertion have been shown by John Reid, Solly, and Valentin to be deceptive. By Mr. Solly it was thought that a decussation of the sensory columns could be shown to exist higher up than this—namely, during their passage through the pons varolii; and Dr. Radclyffe Hall arrived at a like result. Their statements, however, have not met with general acceptance; and by the greater number of anatomists and physiologists the problem has been considered as still waiting for its solution. Its difficulty was enhanced by the fact, that while, in cases in which sensibility and motor power are conjointly affected, the loss or impairment of both endowments usually affects the *same* side, cases every now and then occur in which the loss of sensibility occurs on the *opposite* side to that of the motive power; and although the idea of a decussation of the sensory tract at a point different from that of the motor tract seemed to offer a feasible explanation of this difficulty, yet its applicability to the supposed locality either of Sir C. Bell's or of Mr. Solly's decussation, so far from being established by pathological evidence, seemed to be negatived by it; and the only other possible solution seemed to be in the idea that some of the sensory fibres decussate, while others do not, and that one or other of these tracts might be separately affected. But this solution was far from satisfactory; since it did not account for the complete extinction of sensibility which is sometimes observable, either on the side paralysed to motion, or on that which retains its motor power, whilst the sensibility on the other side is unimpaired. As regards the course of the sensory impressions in the spinal cord, the only modification of the previously-current doctrine that had been proposed before the researches of M. Brown-Séquard, was that put forth by Stilling, Schiff, and other German physiologists; who, finding that a section of one entire half of the spinal cord is not followed by loss of sensation in the part of the same side posterior to the section, concluded that when the sensory impressions once reach the grey substance of the cord, they are (so to speak) diffused through it, and are thus conducted onwards to the sensorium through its undivided lateral half,—either lateral half being sufficient (according to this view) for the transmission of sensory impressions from both sides of the body.

It seems to have been quite overlooked, however, by these physiologists, that a complete transverse section of either lateral half of the cord, whilst entirely paralysing that side to all but reflex movement, paralyses the other side no less completely to sensation. The result of this experiment is the more remarkable, when the posterior columns have been first divided on both sides, so as to induce a hyperæsthesia in the whole posterior part of the body; for if the lateral section be then made, it has no effect in reducing the hyperæsthesia of the corresponding limb, whilst the sensibility of the opposite limb is entirely extinguished, the difference in the condition of the two being then rendered peculiarly conspicuous. In making experiments of this kind, it is necessary to be very careful in distinguishing the indications of sensibility from those which proceed from reflex movements; and it has been objected to M. Brown-Séquard's deductions, that what he regarded as signs of direct sensibility to painful impressions were really the results of reflex action, of which the animal became cognizant by the general disturbance produced in its body; whilst he accounts for some of the results advanced by his opponents in proof of the persistence of sensibility in parts alleged by him to be anæsthetic, by imputing them to the "recurring sensibility" whose nature has already been inquired into. The following experiment which he has published since the delivery of his lecture, seems well devised to eliminate both sources of fallacy:

> "In a vigorous young rabbit between two and a half and three months old, I lay bare the spinal cord through the whole length of the lumbar region, taking care to avoid wounding the large vessels. I then cover up the wound with the skin, and leave the animal in tranquillity for an hour or two. After this long repose, I divide all the anterior roots of the nerves of the posterior members and of the other lumbar pairs; and again leave the animal in tranquillity for an hour or two. In examining the state of the sensibility of the hinder limbs, after this period of repose, I find that it exists there in a degree manifestly above the normal. It is unnecessary to say that there does not exist any trace of movement in these members, either reflex, voluntary, or convulsive: the hyperæsthesia is then obviously independent of those causes of pain which may proceed from muscles affected with spasmodic contractions. After having fully satisfied myself that the posterior members are both endowed with a high degree of sensibility, I divide one lateral half of the spinal cord (including the anterior, lateral, and posterior columns, and the corresponding portion of the central grey substance, of one side) at the level of the first lumbar vertebra. If this section has been made on the *right* side, I find that the *right* posterior member has an even yet greater sensibility than before, whilst the *left* posterior member has entirely lost its sensibility. Neither pinching, pricking nor cutting of the skin or other parts of the *left* limb, neither burning, nor the application of caustics nor of galvanism, seems to excite the least sensation of pain. Sometimes, it is true, it has happened to me to recognise the existence of some signs of pain in crushing the sciatic or the crural nerve of the left side; but most frequently I have found in such cases, at the autopsy made after the cord has been hardened in alcohol, that a small part of the right lateral half of the cord had not been divided. On the contrary, in the *right* posterior member, sensibility is everywhere exalted, so that a slight pressure on any part of this member suffices to produce indications of pain. All causes of pain act with much greater intensity than they do in animals of the same species in a normal condition; such as galvanism, the application of ice, of flame, or of caustics, mechanical irritations (pricking, pinching, cutting), &c. After having fully assured myself of the existence of hyperæsthesia in the right posterior member (that corresponding with the side of the hemi-section of the cord), and of the loss of sensibility in the left posterior member, I divide the nerves of the cervical and brachial plexus, in order to prevent the movements of those members, whether reflex or otherwise, from taking any share whatever in the production of pain, when the right posterior member is irritated. After the performance of this operation, I find that the hyperæsthesia has not disappeared in that member. It is diminished, like the sensibility of the skin of the thorax; but I repeat that its existence is incontestible. The animal cries out when its foot is pinched, burned, or galvanized, &c."*

The results of *longitudinal* sections of the cord, carefully made so as to divide as little as possible except the median commissure, entirely accord with those obtained from transverse hemi-sections. When the operation has been performed through the entire extent of that part of the cord which gives origin to the nerves of the posterior limbs, and when the two separated halves have been themselves but little injured, the very striking result is obtained of the *entire extinction of sensibility in both limbs*, without any considerable reduction in the power of voluntary movement; a result the more remarkable when it is borne in mind that ordinary injuries of the spinal cord produce a greater effect on the mobility than on the sensibility of the parts behind the seat of injury. As it seems impossible to impute this extinction of sensibility to the injurious effect of the section on the lateral halves of the cord, since this would have manifested itself in the like diminution of the motor power, there seems to be no other possible explanation of the fact, than the one offered by M. Brown-Séquard, namely, that the section has divided the commissures by which the entire aggregate of the conductors of sensory impressions from each lateral half of the body passes over into the opposite half of the cord.

The proof seems to be completed by the combination of both methods of experiment, the longitudinal section, and the transverse hemi-section. If a longitudinal section be made in the cervico-brachial enlargement of the cord, so as to divide its two lateral halves, it is found that sensibility is entirely destroyed in the anterior extremities, whilst the sensibility of the posterior extremities is rather augmented than diminished; showing that no injury has been done to their conductors. But if either of the separated halves be then divided transversely, there is found to result a complete anæsthesia of the *opposite* posterior extremity, with a further increase in the sensibility of the hinder limb of the same side.—We cannot see how the results of this experiment can possibly be explained on any other hypothesis than that of the decussation of the sensory conductors in the cord itself. It is true that such decussation cannot be anatomically demonstrated. Of the fibres of the posterior roots which enter the cord tranversely, some appear to cross through the central commissure; but these seem to come into connection with the anterior roots of the opposite side; and there are other reasons (which will be presently adverted to) for the belief that these transverse fibres are chiefly concerned in exciting reflex actions. On the other hand, the fibres which at first pass longitudinally, whether upwards or downwards, in the posterior columns, and which seem to be the chief conductors of sensory impressions, cannot be shown to undergo any decussation; after their entry into the grey substance of the cord, their course can no longer be determined, so that it cannot be affirmed that they *do not* cross; and in so far as the vesicular rather than the fibrous substance of the cord is the conductor for sensory impressions, their passage from one side of the cord to the other, or in any other direction, might take place without any structural indication of their course. This, however, may be specially noticed,—that all the phenomena indicative of this decussation are opposed to the doctrine that the posterior columns are the chief conductors of sensory impresssions; since it can be fully proved anatomically that those columns do not decussate.

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* Journal de la Physiologie, Janv. 1859, p. 66.

In drawing inferences as to the effects of transverse hemi-sections of the spinal cord, upon the sensibility of the parts behind, it is necessary to ascertain in every instance that the operation has been effectually performed; for if, in his desire to avoid cutting too far, the operator leave but a small part of the anterior column and of the central grey substance undivided, he will meet with very decided indications of sensibility in the limb of the opposite side. It is wonderful, says M. Brown-Séquard, how small is the quantity of grey matter which, being left undivided, may transmit sensory impressions. And it is also necessary to bear in mind that the decussation is not equally complete in all animals; for whilst in Mammals (including Man, as pathological evidence indicates) a complete transverse hemi-section entirely extinguishes the sensibility of the opposite limb, the extinction is not by any means so complete in Birds and Reptiles; and this, together with the occurrence of pain as a result of reflex muscular actions, is the explanation offered by M. Brown-Séquard, satisfactorily as we think, of the apparently contradictory results obtained by M. Chauveau of Lyons, who has been the chief opponent to his theory of decussation.* In order to ascertain how far the sensory conductors pass longitudinally through the cord before they decussate, M. Brown-Séquard has made the following experiment:

> "If we divide transversely a lateral half of the cord in two places, so as to have three pairs of nerves between the two sections, we find that the middle pair has almost the same degree of sensibility as if nothing had been done to the spinal cord, while the two other pairs have a diminished sensibility, the upper one particularly in its upper roots, and the lower one in its lower roots; which facts seem to show that the ascending fibres of the upper pair, and the descending fibres of the lower one, have been divided before they made their decussation. If there be only one pair of nerves between two sections, its sensibility is then almost entirely lost, as then the transversal fibres are almost alone uninjured (most of the ascending and descending being divided), which fibres are employed for reflex action, and hardly for the transmission of sensitive impressions. After having divided transversely a lateral half of the spinal cord in the dorsal region, if we divide this organ longitudinally, so as to separate its lateral halves from one another, and at a right-angle with the transversal section, we find that sensibility persists in the segment partly separated from the rest of the cord, if it is not more than two inches long, in a large mammal, whether the longitudinal section has been

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* A full discussion of M. Chauveau's objections will be found in the Journal de la Physiologie, Janv. 1858, pp. 176—189.

made below or behind the transversal one, or above or before this transversal division. If the longitudinal section is more than two inches long, it is not sensitive in all its length. When there are three pairs of nerves attached to it, the one nearest the transversal section is hardly able to give slight sensations; the next is a little more sensitive, but much less than in a normal condition; and the third is very sensitive, though not so much as others on the same side and behind it. With a segment attached to the cord by its upper extremity, similar results are obtained; and it seems certain, both from these facts and from many others which it is not necessary to mention, that the decussation of the conductors of the sensitive impressions in the spinal cord, whether they are at first descending or ascending, takes place at a short distance from the point of insertion of the posterior roots."

We come now to consider the evidence adduced by M. Brown-Séquard in support of his position, from Human Pathology; and this is so cogent as to appear to us absolutely conclusive,—until, that is, we are made acquainted with opposing evidence equally unexceptionable. One of the most interesting cases cited by him, which is unfortunately too long to be extracted in full, is that of a man who received a sword-wound in the back, the position and direction of which were such as to indicate that the sword-cut divided the whole of the *left* half of the spinal cord with the exception of a part of the anterior column, whilst it may have divided a part of the right posterior column, leaving the remainder of the *right* half of the cord uninjured, save by the pressure of the instrument and the effusion of blood. Now the first effect of this injury was to produce paralysis of motion in both legs; but the patient soon came to be able to move his right leg, and in a few weeks he began to recover voluntary power over his left leg. On the other hand, from the very first there was a decided hyperæsthesia of the left leg, and a great diminution in the sensibility of the right; the hyperæsthesia diminished as the motor power was recovered; but a deficiency of sensibility remained in the right leg even three years subsequently, when the motor power had been completely recovered in both. Although in this important case the sanction of a post-mortem examination was deficient, yet its phenomena were so precisely accordant with those which present themselves in experiments on animals, and were so inexplicable on the ordinary hypothesis by any supposition as to the nature of the injury which should be consistent with the course and direction of the wound, that we think M. Brown-Séquard is fully justified in adducing it as furnishing very important evidence in his favour, more especially as its phenomena were very carefully noted by intelligent observers. The evidence deficient in this case is supplied by several others, in which injury to one side of the spinal cord by the pressure of tumours, hemorrhagic effusion, or disorganizing disease, produced loss of motor power in the parts below on the same side, with retention of sensibility or even hyperæsthesia of that side, and with anæsthesia of the opposite side. Another series of cases is adduced to prove that any lesion of the nervous substance which takes place in the medulla oblongata above the decussation of the pyramids, affects both motion and sensation on the opposite side of the body; and this whether the lesion be just above the pyramidal decussation, or whether it be much higher up; so that they leave no doubt that if the decussation of the sensory conductors does not take place in the same locality as that of the non-conductors, it must be *below* rather than *above* this. And thus we have an important basis for the determination of the seat of the lesion on which paralysis depends; for if there should be loss of motor power on one side, without any diminution or even with an increase of sensibility on that side, the sensibility of the other side being decidedly diminished, it seems certain that the seat of the mischief is in some part of the spinal cord; while if the loss of motor power and of sensibility should present themselves on the same side, the lesion must be in the medulla oblongata, or in its upward prolongation; whilst if there should be partial loss of motor power on both sides, with loss of sensibility on one side only, this may be presumed to depend upon a lateral lesion of the lower part of the medulla oblongata on the opposite side to that of the anæsthesia, and at such a level as to involve some of the motor fibres which have already crossed, together with others which have not yet made the passage.

IV. With regard to *the channel along which Motor Power is transmitted down the Spinal Cord from the Encephalon to the anterior roots of the Spinal Nerves*, we do not find that M. Brown-Séquard's researches have much affected what was previously received as probable truth. Although Sir C. Bell had originally regarded the *anterior* columns as the conductors of volitional influence, yet he had himself been led in his later years by the study of the connections of the anterior roots of the spinal nerves with the *lateral* columns, to regard the latter as participating in this function; and the term "antero-lateral columns" was frequently employed in this country, to designate the part of the spinal cord which was supposed to be subservient to it. The statement of Dr. John Reid, confirmed by other eminent anatomists, that the fibres of the anterior pyramids pass backwards in their course downwards, so as to connect themselves with the lateral rather than with the anterior columns, gave additional strength to this view; and the notion that the anterior columns are the exclusive, or even the special conductors of motor power has seemed to receive a complete disproof from certain pathological phenomena, although other pathological phenomena, together with the results of experiment, have appeared to justify the conclusion that they have no small share in its transmission. According to M. Brown-Séquard, transverse division of the *posterior* columns in the dorsal region, has but little effect upon the voluntary movements of the animal; whilst, after transverse section of the whole spinal cord except the posterior columns, there appears to be a complete extinction of the power of voluntary movement in the parts behind the section. And thus, notwithstanding the admitted effect of extensive lesions of the posterior columns in impairing volitional power over the limbs, he thinks that we are justified in affirming that this part of the spinal cord is not directly concerned in the transmission of the mandates of the will to the muscles. If, however, the transverse section be carried not merely through the posterior columns, but also through the posterior horns of the central grey substance and a part of the lateral columns, he states that there is an evident though very slight diminution of voluntary power; whilst if the whole spinal cord except this part be divided, the power of voluntary movement seems completely destroyed. The first experiment would seem to show that either in the posterior horns of grey substance, or in the lateral columns, there exist some conductors of volitional power; whilst from the second it would appear that these conductors are in themselves insufficient for the stimulation of the muscles to action. The further forward the transverse section is carried, the greater is the reduction of voluntary power; if it divide the whole of the central grey substance, so as to involve rather more than the posterior half of the cord, the animal can hardly move its posterior extremities; and if, in addition, it divide the anterior horns of the grey substance, the greater part of the anterior columns being left entire, the loss of volitional power over those limbs seems to be complete. Yet if the anterior columns be alone divided in the dorsal region, there seems to be a complete extinction of voluntary power in the hinder limbs. Thus if the former result were taken alone, it would seem to justify the conclusion that the central grey substance and the lateral columns are the principal if not the sole channels of motor power, the anterior columns not being able by themselves to convey the least influence from the will to the muscles; whilst if the latter result were accepted to the exclusion of the other, it would lead to the opposite belief,—namely, that the anterior columns have this function to themselves, and that the remainder of the cord is incapable of sharing in it. From other experiments it appears that any injury to the central grey substance, and that a deep injury to the lateral columns, in the dorsal region, always produce a diminution in the voluntary movements of the posterior extremities.

> "We do not see any way," says M. Brown-Séquard, "of explaining these various results, except in admitting, what seems to be proved by thousands of pathological cases and vivisections, that voluntary movements require very powerful excitations from the nervous system upon muscles, and that when one-half or one-third of the normal amount of excitation is missing, what remains is insufficient."

It further appears that, in the dorsal region, the various parts of the spinal cord, except the posterior columns,—namely, the lateral columns, the anterior columns, and the central grey substance,—are employed in the conveyance of the will to the muscles; each of the last two seeming to have a greater share in this function than the first, and the grey matter appearing to have as great a share as the anterior columns.

The results of sections of the cord in the upper part of the cervical region, however, are notably different; for in that part it is the section of the lateral columns, and of the part of the grey matter situated between the anterior and the lateral columns, which most completely prevents the transmission of volitional power. Division of the anterior columns alone, when it has been effected without any considerable injury to the neighbouring parts, does not produce by any means a complete annihilation of the transmitting power. Hence it would seem that the conductors of the mandates of the will to the muscles are more limited in the upper part of the cervical region, than they are in the dorsal part of the cord, to the lateral columns and to the grey matter between these and the anterior columns; a result which harmonizes well with the anatomical fact already referred to. If the two anterior pyramids be divided, loss of all voluntary movement is the result; whilst if the olivary columns be divided, no very decided loss of motor power can be observed: these results again being in harmony with the structural continuity of the anterior columns with the olivary tracts and of the lateral with the pyramids. Further, if a section be made longitudinally just at the place of decussation of the anterior pyramids, so as to divide all the decussating elements, it is found that although the animal lives for some time after the operation, it has entirely lost the power of voluntary motion, just as when the pyramids were divided transversely; yet no impairment of voluntary power seems to proceed from the longitudinal division of the spinal cord in any part of its length. Hence we have the same experimental evidence of the decussation of the sensory conductors along the spinal cord, that we have of the decussation of the motor fibres in the medulla oblongata; the only difference being that the sensory fibres which decussate in any one segment of the cord, and which are consequently severed by longitudinal division of that segment, are only those of the parts immediately adjacent, in which alone the loss of sensation is observable; whilst, in the case of the anterior pyramids, as the entire aggregate of the motor fibres decussates at once, and is severed by a longitudinal section, there must necessarily be paralysis of all the parts to which they proceed.

It is well known that it has been doubted by many anatomists, physiologists, and pathologists, whether the decussation of the anterior pyramids includes all the moter fibres; the doubts in question being founded on considerations of three different kinds. Thus Foville, Valentin, and Longet have maintained that there is a complementary decussation of nerve-fibres connected with the olivary columns all along the median line of the base of the encephalon. Again, Cruveilhier and others have maintained that the small size of the anterior pyramids forbade the idea that they could contain all the fibres which communicate between the encephalon and the muscles. And further, the idea that the pyramidal decussation is the only one, seemed to be inconsistent with the fact that cases of paralysis occasionally present themselves, in which the loss of power is on the same side with the lesion of the encephalon. Against this theory of a complementary decussation, M. Brown-Séquard urges arguments derived from pathological phenomena which seem to us conclusive; the main point of these arguments being, that if there were a partial decussation between the fibres of the motor tract, either in the crura cerebri, the corpora quadrigemina, the pons varolii, or the upper half of the medulla oblongata, there must, whenever there is a lesion on one side of that tract, be a partial paralysis of *both* sides; the paralysis being greater on the side of the injury, or on the opposite side, according as the situation of the lesion is such that a larger proportion of the motor fibres has already decussated above it, or remains to decussate below it. Now this result, according to M. Brown-Séquard, is never witnessed; the effect of injury limited to one side of the motor tract being uniformly shown in muscular paralysis of one side only of the body, and that side being, as a rule, the one opposite to that of the lesion. (Of the exceptional cases, M. Brown-Séquard's explanation will be presently given.) It is only when the lesion affects the motor tract on both sides, or when it exists at the place of the pyramidal decussation, that it produces a paralysis of motion on both sides. If, then, it may be deduced from these facts that there is no decussation of motor fibres higher up than that of the anterior pyramids, it follows that, as no decussation of the motor fibres takes place in the spinal cord (at least in Man, though not so certainly in the lower animals), the conductors of the mandates of the will are entirely contained in the pyramidal bodies and in the grey matter associated with them, the olivary columns as well as the restiform being excluded by the fact that they certainly do not decussate in that region. A striking confirmation of the truth of this view is furnished by a class of cases (of which several are now on record) in which there has existed atrophy of one half of the brain and of the corresponding anterior pyramid, with paralysis and atrophy of both limbs on the opposite side, and also atrophy of the opposite half of the spinal cord, the restiform and olivary bodies remaining unaltered.

M. Brown-Séquard has collected fourteen examples of that curious class of cases of paralysis, in which the loss of power has existed on the same side with the encephalic lesion: and he states that all of them were characterized by the same features—namely, incomplete paralysis, no anæsthesia (save in a single case), and frequent fits of vertigo; and that the lesion was of the same nature in all, having been the result of the existence of a tumour pressing upon the anterior surface of one of the crura cerebelli and the insertion of the trigeminal nerve. This paralysis may be explained in two modes; either by the supposition that it is the result of the destruction of some of the conductors of volitional power; or by the hypothesis that it proceeds from the irritation of certain of the nerve-fibres in the peduncle, in the same way as a reflex paralysis is often occasioned by irritation of centripetal nerve-fibres in any viscus, in any membrane, or in any nerve-trunk that contains them. The former explanation requires the hypothesis that there are motor fibres that do not decussate, which is rendered improbable by the limitation of the paralysis to one side; and it seems to be disproved by cases in which the effect of the *destruction* of a large part of one of the crura has been muscular paralysis of the opposite side. The latter is favoured by the fact that reflex paralysis more frequently proceeds from irritation of the peripheric branches of the fifth pair, than from the like irritation of the branches of any other nerve; and that it usually affects the side of the body on which the irritation exists. The question is one of great interest; and the phenomena of every case in which this form of paralysis presents itself should be carefully observed and recorded, for the sake of the aid which a comparison of its symptoms and of its post-mortem appearances may afford to the determination of their real relation.

Some very interesting suggestions are offered by M. Brown-Séquard in the latter part of his course, in regard to the functional relations of the encephalic portion of the motor apparatus, as manifested in the effects of lesions in producing vertigo, convulsions, rotatory and rolling movements, &c. His views on these points, however, are so far from being completely stated, that we think it better to abstain at present from the discussion of them; merely mentioning that he thinks he has evidence that there is much nervous matter in the pons varolii, in the olivary tract, and in other parts about the base of the encephalon, which is not employed in the transmission either of sensory impressions or of the orders of the will to muscles, but which is endowed with the singular property of producing, after even a slight irritation, *persistent* spasms or clonic convulsions in various parts of the muscular apparatus. It is through this part of the nervous apparatus, according to him, that the movements of rolling and rotation are produced; their source being sometimes a lesion or irritation acting directly on the tract in question, sometimes a change in its circulation produced by contraction either of its bloodvessels or of those of some other part of the encephalon, but very frequently a reflex irritation conveyed through some other nerve, especially the auditory—as in the well-known experiments of Flourens on section of the semicircular canals. This idea harmonizes well with the results of various pathological phenomena, especially the occurrence of convulsions on one side and of paralysis on the other from the same lesion, the convulsions being on the same side with the lesion, owing to the non-decussation of the olivary columns. But it cannot be doubted that this set of fibres and nerve-cells, supposing it to exist, must have some important normal or physiological function; and to the nature of this, M. Brown-Séquard at present gives us no clue whatever.

V. Although it cannot be said that M. Brown-Séquard's researches into the actions of the *Sympathetic System* have established any principle that is new to Physiologists, yet we are disposed to regard them as among the most practically-important of all his contributions to Neurology. For if they have not dispersed all the cloud of uncertainty that hung over this subject, they have made such openings through it as to afford us clear and definite glimpses of what was previously so misty and obscure as to present scarcely any appreciable form; and whilst they have thus given a character of reality to much that was before so vague and speculative that it could lay no claim to be accounted scientific truth, they offer many suggestions of the highest value in the interpretation of pathological phenomena, and in the selection and application of remedial means. Amongst the Physiologists, of our own country at least, who have most philosophically studied the functions of the Sympathetic system of nerves, we believe that those views had come generally to prevail which were formally taught by Dr. Alison, and explicitly set forth by him in the pages of one of our predecessors*—namely, that its special purpose is to bring the functions of Organic Life into harmony with those of Animal life, in virtue of the guiding and restraining influence which it possesses over the former; but that whilst it can either excite, modify, or retard the acts of Nutrition, Secretion, &c., those acts are in themselves independent of it, in the same sense that the actions of a horse are independent of those of the rider upon his back, who nevertheless controls and directs them at his own will. The evidence obtained by Valentin and other experimenters as to the power of the Sympathetic to excite contractions in the heart, the muscular coat of the alimentary canal, the walls of the great vessels, and other muscular organs immediately concerned in the maintenance of the organic functions, and as to the derivation of this power from the Spinal Cord, was generally accepted as rendering it probable that a part of its influence was exerted through its regulation of the calibre of the bloodvessels; a *modus operandi* which was specially indicated on the one hand by the distribution of the Sympathetic upon their muscular walls, and on the other by the phenomena of blushing and other kinds of vascular turgescence arising from mental emotion. But although this regulation of the supply of blood accounted for much, there have seemed to be residual phenomena which indicate that the Sympathetic system has some direct relation with the operations of nutrition and secretion; in virtue of which it can excite, modify, or repress them, not by altering the supply of blood, in the first instance at least, but by affecting the uses to which the blood is put in its passage through the capillaries, and thus consecutively augmenting or diminishing the demand for blood in any particular organ. And it has been thought that this agency might not unreasonably be attributed to the system of fibres which has its central terminations in the vesicular matter of the ganglia of the Sympathetic itself, and which may be considered to constitute the essential part of that system, as distinguished from the portion which is directly derived from the Cerebro-spinal.

Such having been, as we believe, the doctrines currently taught, in this country at least, previously to the commencement of M. Brown-Séquard's researches upon this subject, we shall now inquire how far they have been corrected, modified, or substantiated by the results of his investigations. The first point which he has established is the influence of the Sympathetic nerve upon the calibre of the ordinary bloodvessels, by the contraction it has the power of exciting in their muscular walls. In the latter part of 1851 and the commencement of 1852, M. Claude Bernard made public the results of his experiments on the effects of section of the cervical sympathetic; the most marked of these effects being—besides the permanent contraction of the pupil, which had long previously been noticed by Pourfour du Petit, John Reid, and others—an increased afflux of blood to the head, manifested in turgescence of the vessels of the inside of the ear, elevation of temperature, and augmented sensibility. Impressed, like many other physiologists, with the belief that these results (which might in some degree be likened to a state of persistent blushing) were all due to the paralysis of the muscular walls of the bloodvessels, M. Brown-Séquard considered that the normal state of things would be brought back if galvanism were employed to make them contract; and on making the experiment he found that the results were what he anticipated. These results, having been published in the 'Philadelphia Medical Examiner' for August, 1852, of course take precedence of those of similar experiments communicated a few months later by M. Claude Bernard to the "Société de Biologie," and of those obtained about the same date by Dr. Waller, whose experiments we had ourselves the opportunity of witnessing; and they obviously afforded the first *decisive* evidence of the influence exerted by the sympathetic system over the calibre of the bloodvessels. Putting aside its action on the pupil, the principal results of section of the cervical sympathetic are clearly traceable to the increased afflux of blood to the head; and they may be very closely imitated by holding an animal suspended by its hind-legs for a short time, so as to produce congestion in the head. The chief antagonistic effects of section and of galvanization of the nerve may be thus contrasted:

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* British and Foreign Medical Review, vol. iii. p. 1. et seq.

Section of the Nerve.	*Galvanization of the Nerve.*
1. Dilatation of Blood-vessels.	1. Contraction of Blood-vessels.
2. Afflux of Blood.	2. Diminution of Blood.
3. Increase of Vital Properties.	3. Decrease of Vital Properties.

The source of what M. Brown-Séquard appropriately terms the *vaso-motor* fibres of the Sympathetic, distinctly lies (as does that of the fibres which dilate the pupil) in the Spinal Cord. Those proceeding to the vessels of the head come forth, according to him, by the roots of the last cervical and first and second dorsal nerves; but their place of real origin he believes to be partly the upper part of the spinal cord, but chiefly the medulla oblongata and the neighbouring parts of the encephalon, their connexion with which explains the readiness with which they are affected by emotional states. In other parts of the body, the nerves of the bloodvessels seem to come partly from the cervical sympathetic, but chiefly from the cerebro-spinal axis; so that if the spinal cord be transversely divided on one side in the dorsal region, we find in the lower limbs of that side the dilatation of bloodvessels, the elevation of temperature, the hyperæsthesia, and the increased muscular contractility, which in the head follow the section of the cervical sympathetic.

These *vaso-motor* nerves are not only called into activity by direct excitation, but are susceptible of being made to call forth contractions in bloodvessels by reflex action. This conclusion, the value of which we estimate very highly, was first decisively evidenced several years since by experiments performed by M. Brown-Séquard in conjunction with Dr. Tholozan, on the effect of the application of cold to one hand in producing a marked reduction in the temperature of the other.* They found that when one hand was held in water cooled down nearly to the freezing point, the other gave all the evidence of contraction of its vessels, both in its general aspect, and in its rapid loss of heat, which in some instances was such as to reduce the thermometer held in it as much as 32° Fahr. That this reduction was not due to a loss of heat from the body and members generally, was made clear by the fact that the thermometer placed in either axilla showed scarcely any depression; so that no doubt can be reasonably felt that the stimulus of cold, which produced direct contraction in the vessels of one hand, also operated to call forth reflex contraction in those of the other. If any confirmation were wanting, it is afforded by experiments more recently made; which show that after section of the cervical sympathetic, the distended vessels of the ear may be made to contract by irritation of the cutaneous branches of some of the spinal nerves. This reflex contraction of the muscular walls of the bloodvessels may proceed to the extent of spasm, so complete as scarcely to admit of the passage of blood through their tubes; and it is not a little remarkable that transverse section of a lateral half of the spinal cord near the medulla oblongata, whilst it produces paralysis and dilatation of the bloodvessels of one side of the body, should call forth such a spasm in those of the other, that scarcely a drop of blood follows the division of the skin upon that side, and the temperature falls rapidly, often to that of the atmosphere. It might of course be suggested that this diminished circulation on one side might be fairly laid to the account of the increased circulation on the other; and this explanation receives due consideration from M. Brown-Séquard, who put its value to the test by the ingenious experiment of forcing the blood into the vessels of the opposite side by putting a ligature round the dilated trunk on the side on which the section has been made, the result of which was that the temperature rose so slowly as to make it obvious that the circulation was obstructed; and he further states that injections of blood through the femoral artery in the limbs thus affected are made with far more difficulty than in the limbs of healthy dogs.

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* Journal de la Physiologie, Juillet, 1858, p. 497.

The facts thus experimentally ascertained give the fullest confirmation to the doctrine which had been previously taught by various physiologists, ourselves among the rest, that the sudden stoppage of the heart's action in many cases of *shock*, is due to a reflex influence transmitted through sympathetic. It is well known that persons have fallen dead on the spot from drinking cold water on a warm day, or from blows on the epigastrium, especially when the stomach is distended with food; whilst a rapid prostration, if not immediate syncope, is an almost pathognomonic sign of the perforation or rupture of any of the principal abdominal or pelvic viscera, quite irrespective of the hæmorrhage which may result from such an accident. The same result, moreover, is well known to follow extensive lesions of the peripheral expansions of the cerebro-spinal nerves, as by burns, crushing of joints, and the like; the syncope induced by them being exactly like that which is produced by concussion or other sudden and extensive injury of the cerebro-spinal centres themselves. The like result has been obtained by the sudden application of severe cold to an extensive surface peculiarly susceptible of it; thus when shaved rabbits are plunged into ice-chilled water, they die immediately with spasm of the heart. In all these cases, the par vagum has probably a considerable share in the transmission of the reflex influence to the heart. It is well known that a sudden stoppage of the heart's action may be produced by the application of the magneto-electric current either to the par vagum, or to the medulla oblongata near its origin; and that this effect cannot be produced if the par vagum or its cardiac branches have been previously divided. So it is shown to be probable by M. Brown-Séquard, that in those rare cases in which death from the inhalation of chloroform takes place by the stoppage of the heart's action before the suspension of respiration, the effect is due to the impression made by the chloroform upon the pulmonic branches of the par vagum, and transmitted by reflexion to the cardiac; for he has found that in dogs, which seem to be more frequently affected in this way than are other animals, life may sometimes be restored by mechanically exciting the heart to renewed contraction by pressure on the chest; whilst this mode of death has not occurred in any dog in which the par vagum had been divided previously to the administration of chloroform. Still it cannot be reasonably doubted that the Sympathetic, not the par vagum, is the channel of this influence to the vascular system generally; and the existence of such a condition as "spasm of the extreme vessels," to use the phrase of the sagacious Cullen, may henceforth be considered a capital fact in biological science, its insecure foundation of vague hypothesis having now been strengthened by the solid supports of experimental proof.

One of the first fruits of the establishment of this position is a new theory of *Epilepsy;* which is propounded by M. Brown-Séquard as a legitimate inference from facts experimentally ascertained by himself as to the artificial production of this disorder in animals; and which is applied by him not only with great ingenuity, but, as it appears to us, with great success, to the explanation of the varied phenomena of this disease as it presents itself in Man. In the course of his experiments upon the effects of various injuries of the spinal cord, he observed that after certain lesions of that organ in its dorsal or its lumbar region, especially in guinea-pigs, fits of epilepsy appear spontaneously at intervals of a few hours or of a day or two. These fits consist in clonic convulsions of almost all the muscles of the head, trunk, and limbs (except such as are paralysed by the lesion of the spinal cord), together with laryngismus and loss of consciousness; and when the fit has lasted long, it leaves behind it a sort of drowsiness or unwillingness to move. By trying various kinds of irritation, M. Brown Séquard found that fits could be artificially induced in these animals by simply pinching a particular part of the skin; that, namely, which covers the angle of the lower jaw, and extends from thence to the eye, the ear, and nearly to the shoulder. It is only the skin that has the power of receiving the irritation which excites the fit; as the nerves which supply this part may be irritated without the occurrence of convulsions. When the spinal cord has been injured on one side only, it is only on that side that the irritation of the skin will induce the fit; if both sides have been injured, then the fit may be induced by irritation of the skin on either side. That the cause of the fit does not lie in the mere pain produced by the irritation, would appear from the circumstance that the degree of sensibility of that part of the face and neck is not greater than that of the neighbouring skin, and is far less than that of some parts of the skin in one of the hind limbs; and it must be attributed to some peculiar kind of irritation, which only the cutaneous ramifications of the nerves that convey it have the power of receiving, and which may not even be felt as a sensation. From a careful examination of the sequence of the phenomena presented by the animals thus affected, M. Brown-Séquard has come to the conclusion that the immediate result of the excitation is to produce, through the Cerebro-spinal centres and their nerves, a more or less powerful convulsion of the muscles of the face, whilst through the vaso-motor fibres of the Sympathetic, it induces a spasmodic contraction of the bloodvessels of the Cerebrum and of the face, indicated by the pallor which marks the commencement of the seizure; and it is not a little remarkable that the very same inference has been contemporaneously drawn by two able German experimenters, Kussmaul and Tenner, from researches in many respects different. The following is given by M. Brown-Séquard as the ordinary filiation of the other phenomena of the ordinary epileptic paroxysm:

"CAUSES.	EFFECTS.
1. Excitation of certain parts of the excito-motory side of the nervous centre.	1. Contraction of bloodvessels of the brain proper and of the face, spasm of some muscles of the eye and face.
2. Contraction of the bloodvessels of the brain proper.	2. Loss of consciousness and accumulation of blood in the base of the encephalon.
3. Extension of the first excitation, *partly* due to the accumulation of blood in the base of the encephalon.	3. Tonic contraction of the laryngeal, the cervical, and the thoracic expiratory muscles. (*Laryngismus* and *trachelismus.*)
4. Contraction of laryngeal and of thoracic expiratory muscles.	4. Crying, and stoppage of respiration.
5. Farther extension of the first excitation of the nervous centre.	5. Tonic contraction, extending to most of the muscles of the trunk and limbs.
6. Loss of consciousness, and tonic contraction in the trunk and limbs.	6. Falling.
7. Laryngismus, trachelismus, and the fixed state of the chest.	7. Asphyxia, with obstacles to the return of venous blood from the head and the spinal cavity.
8. Asphyxia, and the accumulation of black blood in the encephalon and in the spinal cord.	8. *Clonic Convulsions* everywhere; contractions of the bowels, the bladder, the uterus; erection; increase of many secretions; efforts at inspiration.
9. Exhaustion of nervous power generally, and of the reflex faculty especially, except for respiration, which gradually becomes normal.	9. Cessation of the convulsions; coma or heavy sleep, after which extreme fatigue and headache."

The asphyxia, to which so great a share is due in the phenomena of epilepsy and in its most serious consequences, depends, according to M. Brown-Séquard, not only on the state of the larynx (as Dr. Marshall Hall maintained), but on that of the chest; for not only cannot the blood return easily from the head on account of the trachelismus, but also it cannot enter the chest from either the spinal canal or the head on account of the fixed state of expiration.* Besides, he continues, the bronchial tubes are themselves frequently obstructed; and all these changes co-exist with an increased production of carbonic acid, and with the change in circulation of the encephalon during which the blood accumulates in the base of this organ and also in the spinal cord.

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* We have seen a case of one of the strange varieties of hysteric convulsion, in which there was spasmodic closure of the glottis during expiration, with repeated and most violent expiratory efforts, but without any loss of consciousness; and this was repeated, time after time, in the course of an hour, the obstruction to the return of the blood from the head, as indicated by lividity of the face and the turgescence of the veins of the neck, being such as to occasion the gravest alarm, until it was relieved by the free expiration permitted by the yielding of the glottis.

Thus, then, epilepsy appears to consist essentially in an increased reflex excitability of certain parts of the cerebro-spinal axis, and in a loss of that control which, in the normal condition, the will possesses over the reflex faculty. As the base of the encephalon, and especially the medulla oblongata, is the most frequent seat of the augmented excitability, that part of the nervous system must be considered as the *centre* (so to speak) of the disorder; but for the production of the convulsive paroxysm some excitation is necessary; and this may spring (as all who have studied the disease are well aware) from a great variety of agencies, some affecting the peripheral portion of the system, some more directly operating upon the central organs. In common with every physician who has treated of this disease with a right appreciation of its nature, he lays great stress on the importance of searching out and removing every kind of peripheral irritation that can be discovered; and the chief novelty in his suggestions as to this point consists in his mode of determining the point whence the first impression proceeds.

> "If," he says, "the unfelt aura starts from some part of the skin or from some organ not deep-seated, as the testicle, or some part of the mucous membrane near the skin, either the first contractions in a fit, or the most violent or the most prolonged, are found in the neighbourhood of the point of starting of the aura. If no indication of this kind can be furnished by the persons who have seen the fits, it will be well to try the application of a very powerful galvanic current, with dry conductors, on the various parts of the skin, when the patient expects to have a fit. I have in this way twice ascertained the point of starting of an unfelt aura: a fit has been produced by the galvanization of certain parts of the skin. Of course there are many cases where such a means of diagnosis ought not to be employed; every one will understand what are those cases. Another and the best means (so far as the limbs alone are concerned) to detect the existence of an unfelt aura, consists in applications of ligatures on each limb alternately. Suppose a case of epilepsy in which the fits are frequent and come at nearly fixed times, or after warnings of any kind, so that it may be known that it is to take place in a given time or nearly so: a very tight ligature is put on one limb; and if the fit does not come, it is extremely probable that it depends on the irritation of an unfelt aura; if it comes, the ligature is applied on the other limbs at other times. I am sorry not to be able to give more details in this repect; but I think it will be easy to understand how, by such a means, it may be ascertained if an aura comes from the upper part of a limb, or from a toe or a finger, and from which one.
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> "Even in cases of epilepsy due to a disease of the encephalon, the cause of the fits may originate from some points of the skin; and the prevention of the passage of the aura, in such cases, can prevent the fits. There are four cases of this kind that I know, in three of which the disease consisted in a tumour in the brain. In my animals the same thing exists: although the alteration of the spinal cord—which is the cause of epilepsy—persists, the aura being interrupted by the section of the nerves which go to the skin of the neck and face, epilepsy, so far as I have been able to ascertain, ceases. The aura may originate from any part of any centripetal nerve, and there is no doubt that its place varies according to the location of disease in the nervous centres when it is due to such a disease."

The increased excitability of the central organs, the coexistence of which is necessary for the production of the paroxysm, is best combated, in M. Brown-Séquard's opinion, by the cauterization of the back of the neck by moxas or by the actual cautery; a powerful modification of nutrition, the neglect of which by regular practitioners he much regrets. We presume that want of time prevented him from speaking of those general constitutional means, the efficacy of which, in certain classes of cases, has been placed beyond doubt.

But it is not epilepsy alone, to which the theory of spasmodic contraction of certain bloodvessels, with excessive dilatation of others, depending upon a state of reflex excitation or of paralysis of the vaso-motor nerves, is applicable; for various other forms of nervous disorder may be attributed to it with great probability. Among these M. Brown-Séquard mentions various forms of insanity, of vertigo, of hallucinations and illusions, and also extasis, catalepsy, hysteria, chorea, hydrophobia, tetanus, local cramps, and even the general paralysis connected with insanity; which, as pathological evidence has long since been shown to indicate,[*] are often due to irritations starting from a centripetal nerve, that are frequently slightly felt or even unfelt; and which may then be promptly cured, like epilepsy, by the simple removal of the irritating agency. And it seems to us more probable that the actions of many kinds of *poisons*, especially such as produce their chief results through the nervous system, involve the like change as their primary phenomena. There can be little doubt that some of the most potent of these act (as we have seen that chloroform probably does) directly upon the heart after the manner of *shock*; such appears to be the case with alcohol, when introduced into the stomach in a state of high concentration, hydrocyanic acid, aconitine, nicotine, and sometimes even with arsenic. The extraordinary variety of the combinations of anæsthesia and hyperæsthesia, which characterizes some forms of lead-poisoning, seems to us, like the corresponding vagaries of hysteric disorder, to be better accounted for on the idea of local "spasm of the extreme vessels" than on any other. And even in the action of certain narcotics, especially opium, there are phenomena which, as it seems to us, can be better explained in this manner than in any other. If tetanus is ever attributable to this cause, the artificial tetanus induced by strychnia has its origin by the same reflex agency; and this we believe we may state, M. Brown-Séquard expects to be able ere long to prove. We conclude this part of our subject by expressing a strong belief that the action of the vaso-motor nerves is one of the most important subjects that can engage the attention of physiologists and pathologists; and that it will be found to afford the key to a great number of phenomena, our very familiarity with which seems to be the cause why they have hitherto received so little attention. As one example of what we mean, we may refer to the extreme coldness of the extremities, often suddenly coming on, and as suddenly departing, to which many persons are subject (especially if they work their brains too severely), notwithstanding that the general circulation is by no means deficient in vigour.

VI. *The Influence of the Nervous System upon the Nutritive and Secretory Operations*, and especially its *reflex action*, are very fully discussed by M. Brown-Séquard, who does not omit to expose the fallacy of the claims to discovery on this subject which have been set up by Dr. Marshall Hall and by Dr. H. F. Campbell of Georgia (U. S.). For whilst freely conceding that Dr. Campbell was the first to introduce in science the hypothesis that there exists a secretory and excito-secretory system of nerves, he points out that neither he nor Dr. M. Hall adduced a single fact to prove its existence, and that both these physiologists seem to have been unaware that reflex changes in nutrition and secretion were perfectly known, and that the question was, not to prove that there are such reflex phenomena, but whether they are to be explained by a reflex influence on bloodvessels or otherwise. He gives due credit to our own Whytt for having shown that the natural and morbid sympathies, in regard alike to movement, to nutrition, and to secretion, are reflex phenomena, and that the share of the bloodvessels is very great in these phenomena; and he specially alludes, among modern works, to Müller's 'Handbook of Physiology,' Stilling's 'Treatise on Spinal Irritation,' and various writings of Henle in 1840 and 1841 (to which we would add Dr. Laycock's 'Treatise on the Nervous Diseases of Women'), as having advanced the subject much further than Dr. Campbell did in this first publication. The portion of M. Brown-Séquard's 'Lectures' which is devoted to this enquiry, contains a very able and elaborate summary of various recent contributions, chiefly furnished by pathological observation, by which the doctrine is placed upon a more secure and extended basis than that on which it previously rested; but since, of these contributions, a very small part is furnished by himself, we do not think it requisite to discuss this portion of his Lectures as fully as we have done the preceding; and shall limit ourselves to the citation of a few of the facts adduced by him, which seem to us of special novelty or interest.

Under the head of Reflex Secretions, we learn that M. Castorani has recently confirmed, by decisive facts, the view that it is not through the optic nerve, but through the exalted excitability of the trigeminal, that the secretion of tears is increased in photophobia when the eye is exposed to the stimulus of light; and that M. Deslandes has observed that a man totally blind had an abundant secretion of tears every time he passed from a dark place to a light one. The shedding of tears under the influence of irritation of other parts than the eye and nose, is said by M. Brown-Séquard to be less and less the further the irritation is from the eye; in experimenting upon himself he has found that the pinching of the neck or of the back parts of the head scarcely produces lachrymation, whilst pinching of the face produces it more and more the nearer the eye the irritation is made; and the same result shows itself in cases of neuralgia, the lachrymation which is a frequent concomitant of this affection in the fifth pair being specially produced by neuralgia of its supra-orbital branch. The following experiment performed by M. Brown-Séquard to test the reflex influence of the nervous system on the urinary secretion, is valuable for the precision of its results:

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[*] See especially Dr. Laycock's Treatise on the Nervous Diseases of Women, passim.

> "We place a tube in one of the ureters of a dog, so as to know what is the quantity of urine flowing out in a given time, after the dog has recovered from the shock of the operation. We then pinch the internal surface of the abdominal wall, in a part receiving its nerves from one of the first lumbar pairs; and almost at once we find that the secretion of urine is either stopped or very much diminished. It is not in consequence of a change in the circulation due to the pain caused by the pinching, that the secretion is so much diminished; as we find the same thing taking place whether the spinal cord in the dorsal region has been divided transversely or be left in communication with the encephalon. And if the part of the cord which gives origin to the lumbar pairs of nerves has been destroyed—in which case the urinary secretion, after a short stoppage, becomes normal (as to its quantity, at least), and is rather more than less abundant than before,—we find that the irritation of the abdominal wall remains without effect upon the kidney. We must conclude, therefore, that when the spinal cord exists, the irritation passes through it; or, in other words, that the stoppage of the urinary secretion is due to a reflex action of the spinal cord. I have ascertained, also, that it is through the spinal cord, and by a reflex action, that the irritation of one kidney acts upon the other, sometimes to diminish, sometimes to increase, its secretion."

It is, of course, open to question whether the influence which the nervous system undoubtedly exerts over the *quantity* of the secretions, may not be due simply to its control over the calibre of the bloodvessels; and this, at first view, might seem the more probable, since increase of various secretions of glands and membranes of the head is noticed as one of the results of section of the cervical sympathetic. But it is to be borne in mind, that this increase proceeds from *paralysis* of the vaso-motor nerves, which, by permitting a greater afflux of blood, augments the general vital activity of the parts through which it passes; whilst it is antagonized by *excitation* of those nerves, which, by causing contraction of the vessels, brings back the secretions to their normal condition. Now it would seem unlikely that the various forms of stimulation, with whose action in augmenting the lachrymal, the gastric, the mammary, and other secretions every one is familiar, should exert their influence by paralysing the vaso-motor nerves; and we seem to have, in the experiment just cited, a very clear indication that the augmentation of the secretion is due to a reflex stimulating influence conveyed by the nerves of the gland, as the result of an excitation applied to a remote part. This inference fully accords with the result previously obtained by Cl. Bernard and Czermak in regard to the salivary secretion, which they have shown to be augmented by excitation of the lingual nerve; and it also harmonises very remarkably with the doctrine often maintained in the pages of this Journal, as to the influence of augmentation in the functional activity of a part, in determining the afflux of blood to it.

It may now be accounted as an established fact, that the nutrition of certain parts is often affected in a very decided way by remote irritations, whose influence is transmitted through the nerves leading from the part irritated to the central organs, and is then reflected back from them towards the periphery. Of all the organs in the body, there is none which gives such evident and frequent instances of this kind of affection, as the eye. For notwithstanding the doubts of eminent physiologists like J. Müller, and of able oculists like Walther and Sichel, it must be obvious to any one who carefully weighs the evidence now collected, that when the supra-orbital nerve has been crushed or injured, in such a way that it remains irritated, an inflammation or some other affection of the corresponding eye is very apt to supervene; and further, that when an eye is the seat of a violent inflammation, and particularly if this be of traumatic origin, the other eye is extremely liable to become affected. We have ourselves seen a large number of cases in which amaurosis followed an injury to the supra-orbital nerve; and one of these was peculiar in this respect, that the injury consisted in the lodgment of a single shot-pellet in that nerve very near its point of emersion on the forehead, thus meeting the objection of Müller and Sichel that the amaurosis following a blow on the forehead may be fairly attributed to the commotion of the eye and optic nerve. Dr. Alcock, moreover, showed that injuries to the infra-orbital nerve are not less liable to produce results of this kind, than those of the supra-orbital. We have also ourselves seen many cases in which amaurosis and cataract, together or separately, having supervened in one eye upon a blow, have appeared after a time in the other. And M. Brown-Séquard very correctly states that the danger to the second eye of a long continuance of traumatic irritation has been so fully recognised in this country, that the extirpation of the wounded eye to save the other has now become a recognised practice.

Among analogous phenomena occurring elsewhere, M. Brown-Séquard lays particular stress on the disordered nutrition of one or more of the principal viscera of the head, thorax, or abdomen, which, as the observations of Mr. Long and Mr. Curling demonstrate, is one of the most common sequences of severe burns of the external surface. The whole collection of facts he has brought together upon this subject is extremely well worthy of attention; especially as many of them are drawn from sources but little known in this country; but our limited space warns us to forbear from citing any of them; and we must content ourselves with endorsing his remark that changes of this kind are not explicable by simple augmentation or diminution in the supply of blood, such as may be produced by an action of the vaso-motor nerves in modifying the calibre of the blood-vessels, but point to an influence exercised through the nerves upon the vital properties of the tissues themselves. We see, for example, that after section of the cervical sympathetic, the continued augmentation in the supply of blood to the eye, ear, &c., does not itself produce inflammation; and although this morbid process is produced much more easily in these parts than in others, it does not supervene spontaneously, but requires some special cause in addition to the alteration of the supply of blood and the paralysis of the vaso-motor nerves.

We shall bring our survey to a conclusion by quoting the suggestions offered by M. Brown-Séquard as to the therapeutic applications of which the principle of the reflex action of the nervous system upon the nutritive functions seems most obviously susceptible; and in taking our leave of him for the present, we would offer him our hearty thanks for what he has already done for Physiology, and our cordial good wishes for this further success in the same useful and honourable career.

"1st. When we wish to produce a modification in the condition of any organ, we must apply the means of irritation that we prefer to the part of the skin or of the mucous membranes which have the most evident nervous relations with it. In most cases the parts acting with the greatest power upon another are those which receive their nerves from the same segment of the cerebro-spinal axis. If we wish, for instance, to act upon the kidney the skin of the abdomen in its upper part is the best for the application of any kind of irritation. Do we wish to act on the eye, in cases of amaurosis due to insufficiency in the amount of blood, the irritation ought to be applied chiefly to the supra- or infra-orbitalis nerves. If the amaurosis coexists with hyperæmia, the irritation of those nerves must be avoided and the means of revulsion ought to be applied on the back of the neck, so as to act on the spinal cord, and, through it, by the sympathetic nerve, which has on the eye an influence entirely different from that of the trigeminal nerve. In cases of diarrhœa, an influence upon the nerves of the bowels originating from nearly the middle of the dorsal region might be obtained by the irritation of the skin of the middle of the chest. The ovaries and the uterus being able to influence the nutrition of the mammæ, and these glands being able to act upon the genital organs, irritation will be applied to one group of these organs when we wish to act upon the other. In amenorrhœa, for instance, various means of irritation to the breast have produced menstruation. 2nd. The kinds of irritation which produce the most powerful effects are a great and sudden change of temperature, heat or cold, or the application of a very strong galvanic current. Frequent irritations, with periods of interruption between them, are better than permanent irritations. 3rd. The suppression of the cause of irritation, when a disease is produced by a reflex action, is of course the principal mode of treatment. In cases of paralysis, of anæsthesia, or of a convulsive affection, &c., we must try to find out if there is an irritation on any centripetal nerve, and employ the most energetic means for its removal. But I must say that it is entirely useless to amputate a limb, or a part of it, as has been done sometimes in cases of convulsive affections produced by an external irritation. The section of a nerve will do as well,—and this is already proved by many cases,—and perhaps, as I will show in my last lecture, a simpler means might be employed. Time pressing me to go on, I will only add here that in cases of reflex congestions or inflammations due to burns or to congelation, or, in fact, in any case in which we have to avoid a reflex influence, we must diminish the reflex faculty of the spinal cord and encephalon, and we know no medicine having so much power in this respect as belladonna."

REVIEW II.

Copy of the Statistical Report of the Health of the Royal Navy for the Year 1856. Ordered by the House of Commons to be Printed, 26 July, 1858.

THE Statistical Reports on the Health of the Royal Navy, drawn up from Returns lodged in the office of the Director-General, commenced in the year 1830, and have since that time been printed at irregular intervals. Up to 1836, inclusive, they were brought out under the care of Dr. Wilson, and he was succeeded by Dr. Bryson, who still continues to superintend them. The labours of both these gentlemen have been accomplished in a manner highly creditable to themselves, and conducive to the attainment of the important objects in view.

Dr. Bryson observes that—

> "These returns, modified and improved as from time to time they have been during the last few years, are now, with few exceptions, sent into office in a form so complete, that the labour of constructing the statistical tables, and drawing up any explanatory remarks deemed necessary, has been made comparatively easy." (p. 3.)

But he adds that, notwithstanding the great improvement which has taken place, the returns are still occasionally deficient in the kind of information most essential for inquiry into the origin of endemic or epidemic diseases.

> "When an epidemic breaks out in a ship of war, it is no doubt right and proper that the medical officer in charge should give a description of the weather and the hygienic condition of the ship at the time it made its appearance, but it is also of importance that he should distinctly state whether the persons first attacked had or had not been exposed to infection or contagion, or whether they had or had not visited any port, place, or ship during the three weeks immediately preceding the outbreak, because the medical records of the service have been searched in vain to discover an instance in which either cholera-morbus or yellow fever made its appearance amongst a ship's company, unless one or more of the men or officers had previously—within at most twenty-one days—been exposed in some house, ship, or locality where the infectious virus which emanates from persons ill of the one or the other of these diseases existed. The spontaneous origin of either malady, far away from an infected locality, is unknown in the naval service; hence the great importance of information respecting the absence or presence of disease on shore, the movements of the ship, or the exposure of her men previous to the eruption of epidemic and infectious diseases, for it is much to be feared that many valuable lives have been lost by vainly endeavouring to extirpate from the holds of ships the exciting cause of diseases which exist only in connexion with the men." (pp. 3–4.)

We proceed to give as full a *résumé* as our space will admit of the contents of the present report, dwelling chiefly on those parts of it which illustrate general principles or describe peculiar forms of disease. We take the several stations in the order in which we find them.

Home Station.—There were fifty-eight vessels employed on this station, for periods varying from three to twelve months, with a mean force of about 12,445 men. The men were allowed to go on shore much more frequently than on foreign stations, and in consequence of this, the returns show, in the comparative absence of more fatal maladies, a larger proportion of diseases affecting the respiratory and sexual organs. It is a fact which should be more generally known, that syphilitic diseases are more prevalent in this country, especially in the garrison and great seaport towns, than in any other part of the known world, and it will be found in the course of this Report, that a large proportion of the disease prevalent in ships on foreign stations consisted in venereal affections contracted before leaving the home ports. On the home station the evil seems to be on the increase, for the number of cases in 1856 was more than double the average of the preceding fourteen years."

"That a disease so destructive of health and happiness (says Dr. Bryson), which by an acquired constitutional taint may be transmitted to generations yet unborn, should be allowed to go on increasing in our large seaports to an extent unknown in any other part of the world, is greatly to be deplored, but so long as the municipal authorities of those towns where it is most rife refuse to co-operate with the Government in establishing hospitals for the cure of the degraded creatures that swarm along their pavements, it will be in vain to hope for any abatement of the evil." (pp. 5–6.)

Of the other maladies which prevailed on this station, there are none of which we need take any particular notice. The number of men daily ineffective through wounds and diseases was in the ratio of about 30 per 1000 of mean force. The lowest ratios were in the stationary or harbour ships, but this was in consequence of the particular nature of the duties of their crews, and the removal of all serious cases to the naval hospitals on shore. In fifteen ships of the line more actively employed than these, the average loss of service was about 3·7, in vessels of the frigate class it was 28·8, and in the smaller vessels 35·5; therefore, supposing the number of men sent to hospital on shore nearly equal in all, it may be inferred that the medium class of vessels were the most healthy. The total number of men invalided was 160, and the total number of deaths 129.

Mediterranean.—There were 63 vessels employed on this command, with a mean force of about 11,090 men. The average of sickness daily in 1000 was, for ships of the line 42, for frigates 43·8, and for the smaller vessels 49·7. The total number invalided was 171, and that of deaths 142. In 4 of the line-of-battle ships in which the loss of service was greatest, the excess was entirely to be ascribed to venereal complaints contracted in the home ports. Among vessels of the frigate class, with crews of 150 men and upwards, there were 4 which had the greatest number of men inefficient from sickness and wounds; this appears to have been owing to the prevalence of phlegmonous inflammations, to syphilis contracted by the crew of one of them in England, and to the tardy cicatrization of wounds in all. In four other vessels of this class, in which the loss from sickness was least, there was no phlegmonous disease; so that there appeared to be a superiority in the sanitary condition of some vessels over that of others on the same service and on the same parts of the station. Fever of an adynamic character, which had prevailed in the *Hannibal* line-of-battle ship during the preceding year, continued during the present. This ship had remained stationary at Malta between the 1st January and the 13th May; on the 23rd of the latter month she anchored off Karatch, on the Black Sea; from that date until September she was employed between the Crimea and the Bosphorus carrying troops and stores; she then returned to Malta, and in November arrived in England. The persistence of fever in this vessel for two years, through such varieties of geographical position, can only be ascribed to the successive transmission of infection from one set of men to another, for the vessel was kept scrupulously clean, and the crew were clothed, fed, and employed like all other sailors in the fleet. Fever cases of a similar type occurred on board the *Royal Albert* under nearly the same circumstances of locality and season. As a proof that the fever in these two vessels was not occasioned by climate or the state of the weather, the *Princess Royal* lay in harbour at Malta from the beginning of the year until the middle of April; subsequently she entered the Black Sea, touched upon the coast of the Crimea, and returned to Malta in June; yet during the whole time she had only 3 cases of fever against

130 in the *Hannibal* and 30 in the *Royal Albert.* Cases of other ships are adduced, leading to similar inferences.

One death took place on this station from rupture of the vena cava inferior about four lines within the pericardium, and another from rupture of the coronary vein. It is stated, in reference to the former case, that the heart and large vessels were free from organic disease, and that the man, up to the time of his death, which of course was sudden, had enjoyed very good health. The only other disease in connexion with the Mediterranean station to which we have to advert is malignant cholera, and this only in relation to the question of contagion. After giving an account of 23 cases which occurred in eleven vessels, Dr. Bryson remarks:

> "It thus becomes clearly evident that all the cases of cholera and choleraic diarrhœa that appeared in the fleet during the year were contracted either at Malta or Lisbon, and in almost every instance the first cases in the respective ships were contracted on shore in infected localities, and when the patients returned to their own ships that they communicated the disease, though generally in its milder form, to a considerable number of their shipmates. These facts would, it might be supposed, afford matter for grave reflection to those who still affect to question the infectious nature of Asiatic cholera, and deny the utility or necessity of restricting the communication of the healthy with the sick, as far as may be reasonably practicable." (p. 26.)

West Indies.—There were thirty-seven vessels employed on the North American and West India station, with a mean strength of 7,845 men. There were five ships of the line, and in these the loss of service was at a much lower rate than in any other class of vessels, as they were employed chiefly at Halifax and Bermuda, the healthiest parts of the station. The high rate of sickness in one of them, the *Orion*, was the result of syphilitic disease carried from England, and of ulcerative and febrile diseases contracted off the coast of Central America. The greatest loss of service in any vessel on the station occurred in the *Arachne* (18 guns, 135 men), owing to the prevalence of boils, small irritable ulcers, and the innumerable complaints which make their appearance in almost every newly-raised ship's company. The loss of service was also great in the *Malacca* (17 guns, 165 men) from a most destructive eruption of yellow fever, and in the *Arrogant* (47 guns, 430 men) from remittent fever and ulcer contracted off Grey-town. Most of the vessels which remained for any length of time off Grey-town, or other parts of the coast of Central America, exhibited a high rate of sickness from the prevalence of these two diseases.

The greater part of the report relating to this station is occupied with an account of yellow fever as it occurred in different vessels of the squadron. The chief thing which calls for our notice here is the general conclusion arrived at respecting the source of the disease:

> "It has already been seen that the fever was introduced into the *Malacca* and *Argus* at Port-au-Prince; that these vessels carried it to Port Royal, Jamaica, and that subsequently it broke out in the *Termagant* and *Hermes:* but whether the fever in these vessels was an off-shoot from the fever in the *Malacca*, or from some other infectious source at Kingstown or Port Royal, there is no means of ascertaining. It is, however, time that the hazardous opinions respecting the non-infectious nature of this malady were more clearly established. If, as was supposed, the fever in each of these vessels had arisen from peculiar states of the weather, from marsh or swamp emanations, or from causes other than a personal poison, it is inconceivable why it did not break out in other vessels lying in the same ports; for although it is easy to understand how their crews might escape an infectious poison existing only in circumscribed localities, it is not possible to imagine how they could escape from the influence of causes so generally diffused as those called atmospheric, or from marsh malaria." (p. 62.)

It will be observed, throughout this Report, that Dr. Bryson is a staunch contagionist, for which, however, we are not disposed to quarrel with him, being ourselves somewhat *ejusdem farinæ.*

The total number of deaths from disease on this station was 177, or in the ratio of 22·6 per 1000; and the total number from accidents, suicide, and drowning, 29, or in the ratio of 3·7 per 1000. The total number of men invalided was 185. Deducting the deaths from yellow fever, and those from drowning and external violence, the rate was only 7·8 per 1000 of mean force—a rate so small as to show "that the climate even of the West Indian division of the station is by no means so destructive of European life as is generally believed." (p. 74.)

East Coast of South America.—There were ten vessels on the Brazilian station, five of which were there during the whole year, and the other five from six to nine months. They were chiefly employed in the suppression of the slave-trade, and had a mean force of about 1200. The total mortality for the year was 18 from disease, and 6 from external injuries and drowning, or 15 of the former and 5 of the latter per 1000 of mean force—a mortality at least one-fourth greater than the average for the fourteen years preceding the introduction of yellow fever into the Brazils. The increased death rate for 1856 is to be attributed entirely to this pestilence, which carried off sixteen of the eighteen men who died of disease, and of whom thirteen belonged to one ship, the *Express*, the crew of which contracted it at Rio. It thus appears that, setting aside yellow fever, a recent importation into these regions, the total loss from disease was two only, or in the proportion of 4 to 3000 of mean force.

> "There was actually not one death from disease which could be called climatorial, so that, were it not for the introduction of two contagious diseases, namely, yellow fever and cholera morbus, this coast would still maintain its character for being one of the most healthy regions for Europeans in the whole world." (p. 92.)

Fever of a peculiar character appeared on board one of the ships of this squadron, the *Siren*, at Bahia. Yellow fever, which had been very prevalent at that port, had almost entirely disappeared, and the men on the 4th of April had thirty-six hours' leave of absence, which was not followed by any increase of sickness. On the 22nd an extensive fire broke out in the town, and the greater number of the men were landed to render assistance, and remained exposed to heavy rain till the following morning. For a few days no illness of any importance ensued; but on the 3rd of May there was one case of fever, and another on the 4th. After that the attacks occurred at the rate of two, three, or four per diem, until the 9th of June, when they entirely ceased. At the commencement of this disease all the symptoms were present which usually accompany yellow fever in the same stage. But there were also symptoms which are not commonly met with in yellow fever; thus, epistaxis of an *active* character occurred from the earliest period. In some cases the patients, while recovering, were seized with a kind of fit, of short duration, attended with partial insensibility, and terminating in syncope; there were painful furunculi, generally on the hands and fingers; herpetic eruptions over the whole surface during convalescence, and in one case purpura, with inflamed, swollen, and spongy gums. Throughout the disease there was no black vomit, and out of 96 cases not one was fatal. If, therefore, the malady was of the nature of true yellow fever, it assumed an unusually mild form.

> "Whether," says Dr. Bryson, "this fever was the result of exposure and fatigue while on shore at the fire; whether it arose from some peculiar condition of the atmosphere, or from some cause within the ship, will be viewed differently by different individuals. But, as the ship went to sea on the day the first case made its appearance, it is hardly possible to conceive that any abnormal condition of the atmosphere, to which the men might have been exposed on the night of the fire, could have caused those cases which occurred in June, or that any abnormal condition of the atmosphere could have produced the disease in the *Siren* and not in other vessels in the harbour; nor is it probable that the fever could have arisen from any cause within the ship, as she was clean throughout and well ventilated. The cases which occurred on the 3rd and 4th, and up to the 11th, of June may be ascribed to the exposure on shore on the nights of the 22nd and 23rd of April; but certainly those that occurred subsequently to the 30th of May cannot be ascribed to any malarious influence or personal contagion acting on the men at such a remote period. If this mode of reasoning be admitted, the only other way of accounting for the eruption of the fever, and its continuance for nearly forty days, or until it had attacked nearly every person in the ship, is to suppose that it was contracted on shore at Bahia by one or more of the men, and communicated to others who had not been on shore; whether or not it had originally been communicable or contagious there are no means of ascertaining." (pp. 88, 89.)

But, surely it might have been ascertained whether any such peculiar form of disease was prevalent on shore when the men were landed, or whether the yellow fever, which had prevailed a short time before, had presented anything unusual in its characters—inquiries which do not appear to have been made, and the absence of which tends to justify Dr. Bryson's remark, quoted at the commencement of this article, respecting the occasional deficiency of the returns in information respecting the origin of epidemics.

Pacific Station.—Fourteen vessels were employed on the West coast of America, and in the Pacific, with a mean force of about 2680 men. The daily average of men, ineffective from wounds and sickness, ranged, in the different vessels, from 38 to 83 per 1000 of their crews. The total number invalided was 78, and that of deaths 23. The greater proportion of sickness consisted in venereal diseases—208 cases of syphilis, and 47 of gonorrhœa. Nearly one half of the former were contracted in the home ports, and at least half the remainder at Valparaiso, where syphilis appears to be more prevalent than at any other sea-port in the Pacific. Of all kinds of fever, there were 134 cases, none of which were fatal.

West Coast of Africa.—The squadron here employed for the suppression of the slave-trade, consisted of twenty-one vessels, from one of which the medical returns have not been received. The mean force, including Kroomen and African boys, amounted to about 1680. The daily average of those inefficient from wounds and sickness, were about 55 per 1000 of mean force. The total number invalided was 74, and that of deaths 30. There is nothing calling for especial observation in the diseases which prevailed; but Dr. Bryson's remarks on the causes which have conduced to the comparative absence of fever of late years, and on its improved treatment, are well worthy of attention. They will be found at p. 115–16.

Cape of Good Hope.—There were only five vessels employed on this station, with a mean force of about 890 men. The total number of deaths was 17, of which 7, or a ratio of 7·8 per 1000 of mean force, were from disease; those from accidents were 10, or in the ratio of 11·2 per 1000. The total number invalided was 36. Considering the unhealthy nature of the coast from Port Natal to Zanzibar, off which the vessels were frequently cruising for months continuously, febrile diseases were of rare occurrence.

East India and China Station.—The squadron on this station consisted of twelve vessels, with a mean force of about 3410 men. The ratio of deaths from disease was 26·7, that from wounds, accidents, and causes not known, 7·3, in all 34 per 1000 of mean force, a rate which must be regarded as small, when the nature of the Chinese climate, and the duties in which the force was employed, are taken into consideration. The most prevalent disease was diarrhœa, of which there were 969 cases, 2 only fatal. The greatest number of deaths were from dysentery, 48 out of 293 cases. Diseases of the sexual organs were numerous and severe, 441 cases of syphilis, and 179 of gonorrhœa and orchitis. The greater part of these were contracted at Hong-Kong, where syphilis appears to exist in more than usually virulent form. Of continued and remittent fever there were 448 cases and 11 deaths. Of 242 cases of intermittent fever none were fatal. The total number invalided was 71, and that of deaths 116.

Australian Seas.—There were only five vessels employed in these seas, with a mean force of about 540 men, who enjoyed, as might have been expected from the well-known salubrity of these regions, a high standard of health, whether in port or at sea. The deaths from sickness were in the ratio of only 5·6 per 1000 of mean force. No death occurred from external violence, but six men were drowned by the upsetting of a boat in a squall, making the deaths from accident 11·1 per 1000 of mean force. It happened, however, curiously enough, that while the mortality from disease was less on this station than on any other, the rate of invaliding was greater. This was caused by the appearance of dysentery in one vessel, the *Herald*, and of epidemic dropsy in another, the *Juno;* the former is a disease of rare occurrence in any part of Australasia, and the latter is one of which there is no other example in the records of medicine. We here condense the account of the epidemic ascites, which for some time prevailed on board the *Juno*.

The first case was placed on the sick list in the beginning of January, a short time after the ship had returned to Sydney from a long cruise among the islands of the Pacific. A sporadic case had occurred in May of the preceding year, which did not differ materially in its aspect from those forming the epidemic of 1856. Unlike any of these, however, it terminated fatally. The crew, while at sea, had been healthy, though latterly they had shown a disposition to ulcers, owing, it was supposed, to the long continuance of salt rations. The subject of the first epidemic case, previously a strong, healthy man, was sent to the Military Hospital at Sydney, where he remained for a month, and then joined the ship improved in health; but, beginning soon afterwards to look pale and sallow, and not being likely to recover on board, he was discharged to the military hospital at Hobart Town. On the 3rd of March, two similar cases occurred, which were sent to the military hospital at Sydney, the ship having returned thither. The *Juno* left Sydney on the 8th of March, and arrived at Hobart Town on the 13th. On the 16th, another case occurred, and another on the 19th. About the same time two more men were attacked; these had previously been suffering from abscess and ulcer. From this date up to the 28th, when the ship sailed for Sydney, one or more cases occurred almost daily, till their number amounted to eighteen. In general, the patients, among whom were a large proportion of the strongest men in the ship, exhibited little or no constitutional disturbance;* but their complexion was sallow or pale, and they complained of mental depression, a feeling of uneasiness, and a troublesome barking cough, which had generally existed for some days or weeks previously to the invasion of dropsy.

As no such epidemic was known either at Sydney or Hobart Town, and as there was nothing detrimental to health either in the duties or the diet of the men in the *Juno*, it was difficult to form any opinion as to the origin of the disease; but as it was confined to the crew, and the majority of the cases to the same locality in the ship, it was supposed that the exciting cause might exist in the holds, though they were free from offensive effluvia, and appeared to be thoroughly clean. As it was not possible to clear out the hold till the ship returned to Port Jackson, it was thought advisable to cleanse the lower deck. We omit to describe an accumulation of dark slimy matter, which was found under the decayed shot racks, because, as Dr. Bryson truly observes, similar collections of matter have existed in innumerable instances both in houses and ships, without producing any disease of the nature of that in question. The ship again left Hobart Town for Sydney, which she reached on the 6th of April. Two or three days afterwards the crew were landed, and those who were still in good health went into barracks; the sick were lodged in hospital, and the officers, midshipmen, and warrant officers remained on board. The ship was then cleared of her stores and thoroughly washed, cleansed, and fumigated. Some black slimy matter was found about the keelson and under the limber boards, but not more than is usual under similar circumstances. It did not appear that the persons who were most constantly engaged in cleaning the holds, were attacked in greater numbers than others. Five cases occurred after the crew landed: one on the 16th, one on the 17th, and one on the 19th, 23rd, and 28th respectively. On the 9th and 10th of May, the men returned to the ship. One case occurred on the 9th, one on the 23rd, and one on the 2nd of June, with which the epidemic terminated. Two cases of œdema of the feet occurred in the *Fantome*, but in no other vessel on the station was there any disease of a dropsical character.

The surgeon of the ship, a most intelligent officer, was of opinion that the source of the disease existed in the accumulation on the lower deck already alluded to; while the assistant-surgeon, in a well-written report, considers the disease as dependent "on causes producing a morbid state of the fluids, which most resembled their condition in scorbutus," though he admits that the former cause may have exerted a predisposing influence. We quite agree with Dr. Bryson as to the inadequacy of both these hypotheses to account for the phenomena.

Irregular Force.—Besides the vessels employed on the several naval stations or commands, there were others employed irregularly, or on special duties, which did not remain for any great length of time in any one region or locality, and which, therefore, in a statistical point of view, require to be included under a separate head. These ships were seventy-three in number, and consisted of ships of the line, frigates, and smaller vessels. Their mean force was about 10,000 men. The total number of deaths was 105, or in the ratio of 10·5 to 1000 of mean force. Of these 78 were from diseases, and 27 from accident, the former being in the ratio of 7·8, and the latter of 2·7 per 1000 of mean force. The daily average of men ineffective from wounds and sickness was, for ships of the line, 39·5; for frigates, 45·8; and for smaller vessels, 40·2. The total number invalided was 136.

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* Febrile disturbance we presume is meant, for men with the symptoms mentioned could hardly be said to be free from constitutional disturbance.

There are, of course, fewer data for general sanitary observations among so scattered a force, than in the case of squadrons employed in particular regions and services; nor do we here find anything relating to the prevalent diseases which need detain us.

Total Force.—There is a concluding section thus headed, some of the information contained in which we have anticipated in our notice of particular stations. The total mortality from disease in the navy for the year 1856 amounted to 629, or 71 per cent.; that from accidental injuries, wounds received in action, drowning, and suicide, was 172, or 21 per cent.

The most fatal disease was fever, from which there were 199 deaths, or nearly 25 per cent. of the total mortality: of these deaths 130 were from yellow fever, of which 105 occurred in five small vessels—thus showing how the introduction of a fatal disease into a few vessels, not containing a fiftieth part of the force, may influence the death-rate in the naval service. Next to fever, consumptive diseases were more destructive to life, having been in the ratio of 2·7 per 1000 of mean force. The deaths from all diseases of the respiratory organs were 175, forming 21·9 per cent. of the total mortality. The deaths from disease of the heart and blood-vessels were 4·9 per cent., and those from diseases of the alimentary canal, 13·4 per cent. Of 64 deaths from dysentery 48 occurred in the vessels employed on the coast of China. The total number of men invalided in the navy was 998, or 19.3 per 1000 of mean force.

The mortality from all causes, namely 15·5 per 1000 of mean force, compared with the mortality in civil life in England appears high; but, to make a just comparison, we must strike off at least two-thirds of the accidental deaths in the navy, as well as the deaths from yellow fever, which would reduce the total mortality in the service to about 10 per 1000 annually—showing that the real mortality in the navy from all ordinary causes, is about the same as in the more healthy communities of men living on shore.

The aggregate loss of service from wounds and sickness in the entire navy was in the ratio of 61·7 per 1000, which exceeds the loss in the metropolitan and city police by a little more than one-third. These, it is assumed, are the only bodies of men beside the navy in which this mode of estimating the extent of sickness has been adopted. When we consider that a large proportion of the naval force is employed within the tropics, in regions highly detrimental to European constitutions, and that the police have much better means than the sailors of guarding against the evils arising from exposure to weather, by drying their wet clothing, and refreshing themselves with warm food and drink, the difference in the ratio of sickness is less than might have been expected. Add to this, that, in the police force, a shilling a day is stopped out of each man's pay as long as he is unfit for duty from ill-health, so that it is the interest of every man to keep off the sick list as long as he can; whereas, in the navy, there is no such regulation, so that indolent men and malingerers remain on it as long as they can impose on the medical officers. The majority of the police also are married men, so that they may be supposed to be less liable to contract venereal diseases than the seamen and marines of the navy.

On the whole, this is a very able and satisfactory Report, defective in very few particulars of any importance, though it is, perhaps, written in a more diffuse and less logical style than might have been desired in a document of this description. Here and there we meet with an ambiguity of expression which renders the exact meaning a little obscure; but this occurs chiefly with respect to minor details, and may arise from ambiguities in the returns from which the Report is drawn up: in such cases it is certainly better to leave the meaning a little indefinite, than to determine it too precisely in a direction which may happen to be the wrong one.

REVIEW III.

Mémoires de l'Académie Impériale de Médecine. Tome XXI. pp. ccxlvi. et 600; Tome XXII. pp. cl. et 542. 4to. Paris, 1857–58.

THE two volumes of the 'Mémoires' we have here to notice do not contain any papers of commanding interest; but there are some the contents of which may be usefully made known to our readers.

I. *The Pathological Anatomy of Cysts.* By L. BAUCHET.—After passing in review the different classifications of cysts that have been proposed, M. Bauchet prefers that one derived from the nature of the matters or substances which constitute the contents of these bodies. It allows of the establishment of clear and precise divisions of the subject; while the nature of its contents being known, the structure of the cyst itself can almost always be predicated. In this way he constitutes ten varieties of cysts: 1. synovial; 2. serous; 3. mucous; 4. sebaceous; 5. milky; 6. hæmatic; 7. purulent; 8. parasitic; 9. fœtal; and 10. pilous cysts. Of some of these subdivisions are made. Of course we have not space to go through the catalogue, and will confine ourselves to what the author states respecting *synovial cysts.* These he divides into articular, tendinous, and submuscular.

1. *Articular Synovial Cysts.*—These, again, constitute three varieties, accordingly as they are formed at the expense of the synovial membrane, outside of this, or by the synoviparous follicles. The *first* of these varieties is the only one that has been described even in our most recent works. A fold of synovial membrane protrudes through an aperture in the protective fibrous covering, and its communication with the synovial cavity is obliterated, a pedicle only remaining. The *second* variety, the author does not admit as of independent origin, but believes it originally constituted a hernia of the synovial membrane as above described. The *third* variety is the most interesting and curious, the true nature of its origin having been first established by M. Gosselin. There exist in joints culs-de sac or follicles in a greater or less number, to which he gave the name of *synoviparous follicles.* By the examination of numerous joints, and especially the carpal, we are able to trace the evolution of this form of synovial cysts. Just as with regard to the sebaceous or mucous follicles, we may observe the membrane thrown into relief by little transparent elevations, varying in size from a pin's head to a pea or nut, and having at some part of their parietes a little black point, which cannot be better compared than to the lachrymal point. These are synoviparous follicles having their ducts obstructed and more or less filled with thickened synovia. The little cyst increases more or less in size, according to the region it is placed in and the amount of pressure it is submitted to. These, in fact, constitute the cysts usually met with—those due to hernia of the synovial membrane being rare. The contents of these articular cysts usually resemble synovia, but when the more fluid parts have been absorbed it may assume the consistency of apple or quince jelly. It is rarely of a serous or sero-sanguinolent consistency. Epithelial cells are sometimes found floating in the liquid, or forming little vegetations projecting from the internal wall. In true articular cysts *hordeiform bodies* have not been met with. The sac is of the same structure as the articular synovial membrane, excepting that the fibrous investment is often more dense.

2. *Tendinous Synovial Cysts.*—These vary accordingly as they are confined to one or more tendinous sheaths, and do or do not communicate with this. Cysts of con-

siderable extent, occupying a sheath common to several tendons, are usually of a chronic nature, and almost always contain the hordeiform bodies. These are not usually met with in the common variety of cysts which implicates only one sheath. This is generally more acute in its progress, and frequently exhibits sanguineous, pseudo-membranous, or purulent contents. The *hordeiform bodies* have been considered by some to have arisen from hydatids, and by others to be minute sanguineous coagula; but their true nature seems to be that of vegetations springing from the inner wall of the cyst. They consist in fibrous or cartilaginous tissue, and their pedicle easily giving way, they become free in the cavity of the sheath, deriving a polish from rubbing against each other. They impart a peculiar consistency to the cyst, and give to the examining finger a rubbing sensation, such as may be produced by introducing beans into a vessel full of water—a sensation difficult to describe, but never forgotten when once felt. The absence or presence of these bodies is of importance with respect to prognosis and treatment. Detached and free in the cavity of the sheath, they have no tendency to become absorbed, and present an obstacle to the radical cure of the disease by internal and external resolvents.

3. *Submuscular Synovial Cysts.*—Under this appellation M. Bauchet describes the enlargement of the submuscular bursæ, when these do not communicate with the articulations.

II. *On Bloodletting in Pregnancy.* By M. Silbert.—This is one of the Academical Prize Essays. The author believes that as a consequence of the reaction against the abuse of bleeding in pregnancy, practitioners in our own times are too sparing in its employment. There is, in fact, a tendency to the same exaggeration with respect to the chloro-anæmia of pregnant women which formerly prevailed with regard to plethora.

> "That great consideration should be paid to chloro-anæmia in the pathology of pregnancy is right enough, but only on the condition also of not overlooking plethora, a state of complete reality, and which did not exist merely in the imagination of our predecessors. It is only by studying pregnancy under this double point of view, and taking into account at the same time the 'nervous condition' and albuminuria, which also play their part in the production of the accidents with which it is accompanied, that we can embrace the entire truth. This is not to be done by sacrificing one point of view to the other. The determination of the relative frequency of these different conditions as causes of the diseases of pregnancy, would be of very great importance as regards the general indications for bleeding; but this point of medical statistics is far from being elucidated, and it is not in a restricted circle like Paris that the elements of a very exact appreciation are likely to be obtained. For the practitioners of great cities, whose observations are for the most part made on women etiolated by misery in the hospitals, or relaxed by all the delicacies of luxury in private practice, chloro-anæmia and the 'nervous condition' predominate in the pathology of pregnant women. But the country practitioner attributes to the richness and exuberance of the blood most of the accidents which accompany gestation." (tome xxi. p. 117.)

Having premised thus much, M. Silbert divides his subject into three parts; in the first he treats of its history, in the second he considers the general indications for bleeding in pregnancy, and in the third he passes in review the particular cases which may call for its administration. We pass over the historical part, and proceed to the next.

General Considerations on Bleeding in Pregnancy.—The modifications which pregnancy produces in the system are of two kinds: first *anatomical* and *functional*, which are constant and essential to pregnancy, having their seat in the uterus itself; and secondly, *sympathetic*, which are eminently variable. The accidents which are due to the mere physical development or acquired functions of the uterus are admitted at all hands to be best allayed by antiphlogistic treatment. The sympathetic modifications, which are of great importance in the pathology of pregnancy, are referrible to three heads: (1), to disturbances of the nervous system; (2), to notable changes in the composition and quantity of the blood; and (3), to the presence of albumen in the urine. These three conditions have then to be considered in relation to the question of bloodletting.

(1) *Disturbances of the Nervous System.*—To this condition must we attribute not

only the extreme moral irritability which some pregnant women exhibit, but also a good share of those functional disorders which are so common, so intense, and so obstinate, and which have been too frequently attributed to plethora, as vertigo, loss or perversion of the senses, and disturbances of the circulatory, respiratory, or digestive functions, as shown by palpitations, syncope, vomiting, &c. These generally disappear or become diminished with the progress of pregnancy in women whose nervous system has only become disturbed by the fact of the pregnancy itself; but it is otherwise with those in whom the pregnancy finds this condition of the nervous system already acquired. This "nervous condition" has been laid down by Sandras as a formal contra-indication to bleeding, when it is unconnected with cerebral plethora, and the prohibition is justified by the close relationship which usually prevails between impoverishment of the blood and the production of nervous disorders. But in pregnancy the diminution of the globular element of the blood will not explain most of these nervous disturbances, inasmuch as these in general disappear before the blood has undergone any notable improvement in this respect. The sympathetic excitement is in many cases the direct consequence of the irritable state of the uterus, and capable of relief by small general or by local bleeding. Moreover, the "nervous condition" is an unequivocal cause of uterine plethora. The women who menstruate most are not the strong and plethoric, but the nervous and delicate. Great care is indeed required in employing bloodletting in the nervous affections of pregnancy, especially towards the end of this, but when the state of the strength permits it, the contra-indication must not be regarded as absolute.

(2) *Modifications in the Composition and Quantity of the Blood.*—Modern researches have shown that,—1st. The globules diminish from the commencement to the end of pregnancy, their proportion rapidly decreasing from the seventh month. 2nd. The fibrine, slightly diminished during the early months, is then increased somewhat to the seventh, becoming much augmented during the two last months. 3rd. The albumen diminishes progressively, though only to a slight degree, throughout the whole of the pregnancy. Although these modifications cannot be called pathological, yet is the relation which such blood bears to chloro-anæmia so strong as to lead to the same pathogenic character being attributed to the latter as formerly attached to plethora. M. Cazeau's views concerning the agency of chloro-anæmia are certainly too exclusive. The condition of the blood in pregnancy is, in fact, quite peculiar and special. In some exceptional cases, true chloro-anæmia may prevail, but it is rather as a coincidence and an exaggeration of a condition already present, than a consequence of the sympathetic reaction of the uterus. But true plethora may also prevail in women notably predisposed, or it may do so temporarily and at different stages of the pregnancy in those in whom it is only a result of the increased vitality dependent on pregnancy. And depletion requires to be used with more caution when plethora is a temporary, accidental condition, than when it is a habitual state, aggravated by pregnancy. Even in serous plethora, in which, with a diminution of globules, there may be a proportionate increase of serum, and in which ferruginous preparations may be called for, the mass of the blood being also augmented, careful depletion is not the less indicated. Mere mechanical plethora, determined by the pressure of the uterus during the latter months, may also call for palliative bleeding.

(3) *Albuminuria.*—Although pregnancy may run through its course quite uninterfered with when albuminuria is present, at other times it becomes a most grave complication, signally favouring the production of sanguineous or serous congestions, which in a great number of cases are the point of departure of alarming accidents. Bleeding may often be advantageously resorted to in order to ward off such consequences, when albuminous nephritis coincides with pregnancy, and when the condition of the urine, analogous to that observed in the anasarca consecutive to scarlatina, implies renal congestion. Albuminuria, considered in itself, is most often connected with asthenia, and therefore bleeding is contra-indicated; but the peculiar conditions observed in the pregnant woman often compel practitioners to depart from this rule, no one hesitating, when uterine or cerebro-spinal congestions become menacing, to have recourse to this means.

In the sections *on the inconveniences and dangers of bleeding in pregnancy*, the author makes several quotations, in order to show that injudicious depletion during pregnancy, by impoverishing the blood, may give rise to abortion, and predispose to disease, especially to puerperal fever.

We pass on to the third part of the work, treating of the particular circumstances which may call for bleeding.

1. *Bleeding in the Diseases proper to Pregnancy.*—As long as the exaggerated ideas concerning the plethora of pregnancy prevailed, bleeding was performed without any reserve in all the diseases of pregnancy, and although any such excess would now be unjustifiable, yet does bleeding still constitute our principal mode of treating such affections. This arises from the fact that whenever they reach a certain point, the usual result is the production of congestion. The causes of the diseases of pregnancy are (*a*) the anatomical and functional changes in the uterine system, and the fluxion of which the pelvis is necessarily the seat during gestation; (*b*) the mechanical obstacle which the development of the uterus opposes to the free play of the organs: (*c*) the sympathetic reaction excited by the uterus in certain organs; and (*d*) the influence which the general modifications of the nervous system, the changed conditions of the blood, and the existence of albuminuria, exert upon the economy. Any of these four causes may act in an isolated manner, but usually more than one act together, and concur in the production of the accidents. It would be difficult, therefore, to consider the diseases of pregnancy by distinguishing them according to the causes which give rise to them; and the author prefers dividing them into idiopathic and sympathetic diseases. The former have their seat in the uterus and pelvic organs, and are the result of anatomical and functional changes; and the others interest distant organs, being due to the reaction which the condition of the uterus exerts upon the entire economy.

(1) *Idiopathic Affections*—(*a*) *Uterine Plethora or Congestion.*—This may be sometimes dependent upon a state of general plethora, but it is oftener found in nervous, albuminuric, and hydropolyæmic subjects. Not only does uterine plethora exert a great influence on the production of uterine hæmorrhage and premature contractions, but it determines almost the entire pathology of the ovum, placental congestion and apoplexy being, in fact, intimately dependent on it. Although it may appear at any period, it is yet during the first half of pregnancy that it is most commonly met with. Bleeding is the treatment indicated, the amount of this being regulated by the nature of the cause giving rise to the plethora. (*b*) *Hæmorrhage* is commonly a consequence of uterine plethora, and it should be treated by bleeding, when there is evidence of the permanent operation of an active cause, and especially during the first six months. At a later period greater circumspection is required. (*c*) *Premature contraction* is a frequent consequence of congestion, and especially of hæmorrhage, and bleeding is a powerful means of arresting it. (*d*) Among other pathological conditions, *dropsy of the amnios and hydrorrhœa* admit only of bleeding when signs of congestion and plethora are present. (*e*) *Uterine neuralgia* is sometimes dependent on plethora. (*f*) *Uterine rheumatism* is usually best treated by depletion.

Passing on to the affections of other organs than the uterus, which arise from the physiological fluxion taking place towards the pelvis, we have *congestion* of *the broad ligaments*, which, although a rare affection, must still be borne in mind. The hæmorrhagic molimen of the veins of the rectum, giving rise to *hæmorrhoids*, may become an active cause of abortion. When connected with a state of plethora bleeding should be resorted to, while when the hæmorrhoids are inflamed and painful, leeches may be safely applied, although they are often but of little use. *Cystitis* is not a rare occurrence in pregnancy, and the softening of the *pelvic ligaments*, which is so constant an occurrence, may go on to a true inflammation.

(2) *Sympathetic Affections.*—The great benefit derivable from blood-letting in the idiopathic affections of pregnancy is not obtained in the management of the sympathetic affections. (*a*) *Affections of the Breast.*—Although it is rare for the changes which take place in this organ to assume a morbid character, yet in some instances a true phlegmasia may be developed, and depletion be called for. (*b*) *Disturbance of the digestive organs.*—The stomach is the organ which, of all others, is most readily and most

deeply influenced by the sympathetic reaction of the uterus. In the case of obstinate vomiting, in place of applying means after means to the stomach itself, our attention should oftener be turned to the uterus, whether for rectifying malposition or abating congestion and inflammation. (*c*) *Neuralgias.*—The various forms of these (as cephalalgia, odontalgia, tic douloureux, vulvar pruritus, &c.), to which pregnant women are liable, have almost ceased to be treated by bleeding since the time of Valleix; but that author attributed too much to the agency of asthenia in the production of these affections, for depletion may be advantageously used when the patient is not anæmic, and symptoms of general excitement are present. (*d*) *Vertigo and syncope* should be treated by bleeding or not according to the nature of the cause which has produced them, (*e*) *Eclampsia.*—Whatever difference of opinion may prevail with respect to the nature of this, all are pretty well agreed as to the necessity of bleeding; and not only is this required in the actual attack, but as a preventive, and especially when albuminuria is present or eclampsia has occurred in a former labour. (*f*) *Partial paralysis* is sometimes observed towards the end of pregnancy, chiefly in primiparæ. The causes are often obscure, though the affection usually seems connected with chloro-anæmia, hystasia, or albuminuria. It usually disappears of its own accord, and bleeding should not be resorted to except in the robust and plethoric. (*g*) *Disturbances of the respiratory and circulatory organs.*—The disturbance of respiration during the later months is due to a mere mechanical cause, thrusting up the diaphragm; but when dyspnœa is observed at an earlier period it may be due to the nervous condition or to congestion or œdema of the lung, and according to the nature and prevalence of these causes the treatment with regard to bleeding must be regulated. In some cases palpitation of the heart is also due to local congestion, and may call for depletion; but such cases are rare. Cough, when dependent upon such condition, is best relieved by moderate depletion. (*h*) *Dropsy of the cellular tissue.*—This is not always due to the obstacles offered to the venous circulation, or to the co-existence of a disease of the heart, and albuminuria must be taken into account, in consequence of the frequent occurrence of convulsions when it is present.

2. *On Bleeding in the Intercurrent Diseases of Pregnancy.*—For the bulk of these the treatment differs but little from that which is proper in the non-pregnant condition. As a general rule, prudence in bleeding is advisable; but there are cases in which the greatest energy is alone sufficient, for not only may some of these affections exert an injurious effect upon the progress of gestation, but they themselves may be influenced by the changes incident upon the increase of size of the uterus. Expectation, which would be proper in the unimpregnated condition, may be misplaced here. The superabundance of fluids, or polyæmia, so frequently met with in pregnant women, should also be borne in mind as an additional reason for employing the lancet.

3. *On Bleeding in Narrow Pelvis.*—The author agrees with M. Depaul, that in certain cases of narrow pelvis it is preferable to seek to diminish the size of the fœtus by rigid diet and bleeding, to resorting to premature labour.

III. *On an Epidemic of Measles which prevailed at Abbeville.* By M. Hecquet.—Speaking of the town of Abbeville (pop. 19,304), M. Hecquet states that it presents the unfortunate peculiarity that for the last fifteen years the number of deaths has been in excess of that of births, the deaths during 1841–55 having amounted to 8171, and the births to 6967. The essay is based upon 205 cases observed during an epidemic at the early part of 1855, the rubeola being characterized by active inflammation of the buccal and other mucous membranes at its onset, irregularity in its course, and the production of great exhaustion of the powers and various accidents at its close.

Incubation.—A careful examination of the instances of the disease observed in the same families enabled the author to fix the period of this with exactness in 74 cases, in which it was found to vary from three to eighteen days. In 12, the first symptoms were observed at the end of three days; in 13, after the fourth day; in 13, after the fifth day; in 9, after the sixth day; in 10, after the seventh day; in 7, after the eighth; in 10, after the ninth day; and in 20, at still later periods.

Period of Invasion.—This varied from twenty-four hours to six days. Among the affections of the mucous membrane accompanying it was a severe form of laryngitis, closely simulating croup in the sounds it gave rise to. After three or four days the tone was changed, and the cough became loose. In spite of its intensity, there was only 1 case in which any membranous secretion was present. Convulsions were observed in 14 cases, varying in duration from several hours to two days. The treatment of these consisted in favouring the development of the eruption by warm air, and applying rubefacients to the lower extremities, a little ether being given at the same time internally. The author is convinced that the expectant treatment of the initial convulsions of the eruptive fevers, as indicated by Guersaut and Baudelocque, is very preferable to that of a more active character.

The Eruption.—In 11 cases this manifested itself twenty-four hours after the first precursory symptoms; in 82 instances, on the second day; in 77, on the third; in 31, on the fourth; and 4 times on the sixth. In three-fifths of the cases it became developed between 8 P.M. and nine A.M.; and in the other two-fifths between nine A.M. and eight P.M. There was no relation observed between the confluent character of the eruption and the development of complications. In more than half the cases the eruption appeared upon the trunk and limbs before the face; in 9 cases no eruption whatever was observed on the face; and in other cases slight and pale traces could only be detected on the chin and around the alæ nasi, although the trunk and limbs might be intensely affected. The appearance of the eruption was never accompanied by a moderation of the fever or other concomitant symptom, the irritation of the air-passages becoming, indeed, more severe, and extended. In 9 cases the eruption disappeared on the first day; in 32, on the second; in 98, on the third; and in 66, on the fourth day. In only 17 patients was anything like desquamation observed.

Prognosis.—During the whole epidemic the rubeola pursued a very irregular and insidious course, complications springing up under the most unexpected circumstances; so that even with the most regular commencement of the disease the prognosis required to be most guarded. The gravity of the disease was always found to be inverse to the age of the patient. Between 17 months and four years death took place in four-sevenths of the patients; between four and eight years, in a little less than one-third; and between eight and fifteen, in one-tenth, no patient older than fifteen dying. It was especially during convalescence, when all danger appeared to have passed away, that caution was especially required.

Predisposing Causes.—First, as to the age, 93 of the patients were between one and four years old; 76 between four and eight; 25 between eight and fifteen; and 11 between fifteen and twenty-eight. There were 108 male to 97 females. All classes of society furnished their quota; but the mortality was much less considerable in the upper classes—bad and insufficient food, owing to the dearness of bread, having placed the children of the poor in a very unfavourable condition for struggling against the epidemic influence. M. Hecquet observed the *meteorological* phenomena with great exactitude, but we have not room for his details. One of the results was, that of the 140 cases of pneumonia complicating rubeola, seen by himself and a colleague, the great bulk occurred in March, April, and May, months in that year remarkable for their sudden changes of temperature. Without denying the great part cold plays in the production of pneumonia, this complication of measles is dependent upon other than mere thermometrical conditions of the air. In this epidemic children were seized with it who were in nowise exposed to cold or to any other anti-hygienic influence. The medical constitution will alone explain the great prevalence of pneumonia on this occasion.

Complications.—1. *Pneumonia.*—Of the 108 males and 97 females the subjects of rubeola, 43 males and 30 females also suffered from the complication of pneumonia, the females, therefore, showing a somewhat less disposition to contract pneumonia than the males. The pneumonia set in suddenly and rapidly, when the mildness of the symptoms inspired no inquietude, the affection of the air-passages only a few hours before seeming a mere secondary element of no consequence. Initiatory shivering was only noted in fifteen instances; and the crepitant *râle* was less distinct and less durable than in ordinary pneumonia, owing partly to the presence of mucous *râles*, and partly to the rapidity with which the disease ran its course. Sometimes the *souffle*, which announced its passage into the second stage, was heard at the end of forty-eight hours. In 3 patients the disease terminated fatally between the fourth and sixth days. Of all the symptoms the *dyspnœa* was the most prominent and the most important. The number of inspirations were carefully noted, and were found to vary between thirty and sixty. This symptom was found to be a valuable one for the *prognosis* in the case of children. Of the 73 patients, 22, or a little less than one-third, died. The influence of age upon the mortality is seen from the following figures: between 15 months and 4 years, there were 37 patients, with 21 recoveries and 16 deaths; between four and eight years, 25 patients, 19 recoveries and 6 deaths; between 8 and 15 years, 7 patients, 6 recoveries and 1 death; and between 15 and 28 years, 4 patients and 4 recoveries. The diminution of the rapidity of respiration was the only sign of approaching recovery that could be relied on. All others might be present, and yet without this, would prove fallacious.

2. *Stomatitis.*—This complication was observed in the erythematous, ulcero-membranous, and gangrenous forms. *Erythematous stomatitis* was observed in almost all cases in different degrees, an intense redness of the mucous membrane persisting long after the exanthem had disappeared. In several cases the gums were turgescent, softened, and easily made to bleed. Borax constituted the best application, and when the affection tended to become chronic, tincture of rhatany was employed. *Ulcero-membranous stomatitis:* In some cases the mucous membrane became more or less deeply ulcerated, pseudo-membranous exudations covering deep ulcers, and being soon reproduced when removed. The breath was terribly fetid, though not in the same manner as in gangrene. This form was observed in 27 instances, and in 16 of these it accompanied pneumonia or other complications. Of the 27, 9 died during the course of the stomatitis, 8 succumbing to pneumonia, and 1 to enteritis. Of the 18 cases which recovered, in 11 the ulceration, as fast as it healed up in one part, spread to another, much prolonging the case; and in the others, in which it was confined to its original seat, from ten to fourteen days elapsed before it was healed. *Gangrenous stomatitis:* Of the 7 subjects in whom this appeared, it came on during pneumonia in 6, and during gastro-enteritis in 2. In 4 of these, the gangrene was limited to the gums, but in three others spread to the face. Five of the cases proved fatal and 2 recovered. All the children belonged to indigent families, and were exposed to various anti-hygienic influences, especially over crowding. The details of several of the cases are furnished by the author.

3. *Enteritis.*—This was observed at different periods of the rubeola and its incidental diseases. It was, indeed, seldom present without other complication. In only 2 out of 26 patients dying as a consequence of the rubeola, could their deaths be attributed solely to enteritis. Intestinal affections, particularly entero-colitis, were chiefly observed appearing towards the end of pneumonia or stomatitis. The diarrhœa, which frequently set in during convalescence, was not of serious moment.

Convalescence.—This was always difficult and prolonged, and often disturbed by accidents; and its duration was not always proportionate to the intensity of the disease. Often when the disease had seemed to pursue a favourable and uncomplicated course, accidents would occur when the patient seemed out of all danger. In 18 instances the persistence of the bronchitis delayed convalescence; in 5, abscess of the face or cervical region appeared towards the decline of the disease; and in 4, anasarca, unattended with albuminuria, was developed during the convalescence from pneumonia.

Treatment.—In consequence of the serious nature of the epidemic, every possible means was resorted to in order to preserve children from its influence. In 40 instances, the belladonna treatment was pursued, but only in one of these did the child escape th disease. The author is of opinion that although in benign rubeola expectant treatmen is alone called for, more active procedures are required on the occurrence of serious complications. While treating the phlegmasiæ, attempts were made to reproduce the exanthem, when this had disappeared, by means of rubefacients, but only in two cases were these successful. Bleeding, as the principal means of treating the pneumonia, was not found useful, the cases in which it was employed resisting most; but in robust children, when the pneumonia was very intense, leeches to the malleoli were sometimes of use. Tartar emetic, too often without effect, was sometimes useful, and at others did harm by increasing the tendency to prostration. In 60 cases large doses of the white oxide of antimony were tried, and in 46 of these it seemed to have been of service. Blistering the thorax was also sometimes of use.

IV. *On Nervous Vertigo.* By M. MAX SIMON.—After adverting to the few accounts of this condition given by the older writers from the time of Sydenham, M. Simon observes that there is scarcely a writer of the Paris school, with the exception of Trousseau and Sandras, who acknowledges the existence of vertigo otherwise than as a symptom of some anatomical cerebral lesion. M. Simon adduces his own and other cases in proof of the occurrence of such vertigo independently of cerebral congestion. Considering it, then, as an essential affection, without appreciable lesion of the nervous system, and without the concomitance of other general conditions of the economy which explain its nature, he divides the individuals liable to nervous vertigo into two groups. The first are persons who, the subjects of vertigo, exhibit no dynamic or statical disturbance of either the nervous system or any other of the apparatuses of the economy that can explain its occurrence. This is *idiopathic vertigo*, properly so called, a pure neurosis. In the second group are placed those cases in which the vertigo takes its origin in the functional disturbances of some other organ than the brain, and consequently becomes mingled with various accidents, from which, however, it is separable by the clearness of its manifestations, its intensity, the frequency of its return, and its duration. This is *sympathetic vertigo.* Of the organs thus acting sympathetically in inducing vertigo, the stomach and genital organs stand foremost. Idiopathic vertigo is observed under different forms and degrees of intensity; and the author pursues the subject of sympathetic vertigo at length, as it is met with in dyspepsia, hypochondriasis, venereal excess, spermatorrhœa, the convalescence of disease, and sea-sickness. The duration of the disease is very various; but the prognosis of purely nervous vertigo is usually not very unfavourable. That a different opinion upon this point prevailed among the older writers arose from their not having distinguished between it and the vertigo which is merely symptomatic of other diseased conditions.

Diagnosis.—M. Simon details the distinctions which exist between nervous vertigo and the vertigo observed in *anæmia*, which is frequently characterized by syncope in addition to the symptoms observed in pure vertigo. The elements of diagnosis are not found in the subjective symptoms of the two conditions, which much resemble each other, but in the subjective symptoms furnished by the anæmia. In the vertigo of *plethora* other symptoms are usually present imparting to it particular characters. When in a doubtful case the establishment of a diagnosis becomes urgent, this may be effected by an analysis of the blood, exhibiting the globular element in excess in plethora. The vertigo met with in *organic disease of the brain* may sometimes be confounded with nervous vertigo, but in most cases there are sufficient accompanying symptoms to distinguish between them.

Treatment—Idiopathic Nervous Vertigo.—It is in this form that nervous vertigo is liable to irregular periodical returns, such as are met with in other neuroses; and even when not periodical, it is rare for it not to be attended with more or less prolonged intermissions. During the paroxysm the patient should assume the horizontal position; quietude, looseness of dress, fresh air, and the absence of light being also insisted upon. When the vertigo has arrived at such an extent that the patient loses consciousness, we should also endeavour to assist the passage of the nervous system from the collapse by the employment of aromatic or spirituous frictions, aromatic stimuli, the inspiration of ammonia, &c.; and we may also avail ourselves of any substance which the personal experience of the patient has taught him the advantage of. Generally speaking, however, the paroxysms are not so violent; and we have rather by hygienic means to combat the vertiginous habit dependent upon a morbid activity of the encephalon. It is by gentle stimuli, and especially by those remedies termed by the ancients "nervine," or "cephalic," that this habit is to be treated. In this class the author includes infusions of sage, mint, balm, &c., which, supported by the authority of MM. Andral and Trousseau, the author declares are not the inefficient remedies supposed by some. Other authors recommend more active agents, as valerian and assafœtida. In the *vertigo of dyspepsia*, treatment is often of avail, and Trousseau attaches great importance to counteracting the excess of acid secretion by alkalies. In the vertigo of *hypochondriasis*, moral and encouraging means must be resorted to, for the patient always regards it as portending apoplexy or sudden death.

V. *On the Sudden Death of Puerperal Women.* By M. Mondret.—This prize essay, expanded to a somewhat tedious extent, gives a detailed account of the different forms of functional disturbance observed in pregnancy, which may predispose to disease and death. The various forms of sudden death are then considered, accordingly as they arise from affections of the respiratory, circulatory, or nervous systems.

In the numerous cases he has collected, the author has found that of the affections of the *respiratory* organs which oftenest give rise to sudden death in puerperal women, *pulmonary congestion and apoplexy* stand foremost. Amongst those of the *circulatory* organs, *disease of the heart* may be very unfavourably influenced during pregnancy, and rupture of this organ takes place more readily. The possibility of the admission of air into the uterine veins after delivery seems to have been demonstrated as one of the causes of sudden death; and although the spontaneous development of aeriform fluid is not so certainly established, it appears to have been so in two cases cited. Although organic affections of the *nervous centres* are of the gravest import in the puerperal state, and a predisposition is often established to apoplexy, yet these seldom give rise to sudden death in the above acceptation of the term; complications by some affection of the lung or heart usually also existing when death is so produced. Among *puerperal diseases*, peritonitis sometimes terminates very suddenly, after having passed through its stages in a latent and unsuspected course. Injudicious purgation and excessive tympanites have, too, in some instances been the cause of sudden death. In some of these cases the appearances after death may scarcely seem to explain its occurrence; but we have to take into account their operation upon the morbid nervous sympathies, and especially upon those of the ganglionary system.

> "My last chapter is the most important, as it contains the knot of the problem to be solved; for I have sought in it to determine what are the causes of inorganic origin which may give rise to sudden death in puerperal women. First, we have to observe that death is accompanied by nearly the same symptoms whether a lesion of an organ be present or not; for it is always, in fact, the arrest of one of the three cardinal functions of the economy—respiration, circulation, and innervation. But as these three functions are mutually dependent, and as besides any material lesion, there can be but a dynamic lesion, that is, a lesion of innervation, it is always to this that death must at last be referred in these cases. This must be the case whether the innervation has undergone sideration in its totality, the cause of death operating on the vital principle itself, or whether the innervation of an important organ be alone affected and death produced by the arrest of the function depending upon it. Thus, we may have three kinds of death—nervous apoplexy, nervous syncope, and nervous asphyxia—in which we may find no material cause of death, unless, indeed, this has not been very sudden, for then we may find indications of the condition of suffering under which the function had for a time been carried on. But such signs should not be confounded with those derived from a primary material lesion, and they imply nothing as to the essential cause of death. Moreover, these three causes of death may be combined and separated with difficulty from each other. Nervous apoplexy may sometimes be recognised as existing alone; but it seems impossible to distinguish nervous syncope and asphyxia in all the cases in which they give rise to sudden death. Authors, indeed, have united the two into one affection, under the name of idiopathic asphyxia." (tome xxii. p. 325.)

The author has not met with a case in which death could undoubtedly be said to have resulted from nervous apoplexy alone. This offers indeed the greatest analogy in its symptoms to apoplexy from effusion; and the puerperal state seems equally to predispose to the two affections, so like in appearance but so different in reality. The term idiopathic asphyxia cannot be said to correspond to a single morbid entity, authors seeming to have designated under the same name different affections, the nature of which is as yet undetermined, which are developed silently, and terminate suddenly and unexpectedly—unexplained by any apparent organic lesion. Flaccidity of the muscles of the heart and emptiness of its cavities are negative characters which may depend upon various causes. Fatty degeneration of the organ can at the utmost be regarded as predisposing to syncope. Admitting death by idiopathic asphyxia, or some affection related to syncope, that this may be expected not frequently to occur in the puerperal state may be deduced from a consideration of the various anatomical and physiological modifications determined by that condition.

We pass over much of the author's paper, and confine ourselves to reproducing his general conclusions:—

> "1. When a woman dies suddenly during the puerperal state, it is highly probable that her death should be referred to that state, whether there did or did not exist an organic affection of a nature to give rise to sudden death. Such affection would then be mischievously influenced, and its termination may have been precipitated. 2. When a woman dies suddenly, a latent organic affection may have existed prior to pregnancy or have become developed under its influence. In these rather numerous cases an autopsy can alone reveal the true cause of death. 3. The introduction of air into the uterine veins is possible soon after delivery, before contraction has taken place; and this is a material cause of sudden death that may easily be overlooked. The spontaneous development of gas in the blood may be of easier occurrence in the puerperal state; but facts are not yet sufficiently assured to maintain its reality. 4. There is reason to believe that the puerperal state predisposes to the formation of sanguineous concretions in the heart and large vessels. This is a material cause of sudden death which may often be misunderstood. 5. The chloro-anæmic state, frequently met with in pregnant women, seems to predispose them to sudden death. At all events it is a debilitating cause, diminishing vital resistance. 6. All debilitating causes would seem to predispose to the occurrence. It seems to me that this is why sudden death occurs oftener among multiparæ than primiparæ, and in persons of a lymphatic and nervous constitution than in those who are more robust. 7. All sudden puerperal deaths, inexplicable by an anatomical lesion, seem to be the result of a nervous affection, the point of departure of which may be located in the cerebro-spinal or in the ganglionary system. 8. The pernicious effects upon the pregnant woman of pain, of vivid moral emotions, errors of regimen, &c., are incontestible. Under all these circumstances the nervous element is powerfully affected; and it is highly probable that we should not seek elsewhere for the cause of inexplicable sudden deaths, which take place in the puerperal state, beyond all prevision, and without any organic lesion." (tome xxii. p. 332.)

VI. *On the Yellow Fever.* By M. DUTROULAU.—In this memoir M Dutroulau, a medical officer in the French navy, describes two epidemics which he witnessed at Martinique in 1851, and at Guadaloupe in 1853–6. We confine ourselves to his observations on the *treatment* of the disease. In his view, the disease consists in a peculiar alteration of the blood, determined by a specific miasm, and passing through two phases entirely opposed to each other; one of these is inflammatory, giving rise to the early series of symptoms of irritation and vascular congestion; and the other is the stage of decomposition or dyscrasis, indicated by ataxo-adynamic phenomena. In the first period bleeding was resorted to; one or two venesections, together with numerous leeches, were employed during the first twenty-four hours, warm and aromatized drinks and mustard poultices or foot-baths being used at the same time. Ample evacuations were also induced by purgatives given by the mouth or in clysters. Whenever the yellow fever had been preceded by one or more intermittent paroxysms, or whenever abundant and deceptive sweating masked the true characters of the affection, quinine was exhibited, but it never exerted any effect on the course or ultimate gravity of the disease. Although this antiphlogistic treatment is the rational one to employ in the early stage of the disease, it often failed in its effect or the cases were unsuitable for its application. The author was therefore glad to try the effect of various empirical remedies, which were said to have been of use in former epidemics. From these, however, little or no benefit accrued; and he found the best plan was to modify the antiphlogistic treatment, so as to bring it in harmony with the powers of the patient. When the second stage had set in, the individual symptoms—such as vomiting, hæmorrhage, cerebral complications—were met as they arose. Whatever these predominant symptoms might be, the ataxo-adynamic state which accompanied them was usefully combated with quinine, employed in frictions and lavements. Stimulating drinks were also found useful—such as Madeira diluted with Seltzer water; but musk, camphor, or ammonia, given on account of the typhoid complications, seemed but of slight utility.

The great mortality from yellow fever, whatever mode of treatment may be employed, has led to great attention being paid to the subject of *prophylaxis*, and in the author's opinion experience has now sufficiently shown, at least the means of arresting an epidemic. The disease never spreads beyond a short distance, either in extent or height, from the shore of an infected spot. It is true that the rigorous determination of such distance has not yet been made; and it is probable that it may vary in different localities. Nevertheless, the magnificent establishment at "Camp Jacob," at Guadaloupe, placed at five and a half kilometres only from the coast, and at an elevation of 550 metres above the surface of the sea, has in no wise disappointed the hopes entertained concerning it during the six years of its existence, and may be regarded as the type of preventive localities in the French Antilles. As soon as an epidemic breaks out, the portion of the garrison and of the European civil population not acclimatized, should be evacuated on such spots, all communication being cut off with the infected locality. Vessels at anchor, too, should, when invaded by the disease, evacuate their crew upon the heights; or, better still, if they can put to sea prior to any accident appearing. To retain a garrison at the seaside, or to set sail for more favourable latitudes, having the disease already on board, will almost certainly expose to irreparable misfortune. Vessels already infected, or coming from infected places, should be sequestrated in healthy spots at a distance from European populations, or the preservative means already indicated should be applied to them.

VII. *On the Origin and Development of the Fungus of Muguet (Oidium Albicans).* By Dr. Gubler.—The following are the conclusions with which M. Gubler terminates his memoir:

"1. The concretions of a pultaceous appearance known by the name of muguet are formed by the fungus oidium albicans. 2. Without having recourse to the hypothesis of spontaneous generation, we may admit that the oidium is the product of spores disseminated in the atmosphere, some of which become attached to the entrance of the digestive tube, and there undergo development. 3. As the muguet originates in spores transported through the atmosphere, such spores must be necessarily more abundant where the disease prevails; and the invasion by the cryptogam is more imminent for those inhabiting such localities. 4. Another mode of propagation has been confounded with contagion properly so called. This has been demonstrated by successful experiments, in which the byssoid filaments taken from the mouth of one child, and applied to the healthy mouth of another, have given rise to muguet in the latter. 5. But the spores held in suspension in the atmosphere, or the filaments applied to the mouth, do not necessarily induce muguet, the development of this microscopic fungus requiring conditions which are only found in certain morbid states. The diseases in which muguet has oftenest been met with are derangements of the digestive organs in young infants; and in the adult, the latter stage of phthisis, typhoid fever, and angina. In all these affections there is one common character—viz., a morbid state of the alimentary canal, with a changed condition of the buccal fluids, which from being alkaline have become acids. 6. All leads to the opinion that it is upon this acid reaction that the development of the oidium depends. On the one hand, it is constantly present as long as the cryptogamic vegetation is progressive, or at least stationary; and, on the other, acidity of liquids holding organic matters in solution remarkably favours the production of mould. Finally, clinical practice teaches us that, with the exception of mechanical or caustic agents of destruction, there is no better means of ensuring its radical dispersion than the employment of alkalies. 7. The spores of oidium, then, meeting with an acid medium, germinate rapidly in a soil congenial to them. Their filaments become developed, either among the masses of epithelial cells in a state of desquamation, mingled with concretions of altered mucus or parcels of food, or in the intervals left between the raised epithelium and the mucous dermis, or, again, in the glandular cavities. The fungus lives exclusively at the expense of this humus, and does not penetrate into the interstices of the tissues, nor abstract anything from the circulatory juices. It is then a false parasite. 8. The production of muguet is, then, a simple accident, an epiphenomenon observed in the course of affections varying both as to their nature and their gravity. 10. It may, however, constitute a complication, inasmuch as by obstructing the glandular ducts, by lining the mucous surfaces with a thick and continuous layer capable of impeding sucking and deglutition, by keeping up the acid fermentation of the products of secretion, and by irritating the surfaces to which it has become attached, it may for a period prevent a return to the normal condition. 11. From these propositions we may deduce some therapeutical consequences. In the first place, we must remove healthy children from the vicinity of, and especially from contact with, the subjects of muguet. When it has become developed, it must be mechanically removed from the parts to which it has become attached, the parts being well washed with strong alkaline lotion. Where there is no contra-indication, Vichy water may also be given internally." (tome xxii., pp. 460-2.)

VIII. *Observations on the Toxicological Search for Arsenic.* By M. Blondlot.—The author is of opinion that by the process employed by most toxicologists for the destruction of organic matters by sulphuric acid, a more or less considerable portion of the arsenic is lost, to the risk when the tissues only contain traces of the poison, of missing it altogether. His attention was drawn to the subject by observing, during post-mortem examinations after poisoning by arsenic, portions of this substance in the stomach converted into a beautiful yellow colour by the sulphuretted hydrogen engendered by putrefaction; and he came to the conclusion that arsenious acid dissolved and disseminated in the parenchymata may, under the influence of putrefaction, be converted into an insoluble sulphuret, which MM. Danger and Flandin's method is powerless to detect, inasmuch as in the process of carbonisation by sulphuric acid the sulphuret undergoes no change, and remains as insoluble as before. Moreover, carbonisation by sulphuric acid also gives rise to the formation of a certain amount of insoluble sulphuret. Thus he has found that one-half of the arsenic sought may be lost. To avoid this source of error, after repeated washings with boiling distilled water to remove all the soluble arsenious acid from the carbon, a second washing should be performed with ammoniacal water in order to remove the sulphuret. After careful evaporation to dryness, the residue may be treated by concentrated boiling nitric acid, added several times in small quantities, and the excess of acid having been expelled, a second solution of arsenic may be obtained, which, added to the first, constitutes the suspected liquid to be submitted to Marsh's apparatus.

IX. *A Case of Cæsarean Operation performed with success.* By M. Borie.—This was performed at the Maternité, at Tulle, on the person of a ricketty but hardy primipara, aged twenty-nine, in whom the space between the sacro-vertebral angle and the triangular ligament of the symphysis pubis measured but from five to six centimetres at the utmost. A living child was delivered, and the woman recovered rapidly. Chloroform was employed, and a longer incision than usual was practised in opening the cavity of the abdomen.

X. *On the Amount of Utility of Permanent Exutories in the Treatment of Chronic Diseases.* By M. Zurkowski.—This testimony in proof of the utility of exutories is not derived from original observations made by the author, but from the collection and comparison of cases which have been already recorded. These cases are arranged under three categories.

1. *Permanent Exutories in Chronic Phlegmasiæ.*—Lesions which result from chronic inflammation, when exempt from all diathesic influence, are generally, even after a very long period, susceptible of resolution. Such fortunate terminations have frequently been due to the employment of exutories. It is especially in disease of the articulations that the greatest number of successful cases have been observed. Of 58 of the cases of Pott's disease and white swellings, the exutories were the sole means employed in 22; they were used in conjunction with other means in 12, and in 24 they were resorted to after other measures had failed. A no less positive amount of success has attended their use in chronic myelitis and the consequent paralysis —44 instances of recovery from such paralysis, with or without vertebral disease, being on record. Of 20 cases of amaurosis, 7 were treated exclusively by exutories, and 13 after the failure of all other means; permanent success resulting in the whole. So with 30 cases of various descriptions of ophthalmia, the great bulk of which had previously been treated without success. Besides these, may be mentioned old cases of pleuritic and peritoneal effusion.

2. *Exutories in Tuberculisation.*—The author reports 10 instances of pulmonary consumption treated with success by exutories. These individuals were all the issue of healthy parents, with no antecedent phthisis in their families. There were no concomitant or anterior abdominal affections, signs of scrofula, or disease of the bones or joints. But all the patients had cavities at the upper part of the lung, accompanied by the usual cortege of symptoms.

3. *Exutories in Neuroses.*—Their beneficial effect has been observed in the various forms of these, whether relating to modifications of sensibility, motility, or impressionability, or to aberrations of the perceptions, of the intellectual powers, or of the moral and affective faculties.

Seeing, then, how useful this means may often prove, how comes it that it has fallen into discredit? By reason of the abuse which arose from its indiscriminate employment, whether suitable indications were present or not. Among the conditions which should oppose the use of permanent exutories as a means of treating chronic disease, are the following:—

1. *Deep-seated Alterations of Structure.*—For example, the atrophy or melting down of an organ, which has already given rise to symptoms of resorption or colliquation. In subjects placed even in the most favourable conditions, if the organs have undergone deep-seated alterations, if the general reaction is continuous, giving rise to disturbance of some important function, and especially if nutrition be already deeply impaired, not only have exutories no longer any chance of success, but they may even hasten the fatal termination.

2. *Degenerations.*—Without speaking here of primary heteromorphies, for which no one would think of employing exutories, we allude to those insidious transformations of simply indurated or hypertrophied tissues, which are brought about either by the sole effect of chronicity, or under the influence of some diathesic or hereditary condition.

3. *Tuberculisation.*—Although exutories may exert a beneficial action in cases of isolated tubercles, limited to a circumscribed portion of an organ, they offer no chance of success in general tuberculisation—that is, when the diseased process has been set up in several organs at once, or even in several parts of the same organ. It is from their having been too frequently employed in cases of this nature, that their credit has become compromised to the extent of causing their utility to be doubted in cases in which they are really indicated.

4. *Hereditary Influence.*—This exerts great pathogenic influence in chronic disease. Next to tubercular affections, it is in the neuroses especially that it plays so immense a part. In the examples of epilepsy and insanity, in which exutories have proved useful, the patients have been exempt from this fatal influence. Unfortunately these are the rarest cases; the immense majority are subjected to hereditary influence, and exutories will fail to exert any salutary effect upon them.

Besides the Memoirs we have noticed, there are in these volumes *éloges* on Roux and Magendie; Reports on the Mineral Waters of France in 1854 and 1855; and Reports upon the Epidemic Visitations in 1855 and 1856. In this last, from the pen of M. Trousseau, there is some account of the epidemic of diphtheritis which prevailed at Boulogne in 1856. We have also passed over a paper by M. Poterin du Motel on Melancholia; another by M. Reynal, on Herpes Tonsurans as observed in the horse and ox; and one by M. Michel, on the Applications of the Microscope to the Diagnosis and Treatment of Disease. This last is an able *resumé* of what is known upon the subject, but defies analysis.

REVIEW IV.

On Wounds and Injuries of the Eye. By WILLIAM WHITE COOPER, F.R.C.S., Ophthalmic Surgeon to St. Mary's Hospital, Senior Surgeon to the North London Eye Infirmary, &c. Three coloured Lithographic Plates, containing seventeen figures, and Forty-one Woodcuts.—*London*, 1859. 8vo, pp. 330.

IN his preface Mr. Cooper states, that although his subject forms part of systematic treatises on the eye, he is not aware of any English work specially devoted to injuries of this organ; and that when in charge of difficult and anxious cases, he has often felt the want of such a book of reference as that which he now endeavours to supply. Those who shall peruse Mr. Cooper's work will readily agree with the author, that to render it interesting and useful no pains have been spared. Believing that cases tend to impress facts upon the memory more strongly than precepts, Mr. Cooper has introduced them freely, although generally in a condensed form, and confining them as much as possible to the illustration of leading points of practice.

The order in which Mr. Cooper takes up the several divisions of his subject is as follows:—1. Foreign Bodies behind the Eyelids. 2. Foreign Bodies in the Eyeball. 3. Gunshot Wounds. 4. Incised and Punctured Wounds. 5. Contusions. 6. Rupture of the Eye-ball. 7. Intra-ocular Hæmorrhage. 8. Burns and Chemical Injuries. 9. Sympathetic Inflammation.

In his first chapter, among other causes of irritation, Mr. Cooper notices the exposure of the eyes to the dust and emanations arising from the preparation of drugs of various sorts.

> Euphorbium (he tells us) is the plant most dreaded by drug-grinders. It causes so great irritation that it is necessary to protect the face and eyes with a mask, having glasses to see through. Not only does the dust cause violent ophthalmia, but, if inhaled, may produce insanity. Cantharides is very injurious to the eyes of workmen, and even the vapour of cantharidin produces powerful effects. The preparing of oil of mustard, infusion of black mustard, powder of hellebore, gamboge, squills, and veratria, is also injurious. The vapours of iodine, bromine, and chlorine, are very irritating; as are the fumes of ammonia and strong acetic acid. (pp. 5–6.)

Mr. Cooper considers minutely the effects of foreign bodies within the conjunctival sinuses, or fixed in the epithelium of the cornea. One of the consequences of such injuries we think he has omitted, and that is, the occasional formation of pterygium from foreign particles remaining fixed for a considerable length of time near the margin of the cornea.

Under the head of Foreign Bodies in the Eyeball, Mr. Cooper mentions a number of interesting cases of wounds of the crystalline, either occurring in his own practice, or recorded by others.

One of these is a case in which a scale of metal struck the eye of a man, who ten days afterwards applied to Dr. Von Gräfe. Close examination discovered a small cicatrice of the cornea, a wound near the centre of the anterior capsule, and, finally, the foreign body in the posterior cortical substance of the lens. Dr. Von Gräfe passed a cataract-needle in the track of the foreign body, and enlarged the opening in the capsule, hoping that with the increase of imbibition the fragment might descend towards the aperture, and so become more accessible. After fifteen days it had moved to the middle of the crystalline, and eight days later into the anterior chamber, enveloped in cortical substance. A puncture was made, and as the knife was withdrawn, the foreign body escaped. Recovery followed, with as good vision as the loss of the crystalline would allow. (pp. 29–30.)

The following case, quoted from M. Desmarres, while it shows the utility of the ophthalmoscope in the examination of cases of minute deep-seated injuries of the eye, excites suspicion as to its alleged ultimate result—namely, the preservation of good sight with a wounded lens.

A mechanic was struck by a chip of metal, which penetrated the crystalline near its border. Inflammation came on, not very intense, but which could not be subdued. The ophthalmoscope revealed in the lens, in a line with the wound in the cornea and iris, which remained visible, a black mass, of the size of an ordinary pin's head, and surrounded by a haze evidently due, it was thought, to commencing traumatic opacity of the crystalline. This state continued four weeks; then the inflammation disappeared, the haze diminished in extent, and there remained little else than the black mass enveloped in a dull spot about double its size; the sight was good, and continued so when the patient was seen three months later by M. Desmarres. (p. 30.)

We certainly have never seen the sight preserved entire in any case where the crystalline capsule was penetrated. Cataract has been the never-failing result of such an injury.

Our author remarks, that if a chip of metal or similar body lodge in the vitreous humour, without wounding the lens or its capsule, it will readily be discovered by the aid of the ophthalmoscope, unless buried in such a position as not to admit of its being brought into view. In illustration he quotes the following case from Dr. E. Jäger:

A workman, engraving steel, was struck by a chip, which, passing through the cornea and iris, lodged in the vitreous humour. Without suspecting the gravity of his wound, he consulted Dr. Jäger at the end of ten days for a slight affection of his sight. There was only a very small trace of a wound in the cornea and iris. On examining the transparent media, a foreign body was seen enveloped in plastic exudation; as a consequence of inflammatory action, the fragment of steel became encysted at the end of a week, and the vitreous humour recovered transparency, but the sight gradually declined. Five weeks after the accident, separation of the retina was discerned in the neighbourhood of the cyst. The separation soon extended over a third of the inferior and external portion of the retina, whilst the encysted fragment had moved from its first position, and was gravitating towards the middle of the eye. This displacement was attended with a slight pricking in the external parts of the eye. A plastic deposit then formed, raising the retina and hyaloid in the form of a cone, at the summit of which was the encysted body. In three months the fragment had reached the centre of the globe. At first horizontal, it had now become vertical. The eye retained its form, the lens its transparency, and there was some amount of oblique vision. (p. 42.)

Since the publication of Mr. Cooper's work, a case by Mr. Dixon, illustrative of the same sort of injury, has appeared in the 'Ophthalmic Hospital Reports' for January, 1859. The result, however, was favourable; as four weeks after the accident, the chip of iron was extracted from the eye, leaving the lens untouched and the retina sound. Regarding such cases, Mr. Dixon observes that opaque bodies in the lens or vitreous humour assume very deceptive appearances as to their real position. The foreign body, in the case now referred to, which was really behind the lens, seemed, when viewed upon the illuminated ophthalmoscopic field, to be in front of the lens, and on the plane of the iris. When examined by means of daylight, concentrated through a convex glass, its true position was at once recognised, as it swung to and fro on a level with the equator of the eyeball.

Demonstrative of the remarkable degree in which the eyeball rolls upwards, when the eyelids instinctively close against the intrusion of a foreign body, Mr. Cooper narrates the following case:

While a policeman was cutting a piece of wood with a penknife, his head being bent forward, the knife slipped, flew up, and passed through the upper lid into the eyeball. The lid presented a clean incised wound nearly in its centre a full quarter of an inch above its margin, while the wound in the eyeball was considerably *below* the cornea, and somewhat to its nasal side. Sight seemed extinguished. There was a free discharge of vitreous humour through the aperture in the sclerotica, the eye was injected with blood, and the patient complained much of pain. Vitreous humour continued oozing for three days, then gradually ceased, and the wound united; but three weeks elapsed before the cicatrice was firm. The lens was uninjured. It was three weeks before sight began to return, and then very gradually. The treatment was simple, and mercury not used. At the patient's discharge, twenty-five days after the accident, the sight of the injured eye was sufficient for discerning large objects. He continued an out-patient for three weeks longer, by which time the sight of the injured eye was nearly equal to that of the other. The wound in the sclerotic had cicatrized with a dense tissue, apparently as firm as the surrounding membrane. (p. 116.)

In the following directions for the treatment of wounds of the cornea, with prolapsus of the iris, Mr. Cooper seems to us rather too eager to snip off the protruding membrane, and scarcely to appreciate sufficiently the use of belladonna or atropine in such cases:

> "If the patient be seen soon after the injury, and prolapse of the iris has taken place, we should direct his face to the light and close and open the lids several times, allowing a pause between each; the sudden exposure to light powerfully stimulates the contractility of the pupil, and thus the iris may be drawn back; this failing, we may with the utmost gentleness endeavour to replace the protruded portion with the rounded extremity of a probe; but if the aperture be small, and the portion of iris tightly girded like a strangulated hernia, such attempts will seldom succeed, and it is better to snip off the protruded portion with scissors. This may be done without much difficulty when the wound is quite recent, but after the lapse of some hours the eye will become irritable, intolerant of light, and unable to bear the necessary exposure; the iris, too, will then more easily resent any pressure. According to my experience, prolapses are very seldom overcome by belladonna, and I believe that the simplest, and on the whole the best, mode of proceeding is to bring the patient under chloroform, and to remove with scissors the extruded portion; the wound will then unite, and a tedious confinement and much neuralgic suffering be averted." (p. 113.)
>
> "I am quite sure that it is not safe to use too great endeavours to return a prolapsed iris into the eye; it may be pushed back, but again and again will it protrude, and the unavoidable bruising with a probe will be very likely to excite iritis and all its attendant evils; it is far safer in such a case to snip off the prolapse as close to the wound as possible." (p. 114)

We believe the plan to follow in such cases is, first, to act on both irides by dropping the solution of two grains of sulphate of atropine to the ounce of water into each eye, and painting the eyelids and eyebrows with moistened extract of belladonna; secondly, to bring the patient under the influence of chloroform, which at once favours the dilatation of the pupil, enables us to examine the eye composedly, and prevents the patient from opposing by any motion of his eye or head the manipulations necessary for returning the prolapsus; thirdly, by gentle continued friction of the eye with the upper eyelid, to endeavour to return the protruding portion of iris; fourthly, should this fail, by cautious pressure with the blunt end of a small probe to replace the iris, the probe serving to displace the aqueous humour, which swells out the prolapsus like a little bag, but which, on being emptied in this way, will often shrink into its natural place; fifthly, if the wound of the cornea is very small, so that the prolapsed bit of iris is firmly girt, and the small probe cannot enter, to enlarge the wound with the iris-knife, and repeat the attempts at reduction; sixthly, should none of these means succeed, to puncture the prolapsus, so as to allow the aqueous humour within it to escape, when it will fall into a flaccid state, and may in general be replaced; lastly, even this means failing, Mr. Cooper's advice may be followed, and the prolapsed bit of iris be cut off.

The tendency noticed by Mr. Cooper for the iris, once pushed into its place, to protrude again, is much diminished if the membrane has been brought fully under the influence of atropine.

Snipping off the prolapsed portion causes great deformity of the pupil, and ought not to be had recourse to unless all attempts at replacement have been unsuccessful.

Wounds of the crystalline are carefully considered by our author. We shall quote two of the cases related under this head:

> "F. M., aged eight, was brought to St. Mary's, July 19th, 1858. On the previous day he was looking into the muzzle of a toy-gun, which he had charged with a piece of wood having a needle stuck in it, when the gun accidentally went off, and the needle entered the right cornea near its centre, where it remained until pulled out. When I saw the eye there was a general sclerotic blush, and the iris, naturally grey, had a greenish tinge; a hazy point on the cornea indicated the seat of the wound, and corresponding therewith was a hazy point in the capsule of the crystalline lens, close to the margin of the contracted pupil. Simple treatment was adopted, and on July 26th the eye was quite free from inflammation, the iris of a natural

colour, but adherent to an opaque spot in the capsule; the lens was perfectly clear, and I thought the case would be one of those exceptional instances in which the lens escapes opacity, but I was mistaken; after the lapse of a month the sight became impaired, and an unmistakeable grey film occupied the pupil. This I watched, and saw it gradually increase in opacity, but very slowly, so that three months elapsed before traumatic cataract was fully developed; the opaque spot in the capsule remained unaltered; the pupil was disengaged by atropine." (p. 118.)

"T. W., aged eight years and a half, was brought to St. Mary's July 31st, 1858. A fortnight previously he was looking through a keyhole, when a boy on the other side thrust a pin through and wounded his right eye. There was much pain, and the eye was poulticed with a mess in which bruised snails formed an ingredient. The cornea now presented a wound near its centre, still open, and surrounded by a considerable haze; the iris, naturally hazel, was dark reddish brown, and in contact with the cornea; the capsule of the lens was opaque, and the pupil, reduced in size, and of a narrow crescentic form, was adherent to it. There was much venous congestion of the conjunctiva and sclerotica, and a purple zone surrounded the cornea.

"The child was feeble, and not in a condition to bear powerful treatment; two leeches were applied, and grey powder, with sesquioxide of iron, administered twice daily: the eye to be frequently fomented with a belladonna lotion, and the brow to be rubbed with extract of belladonna and opium. At the expiration of a week great amendment was visible; the vascularity had diminished, and the iris had to a considerable extent recovered its natural hue; the mercurial was after a time suspended, and quinine with iron substituted; the eye gradually lost the inflammatory condition, but the pupil remained closed." (p. 119.)

In all cases of wounds of the eye, and especially in wounds through the cornea, such as those now quoted, the prognosis should be extremely guarded. Much depends on the force with which the instrument of injury has been propelled against the eye, much on its size and condition, whether sharp and polished or angular and rough, and much on the constitution and vulnerability of the patient.

One patient meets with a wound of the crystalline through the cornea, the opaque disorganized lens is extracted through a puncture of the cornea, in Mr. Gibson's method, an operation which has lately received the absurd name of *linear extraction;* no bad symptoms follow, the eye is, on the contrary, immediately relieved from the severe pain arising from the pressure of the lens against the iris, and as good vision is speedily restored as after the most successful operation for cataract.

Another patient meets with almost identically the same injury, the same operation is performed, but no relief to pain follows; the cornea next day appears hazy and flaccid, and speedily there set in all the symptoms of a violent ophthalmitis, under which the eyeball is pushed from the orbit, suppurates, bursts, and ends in atrophy.

"When the system," observes our author, "is in a bad state, a trifling wound of the eye will bring on violent general inflammation and suppuration of the globe; I have principally observed this in persons who had led irregular lives, and whose powers were low; soon after the injury violent pain attacks the eye, shooting back to the brain; the lids rapidly swell and assume a crimson hue; the eyeball itself enlarges, girded by the lids; and in a short time all natural appearance of the organ is lost, the cornea becoming first yellowish, then brown, the conjunctiva enormously chemosed, and more or less dry. . . . . There is little or no pus till the eye gives way, and then the pus flows from a small aperture, which gradually enlarges as the slough separates. The relief from pain is marked when the eye has given way." (p. 135.)

Mr. Cooper observes that he had "never known an instance in which a lens, rendered opaque from infiltration of the aqueous humour, has become clear." (p. 122.) He then quotes the following case from the 'Gazette des Hôpitaux,' in which such an event is presumed to have taken place:

"A countryman, whilst gathering chesnuts, was wounded in the left eye by one of the prickles of the outer husk. This sharp body had traversed the cornea, and implanted itself in the crystalline. Some days elapsed before M. Robert saw the case, and when he did so, the crystalline apparatus presented a uniform milky whiteness, of which it was impossible to determine the precise seat. M. Robert extracted the spine entire through an incision in the cornea, bled the man largely, and covered the eye with cold water dressings. The following day the opacity had greatly diminished, and forty-eight hours after had completely disappeared." (p. 122.)

It does not appear whether the following remark on this case belongs to M. Robert or to Mr. Cooper:—"The whiteness spoken of must have been either in the lens or the capsule, and in either case its rapid disappearance is marvellous." There can be little, if any doubt, however, that neither the lens nor its capsule had been wounded, and that the cause of the opacity must have been lymph deposited in the aqueous humour.

Contrary to the opinion of some authors, Mr. Cooper shows that danger attends small wounds of the retina—such as that which is sometimes from carelessness or ignorance made with the cataract-needle. He says:

> "I have seen cases where the operation of solution was performed by puncture through the sclerotic, made rather far back to avoid the ciliary processes, in which no inflammation followed, absorption progressed, and the pupil became clear; but about six months after the first operation, and one month after the disappearance of the cataract, muscæ volitantes and scintillations appeared, the sight began to deteriorate, and, despite of every treatment, permanent amaurosis from chronic retinal inflammation resulted." (p. 134.)

A very large proportion of Mr. Cooper's work is made up of cases, either original or selected from other authors. Some of the latter are really so astonishing as severely to tax our power of belief. The following is an example:

> "The eye of an infant was wounded by two fragments of glass, of which one penetrated through the sclerotic and the other membranes to the bottom of the eye. A great quantity of vitreous humour escaped, and the anterior chamber was half filled with blood. The pieces of glass were extracted, the lids closed, and ice applied. Antiphlogistic treatment, and the position of the child on its back, favoured the rapid healing of this wound, which was happily followed by no imperfection of sight. (p. 141.)

Our author occupies fourteen pages with the treatment, general and local, to be followed after the removal of foreign bodies from the interior of the eye, and after wounds of the globe. We shall extract a few passages from this important portion of the work, as both affording a fair specimen of Mr. Cooper's style, and of the judicious practice which he recommends.

> "It may be laid down as an axiom, that if the eye receives an injury, the speedy recovery will depend far more on the state of the system of the patient than on the extent of the wound. A mere scratch will light up a flame that will destroy one eye, whilst another eye will bear with impunity the most severe laceration or incision. . . . . . .
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> "One patient will require support and stimulants to urge the sluggish powers to the reparation of the injury; whilst another will need the most rigid discipline and active depletion to keep the inflammatory action within reasonable bounds." (pp. 144–145.)
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> "It appears to me, that the undoubted value of mercury as a remedial agent in iritis and some other inflammations of the eye, has led to an exaggerated idea of its necessity in cases of injury of that organ. It is too much the custom among young practitioners to begin with calomel and opium as soon as they undertake the management of a wound of the eye—a simple cut for instance—and their reason is doubtless a not very defined notion, that as mercury cures iritis, so it ought to be given to counteract the effects of an injury; but a little consideration would convince them that their conclusion is premature. In a healthy subject a simple wound of the eye will heal with so little redness, pain, or disturbance of the organ, as not to deserve the name of inflammation; there is merely such an amount of vascular action as is necessary for the carrying out the process of union. . . . . . What are extraction of cataract, and artificial pupil operations, but severe wounds? Yet we never give mercury to enable these to heal! And the speedy and happy recoveries which take place under simple treatment should lead a reflecting mind to hesitate before deciding on a mercurial course for an injury, less severe, probably, than either of the operations referred to." (p. 146.)
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> "It was formerly the practice to deplete largely, and to confine to the most limited liquid diet, old persons who had undergone operations on the eye, or who were suffering from wounds of that organ; the phantom of inflammation seems to have been ever present before our predecessors. This much is certain, that the opposite plan of treatment is generally

adopted at the present day with the happiest results; and of those cases which take an unfavourable turn, for one patient who is attacked with acute inflammation after extraction, six or more suffer from non-union of the section from deficiency of power." (p.148.)

"The spirit-drinking, tobacco-smoking, ill-nourished artisan, who so frequently falls under our notice in this metropolis, is a bad subject for any injury of the eye; but it is less frequently acute inflammation which attacks him, than a low but scarcely less destructive form; there is great tendency in the cornea to take on suppurative action after wounds in such people, and the management requires much nicety; depletion they will not bear, and local irritants do more harm than good; it is very important to correct the secretions in the first instance, the tongue being generally foul and liver deranged." (p. 149.)

"If there be one thing more than another calculated to light up the flame of deep-seated inflammation, it is daily opening and examining the eye. I cannot too often or too strongly deprecate such meddlesome proceedings. . . . Another thing which often leads to mischief is carelessness or reckless exposure on the part of the patient; feeling no particular pain in the eye he presumes too much—reads, exposes himself to light and to cold draughts; a relapse is the consequence, and a heavy penalty is paid for the neglect. (p. 150.)

"If a delicate child be the subject of injury to the eye (and it is among children that serious wounds from forks and other pointed instruments are most frequent), the tendency to a strumous diathesis must be steadily borne in mind. The lowering, starving, and depleting system, would here entirely defeat its object." (p. 155.)

Mr. Cooper commences the consideration of Contusions and their effects with some remarks on ecchymosis under the conjunctiva and into the areolar tissue of the eyelids, a symptom which, he says, accompanies concussion of the brain. Of this we were not exactly aware. He says nothing of ecchymosis of the eyelids as symptomatic of counter-fractures of the orbit, an accident with which concussion of the brain, indeed, may be conjoined. After a fall or blow on the head, should extravasation of blood appear in the upper eyelid, without its having received any contusion, we are told that a counter-fracture of the upper wall of the orbit may be suspected; if in the lower eyelid, that the floor of the orbit is broken.

"Dans les percussions de la voûte du crâne sans contusion directe des paupières," say MM. Laugier and Richelot, "l'ecchymose de l'une ou l'autre est tellement caractéristique aux yeux des chirurgiens expérimentées, qu'elle suffit pour faire admettre sans autre signe, une fracture par contrecoup des parois de l'orbite."

It is stated that it is a character of such symptomatic ecchymoses that they increase during several days, and that they are not necessarily nor even commonly attended by any notable swelling of the eyelids; whereas direct contusions of these last are from the first accompanied by swelling and sanguineous extravasation. The symptomatic ecchymosis reaches the lids gradually, discolouring them more and more; while that which arises from direct contusion spreads, on the contrary, from the lids to the neighbouring parts.

The fact that extravasation of blood into the interior of the eyeball sometimes attends that which is external, and, unless a very careful examination of the case be made from the beginning, may not be discovered till the conjunctiva and eyelids begin to resume their natural colour, and the patient finds the sight of the injured eye seriously impaired or completely lost, seems also to be passed over without mention. In such cases ophthalmoscopic examination of the eye will occasionally reveal the existence of a clot within the sphere of the retina, or the signs of blood effused behind that membrane, or behind the choroid.

From page 159 to page 163, a very interesting series of observations is given, illustrative of the serious effects of apparently slight blows on the eye. Next follows the consideration of separation of the iris from its ciliary attachment in consequence of blows, the separation presenting every degree, from a minute chink to total detachment of the membrane, and being accompanied by a variety of coincident effects, such as hæmorrhage, displacement of the lens, and concussion or even rupture of the retina.

Mydriasis from a blow on the eye is sometimes attended, Mr. Cooper states, by rupture of the pupillary margin of the iris. He gives, in illustration, the case of an officer in whom the iris was reduced to a narrow band, was quite motionless, and presented at its lower portion a serrated appearance from laceration. The case affords a good example of what generally happens under such circumstances—namely, a slow and incomplete recovery. (p. 171.)

From the practice of Mr. St. John Edwards, our author quotes the following case of fracture of the orbit and effusion of blood on the brain, from a blow on the eye:

> "An unfortunate girl received a blow on the left eye, which blackened it; eleven days afterwards she died, and on post-mortem examination from five to six ounces of blood were found in the left arachnoid cavity, partly fluid, partly coagulated; the latter portion contained in its centre a fibrinous clot about the size of a small nut. The fluid portion of the blood was found to extend downwards to the base of the brain. The membranes were deeply stained with blood, as also the substance of the convolutions. The small wing of the sphenoid bone on the left side was found disarticulated and displaced backwards and upwards, exactly in a position to have wounded the middle cerebral artery in the fissure of Sylvius. Mr. Edwards was of opinion that the blow on the eye displaced the bone, which in its turn gave rise to the hæmorrhage by rupturing some vessel, but that it temporarily plugged the vessel, and ultimately failing to do this, rapid extravasation of blood and death ensued." (p. 177.)

The following cases of detachment of the retina, the one in consequence of a blow on the eye and the other of a fall, are interesting:

A countryman, aged twenty-six, received accidentally a violent blow over the right eyebrow from a flail. He fell stunned, and was unconscious for some minutes. He then became sick, and discovered that he had lost the sight of his right eye. A week after the injury the brow still bore marks of the blow, the pupil was dilated and motionless, and the only sight was perception of shadows of objects when the image was thrown on the temporal portion of the retina. The ophthalmoscope revealed detachment of the retina over a space about two lines in diameter, behind which was a coagulum of blood, of a deep reddish brown. Where detached, the retina was slightly opaque, and had a generally congested and unhealthy aspect.

A man, aged forty-five, of robust constitution, in running violently, fell, and received a severe shock. About three weeks afterwards, on accidentally covering the right eye, he perceived to his astonishment that he could not see with the left. Examined about four months after the accident by Dr. Williams, he stated that he had never experienced the slightest pain or uneasiness in the eye. Of objects placed before him, or to his right side, he had not the faintest perception, but recognised, although very imperfectly, large bodies placed towards his left side. The pupil was dilated and immovable.

Examined with the ophthalmoscope, the refracting media appeared perfectly transparent, and the retina presented no abnormal vascularity; but it was easy to recognise that this membrane, to a large extent, and all round the entrance of the optic nerve, was elevated by a liquid, and had a trembling movement during the oscillations of the eye. During these movements those deep folds which the membrane ordinarily forms in dropsy of the retina, were not discernible; the folds were superficial and the undulations quite limited. These phenomena, taken in connexion with the pearly colour which the elevated membrane presented, could be explained only by supposing that the retina was raised by a turbid liquid, similar to what is often observed in pericarditis and other serous inflammations.

Mr. Cooper mentions that he had seen several instances of cysts within the eye, arising without any well-ascertained cause; but three which fell under his notice were clearly traceable to injury.

> "These cysts," he says, "as they ordinarily present themselves, appear to consist in the morbid formation of fluids between the iris and the uvea; but in some cases the seat of origin seems to be rather the ciliary margin than the posterior surface of the iris. In one of the cases, ... the growth took place behind the iris, and gradually pushed its way through the pupil. The irritation caused by these cysts is great, and is mainly the result of their being enclosed within the unyielding tunics of the eye; as they increase and require more space, painful tension is excited." (p. 186.)
>
> "There is a tendency on the part of these cysts to refill if they are merely punctured, and

therefore I prefer lacerating the membrane with a broad needle; when punctured the fluid jets out, and the delicate membrane, which has been kept on the stretch, collapses. If this treatment does not succeed, and the pouch is large, it may be drawn out of the eye with canula-forceps, and a portion snipped off. This will effectually cure it." (p. 191.)

The following remarks are illustrative of a subject which has hitherto scarcely attracted notice—namely, posterior rupture of the eye.

> "Rupture of the eye posteriorly, that is, behind the point of reflection of the conjunctiva, is a rare accident; and as the true character of the injury can only be ascertained by excision of the eye, doubtless cases in which it has taken place have escaped observation in the absence of the performance of that operation. It is the result of the sudden and violent blow inflicted full on the eye, and is attended with the sensation of the globe bursting; the eye fills with blood, and pain of the most distressing character racks the patient for many weeks, not a gleam of light being bearable; for though the injured eye is absolutely blind, the other is exquisitely sensitive to light. The symptoms subside by slow degrees, and atrophy of the eye takes place, varying in degree according to the extent to which the coagulum within the eye is absorbed: for it must be borne in mind that this coagulum does not always disappear. I have met with a case in which the colouring particles were absorbed, but the fibrin remained in a firm mass." (p. 197.)

In illustration, Mr. Cooper gives a case which occurred in the practice of Mr. Bowman. An elderly gentleman was struck full on the left eye by the door of a brougham suddenly thrown open. The agony was intense, but sight was not immediately extinguished, though after a few hours it became so. The anterior chamber was so full of blood that no part of the iris was visible, and the conjunctiva was chemosed. The accident had occurred a month prior to Mr. Cooper's seeing the patient, during the whole of which time his sufferings had been intense. He could not bear a gleam of light. When the right eye was completely covered the left could be opened, but it was absolutely blind. The pupil, of a dull reddish-brown colour, appeared as if enormously dilated, and the iris reduced to a mere strip. There did not appear to be blood in the anterior chamber, but the back of the eye seemed full of it. The conjunctiva and sclerotica were acutely inflamed.

During the operation of removing the eye by Mr. Bowman, it was found that the sclerotica had been ruptured in the posterior part, and that a large coagulum lay partly without and partly within the eye. The vitreous humour had escaped at the time of the accident. The aspect of the anterior chamber had been deceptive, for though it had appeared free from blood, it was found filled with coagulum, behind which lay the iris, with the pupil of natural size, the apparent strip of iris being really the small portion visible beyond the margin of the coagulum. (Ibid.)

The facts of this case, as stated by our author, seem in some measure at variance with one another. The sight was not extinguished for some hours after the accident, and yet the vitreous humour had escaped through a posterior rupture of the eyeball. Either the patient must have been mistaken in supposing that he saw for some hours after the accident, or the rupture of the eyeball must have happened during the operation of extirpation.

The subject of dislocated lens is fully considered by the author. (pp. 200–220.)

When an opaque lens lies in the anterior chamber, it can only be regarded in the light of a foreign body, and ought to be removed under chloroform. In such a case, the moment that the section of the cornea is made, the lens is apt to sink back either entire or divided by the knife, through the pupil into the vitreous humour. In a case related by Mr. Cooper (p. 206), one portion of the lens retreated through the pupil as the other was extracted. The best plan for preventing such an accident, is to begin the operation by passing a curved needle through the sclerotic, and fixing it in the dislocated lens; then, to open the cornea, and with the needle to push the lens out of the eye.

In determining the question which sometimes arises after injuries of the eye, whether the crystalline is in its natural situation in the eye, or in the eye at all, Mr. Cooper has recourse to the catoptrical test; as, if the lens is absent from behind the pupil, neither the inverted nor the deep-erect image is visible, but only the image formed by the cornea. Opportunity is here taken by Mr. Cooper to state, that the catoptrical test, as a means of detecting the existence of cataract, has been superseded by the ophthalmoscope, which certainly is not the fact. In many cases of incipient cataract, the fundus oculi appears under the ophthalmoscope with almost no change of colour, while the haziness of the lens is so slight as to escape detection, especially if a strong and concentrated light is used; but the lighted taper, passed in front of the eye, by showing the changed condition of the the two deep images, instantly reveals the true state of the lens. We regard the catoptrical test as a simple and elegant means of diagnosis, which no one who has studied it with sufficient care, and comprehended its value, will think of abandoning.

The ophthalmoscope, again, is of great use in determining the amount and situation of extravasations of blood in the vitreous chamber, and of injuries of the retina.

The right eye of a farmer having been struck with a hollow wooden pear, thrown in sport, Mr. Dixon discovered with the ophthalmoscope, what he considered to be a rent in the retina, and a considerable coagulum of blood, which lay against that membrane. The case was subsequently examined by Mr. Cooper in consultation with Mr. Dixon.

> "The pupil was slightly dilated and motionless; nothing abnormal visible beyond this; objects were seen by the patient indistinctly, and a black patch obscured the central portion; pica type was read with difficulty, each word requiring to be separately made out.
>
> "The ophthalmoscope showed the seat of the rent in the retina as an opaque, irregular line, nearly in the axis of vision, and there were many small spots around this, evidently the remains of the coagulum of blood which had not been entirely absorbed.
>
> "In our report we were enabled to state with confidence that, though Mr. J. might retain a certain amount of sight in the injured organ, we were of opinion that it would never be restored to its former perfection." (p. 234.)

The patient in this case sought compensation for the injury. He claimed 2000*l*., but the referees awarded 700*l*., which, says Mr. Cooper, was accepted with very ill grace. (p. 318). We think the case should have been watched for a considerable longer space of time before any legal or medical decision was given. It does not seem at all probable that, with a rent of the retina nearly in the axis of vision, the patient could have read pica type or any type. Might not what seemed a rent have been merely a streak of blood?

A blow on the eye is apt to produce effusion of blood between the sclerotic and choroid, between the choroid and retina, or within the vitreous humour. In all these cases vision is likely to be materially injured, if not destroyed.

> "When a vessel," says Mr. Cooper, "has given way between the retina and choroid, there will be seen a deep-red projection in the fundus of the eye, formed by the coagulum over which the retina is stretched. In some instances the retinal vessels are distinctly visible coursing over the surface; after a time the retina may give way, and the blood then comes into contact with the vitreous humour, in which it may remain as a coagulum, or diffuse itself in the form of flakes." (p. 235.)

We cannot deny the possibility of such a rupture of the retina happening as is here mentioned; but we do not recollect any evidence to show that such an event has actually followed any considerable time after the receipt of the injury.

Mr. Cooper remarks that

> "Very serious intra-ocular hæmorrhage may arise after operations on the eye, especially extraction of cataract. It is one of the rarest and most disastrous complications that can present itself, utterly defeating the object of the operation and entailing much suffering on the patient. (p. 245.)
>
> "A sudden and most acute pain darts from the eye back into the brain, and is followed by a sensation of tearing or dragging the eye from the socket. If the vessel gives way during or immediately after extraction, the vitreous humour will escape, and be followed by a flow of blood; but if some resistance is offered by union of the wound, the hyaloid becomes filled with blood, and the vitreous is lost sight of. The agonising pain soon involves the brow and

> side of the head, and the eye-lid is so exquisitely sensitive that it cannot bear the slightest touch. . . . .
>
> "The first burst of pain is followed by faintness and nausea, often amounting to sickness, which nausea may continue many hours. The retching, however, does not prevent the stomach retaining small quantities of sustenance, and is best combated by soda-water, and by effervescing draughts containing dilute hydrocyanic acid; also by swallowing ice, either in lumps or as lemon ice. Cold jelly and cold-beef tea are also grateful.
>
> "When the wound is sufficiently united to offer resistance to the immediate escape of the contents of the eyeball, the hyaloid becomes filled with blood; and when the wound is burst open it gradually protrudes through the corneal section, and then between the lids, as a pouch filled with blood. The retention of this increasing the suffering, it should be snipped off.
>
> "As the eyeball becomes distended with blood, the flap is widely opened, the upper lid thrust forward, and more or less œdema arises, commencing at the inner corner.
>
> "The blood is usually venous, oozing from the eye and trickling down the cheek; there is every reason to believe that it is caused by disease of the choroidal vessels, and their morbid condition prevents their ready contraction, nor probably does the bleeding cease till they feel the influence of the pressure caused by the coagulated blood within the eye." (p. 246)
>
> "The careful examination of four eyes in the museum at Moorfields is conclusive to my mind as to the seat of hæmorrhage. In one case the eye was excised for intra-ocular hæmorrhage after extraction, being the second case in which excision was performed there for that occurrence. Two eyes were excised from one individual; in each Gräfe's operation had been performed for acute glaucoma; in each hæmorrhage took place, and they were removed. In the fourth case a staphylomatous eye was ruptured by injury.
>
> "In all these preparations there is most distinctly visibly the coagulum of blood lying between the choroid and sclerotica, pushing the choroid and retina inwards, or inwards and forwards, according to the magnitude of the clot. In not one did the bleeding take place from the inner surface, but in all clearly from *the external surface of the choroid*, probably from the vasa vorticosa. A precisely similar condition existed in another eye excised for hæmorrhage after extraction, the description of which is in the ninth volume of the 'Transactions' of the Pathological Society. I am therefore of opinion, that intra-ocular hæmorrhage after extraction is due to a diseased condition of the choroidal vessels, that it does not arise from rupture of the central artery of the retina, and that it occurs irrespective of loss of the vitreous humour, though the sudden withdrawal of the support afforded to the weakened vessels by that body may be a powerful predisposing cause." (p. 248.)

Three cases of hæmorrhage after extraction, which occurred in his practice, are related by Mr. Cooper. They are extremely instructive, and we only regret that our space does not allow of our quoting them at large.

Considering the very diseased state of the choroid in glaucoma, whether acute or chronic, we suspect that intra-ocular hæmorrhage must be a not unfrequent attendant on the operation of opening the cornea and snipping out a portion of the iris in cases of that disease. In addition to the instance referred to by Mr. Cooper, in which both eyes of a glaucomatous patient required to be extirpated in consequence of intra-ocular hæmorrhage after iridectomy, we observe it noticed in the number for January, 1859, of the 'Ophthalmic Hospital Reports, p. 299, that—

> "One glaucomatous globe was excised on account of the severe pain which followed the excision of a portion of iris (some vitreous had in this case been lost); portions of the choroid near the entrance of the optic nerve were displaced inwards by blood, which had escaped from the outer choroidal surface, between the sclerotic and the choroid."

We would venture to suggest, if intra-ocular hæmorrhage is a common result of the operation of cutting up the living eye in glaucoma, that instead of assuring the patient that the operation is to restore sight, and adopting a proceeding which causes great suffering—so much so as to require excision of the eye for its relief—it might be better to remove the eye first, and to dissect it afterwards.*

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* That relief to pain, and even preservation of sight, have followed iridectomy in some cases of glaucoma, does not admit of dispute. That excision of a portion of the iris, however, has any share in producing the benefical effects, is not proven: on the contrary, the presumption is, that, as in the following instance, the benefit is owing to the preliminary part of the operation—namely, the opening of the cornea, and evacuation of the aqueous humour:—

"A man, aged fifty, of a lax, bloated appearance, otherwise of sound constitution, came under my care," says a celebrated operator, "about three months ago, on account of inflammation of his right eye, which had troubled him for eight days. I found the eye watering much, intolerant of light, the conjunctiva red and chemosed, the great ring of

The following is an example of artificial ophthalmia coming under Mr. Cooper's observation :

> "An inmate of an orphan school was placed under my care at St. Mary's Hospital, by one of the governors, who took an interest in her forlorn condition. She was suffering apparently from chronic inflammation of the right eye, with slight haziness of the cornea. Week after week elapsed without amendment, notwithstanding a variety of treatment, and suspecting that there must be some cause for this, I took the girl into the hospital, and desired that she should be watched. All I could learn was, that although always complaining of her eye to me, she never at any other time seemed annoyed by it. This convinced me that she in some way irritated the eye, but she was not detected, and after a month she was re-transferred to the out-patients' department, the eye remaining much the same.
>
> "A few days after this, I met the girl a short distance from the hospital, on her way to it to appear before me. I stopped her, and without speaking, drew down the lower lid of the affected eye. The mystery was at once cleared up! A chip of wood had been cunningly placed between the lid and the eye, and was of course always removed when the eye was to be looked at. The chip was quite sodden with mucus, having evidently served this purpose for a long time." (p. 291.)

It is a question of great interest, what are the tissues which, being injured in the one eye, are likely by sympathy to give rise to inflammation in the other. It is thought that sympathetic ophthalmitis "is most apt to be excited if the wound has produced a protrusion of the iris, and such a cicatrix as keeps the remainder of the iris perpetually on the stretch, and that it is especially liable to occur if the retina has been divided or lacerated." (p. 301.) In addition to what is here stated, we believe that it should be mentioned that such injuries as implicate the annulus albidus of the choroid, or in other words, the choroid muscle and the ciliary nerves, are more apt than others to be followed by sympathetic ophthalmitis. The presence, also, of fragments of iron, percussion-caps, and the like, within the injured eye, is a cause which should be particularly noticed, as apt to give rise to disease in the opposite eye.

> "There is this curious fact," continues our author, "that so far as I am aware, sympathetic inflammation is not a consequence of the operations for cataract or artificial pupil, though extensive wounds are thereby inflicted on the cornea and iris, and in many cases the iris is involved in the cicatrix. Again, gun-shot wounds, though generally destructive to the eye, comparatively seldom excite sympathetic inflammation." (*Ibid.*)

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the iris of a dull tarnished hue, the pupil fixed, dilated, and very turbid, and the power of vision so much impaired that the patient discerned only the movement of the hand, but could not count the fingers. He complained of a variety of subjective luminous sensations, and of violent pain in the right brow, temple, and side of the nose. I gave a very unfavourable prognosis, as the disease bore so undeniably the aspect of severe acute glaucoma. The patient was placed in a dark room, an energetic antiphlogistic and derivative treatment adopted, and a powerful opiate given in the evening. The inflammation was thereby considerably reduced, and the pain most completely removed : but the sight was improved only so far that the patient could slowly count the fingers at the distance of some feet, while the field of view was extremely limited. The pupil continued very obscure, depending, as is common in acute glaucoma, on a diffuse muddiness of the aqueous humour, and a deposit over the posterior surface of the cornea.

"In a disease like this, which almost always mocks the influence of treatment, it is an imperative duty to make trial of some new remedy. No doubt we frequently see the symptoms of acute glaucoma subside, and after the use of antiphlogistics and mercurials, and especially after large doses of opium, an improved state of vision ensue: yet the hopes based thereon give way on a more extended experience, inasmuch as either the inflammatory attacks are repeated, leaving each time a more contracted field of view, or a continued loss of sight (with centripetal diminution of the field of view), gradually takes place, without any new attack of inflammation. Sufficient and sad experience of these results determined me to strike into another plan of cure for the above patient. As the amaurosis which arises in the course of acute glaucoma is in a great measure proportionate to the increase of the internal pressure, denoted by hardness of the bulb, anæsthesia of the cornea, paralysis of the iris, &c., although yet deeper changes, probably in the bloodvessels, form the original cause, I determined powerfully to bring into play the means of diminishing pressure. After having with this view employed atropine without effect, I proceeded to paracentesis of the anterior chamber. Immediately after the first evacuation of the aqueous humour, the iris and pupil appeared much clearer, so that proof was furnished how far the diffuse muddiness of the aqueous humour had contributed to the dull appearance of the eye. A corresponding improvement in sight also instantly took place, so that the patient could count the fingers at the distance of seven, in place of four feet. This evidently arose from the removal of the turbid aqueous humour, which had disturbed vision by its absorption and dispersion of the light. The chief effect of the paracentesis consisted, however, not in the immediate, but in the gradual improvement which was observed in the course of the next day, so that the patient was able to make out large type ; this being plainly attributable to some change in the internal circulation of the eye. Some days later, as the aqueous humour again appeared somewhat turbid, the paracentesis was repeated, and this even a third time. Although the patient has now a dilated and nearly motionless pupil, and the iris has changed colour, he congratulates himself on possessing very good sight, so that, with convex glasses suited to his presbyopia, he reads the print No. 3 of Jäger's book, and hesitates only at the most difficult words of No. 1. Besides, the field of view is laterally completely unimpeded—that is, the extent and sharpness of excentric vision is normal, a circumstance which, since I have employed exact and appropriate means of measurement, I have never before observed in any case of glaucoma which had run its course."

Such is the important testimony of Dr. Von Gräfe ('Archiv für Ophthalmologie,' Erster Band, Abtheilung ii. p. 302, Berlin, 1855), in favour, not of any new treatment for glaucoma, as he seems to suppose, but of one long ago advised and found useful in that disease, and for which it is to be hoped that iridectomy will speedily be abandoned.

Mr. Cooper explains, that in the treatment of sympathetic ophthalmitis by operation, we have the choice of two proceedings, the one consisting in excising a portion of the eye originally injured sufficient to allow of the escape of its contents, and the other extirpation of the globe. He confesses that though the former has its advantages, he leans to the latter as the more likely to save the eye threatened with sympathetic dieease (p. 304).

In the operation of extirpation, according to O'Ferrall's method, Mr. Cooper directs the conjunctiva to be divided "at the point of reflection." (p. 310.) It is better, however, to remove as little of the conjunctiva as possible, as the more of it that is left, a better cushion will be formed for the application of an artificial eye. The conjunctiva, therefore, should be divided as close to the cornea as possible; the membrane should then be pressed aside, and the recti divided one after the other.

In all cases in which an artificial eye is to be worn, it is important, no doubt, to apply it early, so as to prevent the eyelids from contracting, and sinking it upon the stump. We should doubt, however, if the time specified by Mr. Cooper for the introduction of an artificial eye after accidental evulsion of the eye, will be sufficient—namely, "about a week." (p. 228.) A month, or a couple of months, will generally be soon enough.

After removal of the tube in O'Ferrall's method, it might be supposed that nothing would be easier than the successful adaptation of an artificial eye.

> "When the eye has been excised in the manner described, the muscles being left, form in the process of healing a solid projecting mass, admirably adapted," says Mr. Cooper, "for supporting an artificial eye. . . . . Not only is the orbit fit to receive it in from four to eight days after the operation, but by raising and supporting the flaccid lids, it affords positive relief." (p. 311.)

As to the successful adapting of an artificial eye after extirpation of the globe, we have found it easier said than done; and we observe that Mr. Gray, a well-known artificial-eye-maker, states that—

> "When the globe has been excised, it is still a desideratum that some artificial support for the artificial eye should be found. Mr. Moon, the late house-surgeon of the Royal London Ophthalmic Hospital, suggested artificial eyes with the edges inverted. Such eyes succeeded in removing the sunken appearance, but in the course of time caused irritation and accumulation of discharge.*

Dieffenbach, it is well known, went the length of transplanting a flap from the temple into the orbit, in order to form a cushion for an artificial eye after extirpation of the globe.

The perusal of Mr. Cooper's work affords abundant proofs of the necessity of more attention being paid to eye-diseases in the education of the general practitioner. He refers to many cases in which injuries of the eye had been wofully mis-treated before coming under his care, or that of some other well-informed surgeon.

Among the vast variety of subjects to which the medical student's attention is directed, there is some danger of the object of all his study, namely, the practice of his profession, and the cure of disease, being well nigh overlooked. The essential thing to teach the student is the business of the medical and surgical practitioner, so that he may be able to treat according to just principles the common cases of injury and disease which will come before him. To this all his education should directly point. The dissecting-room and the hospital are the places where his studies ought chiefly to be carried on, and nowhere, we should say, is knowledge more improving or useful to be obtained than in an extensive and well-conducted eye-hospital, with such a work in the student's hand as the very valuable one of Mr. Cooper, which we have now reviewed. It is an excellent practical book, abounding in most interesting facts, and proving the careful observation and the sound practice of the author, with no nonsense or extravagant pretence, and well worthy of a place in the library of every practitioner. The illustrations—both the coloured lithographic figures and the woodcuts—are extremely good.

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* Ophthalmic Hospital Reports, Jan. 1859, p. 305.

REVIEW V.

Klinik der Leberkrankheiten. Von DR. FRIED. THEOD. FRERICHS, Professer in Breslau. Erster Band. Mit einem Atlas von 12 colorirten Stahlstichtafeln und zahlreichen in den Texte eingedructen Holzschnitten.—*Braunschweig*, 1858. 8vo. pp. 409.

A Clinical Treatise on Diseases of the Liver. By DR. FRIED. THEOD. FRERICHS. Vol. I. Accompanied by an Atlas of 12 coloured engravings on steel, and numerous woodcuts.

THE ancients considered the liver to be the central organ of the vegetative functions. Plato, in his Timæus, styles it a θρέμμα ἄγριον, to indicate its importance in relation to vegetative life, as distinguished from animal or spiritual. Galen regarded the liver as the origin of animal heat; from the liver sprung the veins; in the liver chyle was transformed into blood. The doctrine of yellow and black bile outlived the rest of Galen's Pathology, and even the opposition of Vesalius (1542), based upon his anatomical researches, failed to produce any immediate effect. It was not until Aselli (1622) had discovered the chyliferous vessels, and Pequet (1647) had for the first time demonstrated the thoracic duct, that the ancient opinions on the liver and its functions were modified or abandoned. Bartholin and Glisson (1653 and 1665), maintained that the portal vein and liver had no share in conducting the chyle and assimilating it to blood. This view met with great favour at a time when, through the discovery of Harvey, a new physiology had been introduced. The opposition of Riolan and De Bils only afforded food for the humour of Bartholin, who after having "buried the liver," now doubted the possibility of its "resurrection," and wrote an epitaph, announcing the end of its reign. Swammerdam once more enunciated the ancient doctrine, but with so little effect that Boerhaave remarked: "Dudum in meliori parte Europæ obsolevit hæc sanguificatio nunquam ab eo viscere expectanda." The opinion became prevalent that the function of the liver is limited to the secretion of bile.

Whether it was that Galen was more lucky in his imaginations than Bartholin would admit, or that he was more experimental than the relics of his works, in garbled editions, permitted an inexpert mental age to perceive; it came to pass that the nineteenth century found reason to revive much of the Galenic doctrine on the liver, and Magendie, Tiedemann, and Gmelin showed that a share of the digested contents of the gastro-intestinal canal makes its way to the blood through the portal vein and liver. The liver again came to be regarded as the seat of important changes in the composition of the portal blood (Lehmann). It was perceived that the hydrocarbons are in some way metamorphosed in this gland (Bernard). This discussion, which has for some time been carried on amongst a number of cotemporary physiologists, has not yet become a matter of history. Among some collateral functions of the fœtal liver there appears to be the production of blood corpuscles. The calorific result of the function of the liver has found another defender; and thus it is, that the views of Galen, although modified, have risen into fresh life and significance. The liver is once more the central organ of metamorphoses in the vegetative sphere of the animal economy.

Sensible as appears to us the view which the ancients held of the function of the liver, it did not serve them as the basis of their pathology. In diseases which admit of being easily referred to the liver, anatomically or functionally, the diagnosis of the ancients was as perfect as our own. Inflammation, abscess, obstruction in the liver were recognisable enough to them. But when the many obscure general distempers of the body had to be referred to a *fons et origo*, imagination was allowed free credit, and generally drew upon the liver. Sanguificatio vitiatur hepate vitiato. The responsibility of the liver became unlimited, and plethora, anæmia, cachexia, and dropsy were all and always debited to its account, upon the faith of the Latin flourish. Yellow and black bile, from the position of elementary ingredients of the organism, advanced to an etiological dignity, and either equivocally or at the call of some higher and still less known force, could assume a morbific agency. Through fourteen centuries few doubted the relation of yellow bile to acute diseases, accompanied by a febrile rise of the temperature of the body; and in selecting those diseases which they would attribute, on the other hand, to black bile, writers unconsciously poetic and figurative merely depicted the prospect that met their eyes when they contemplated those dark diseases, which, like convulsions and mental disorders, seem only to remind us of blackness and gloom.

The great advance in anatomy and physiology which signalizes the seventeenth century, had the effect of diverting the attention of physicians from the organ, which hitherto had enjoyed an unmerited share of favour and of blame. The followers of Sylvius, the Iatro-chemists, as well as the Iatro-physicists, were more given to theoretical fabrications based on the weakest foundation of facts, than to observation or experiment. Even the restoration of the practical, truthful, Hippocratic simplicity of clinical experience by Sydenham, failed to advance the pathology of the liver. The age acquiesced in the theory of Franz de la Boë Sylvius, who maintained that the fermentation of the juices formed by the spleen, pancreas, and liver, was of great importance to chlylification and the proper mixture of the blood. But practical medicine paid no attention to affections of the liver.

Meanwhile morbid anatomy had begun in silence to store up materials, from which the pathology of the future might draw supplies for its construction. Benivieni, Vesalius, and Fallopia were the first to give accurate descriptions of biliary calculi, and of the consequences of their presence in the gall-bladder. Vesalius reported a case of bursting of the portal vein in consequence of cirrhosis of the liver; he observed the deleterious influence of spirituous liquors upon the liver, the connexion of intumescence of the spleen with liver diseases, and many similar points which are now well established by accumulated observations.

Glisson's anatomy of the liver contains some valuable observations on tumours of this organ in rhachitis. Abscesses and concretions in it were described in Bartholin's history of anatomy. Baillou, a very practical physician, gave an account of malignant icterus. The comprehensive work of Th. Bonnet (1679) contains a series of post-mortem examinations of persons dead of jaundice, and observations of cases of inflammation, tumour, scirrhus, obstruction, cysts, calculi, &c. This author's discussion of many of his observations is very defective, but some subjects are remarkably well treated. The description of cirrhosis, for example, might have been written in our days.

Bianchi endeavoured to collect the scattered materials of the history of diseases of the liver and bile into a monograph, which, although it contains much crude and ill-digested matter, and was therefore justly subjected to severe criticisms by Morgagni and Haller, nevertheless went through three editions. Its influence upon hepatic pathology, never very great, was soon eclipsed by the clinical works of Boerhaave and Stahl, and by the morbid anatomy of Morgagni.

Boerhaave extended the technical system of observation, which Sydenham had revived, to the disorders of the liver, in which he saw the local origin of the majority of chronic diseases affecting the whole system. The primary cause of those morbid processes he correctly observed to be an aberration of the digestive function, producing a diminution of the quantity of bile secreted. In accordance with this latter supposition he endeavoured to explain dropsy, cachexia, leucophlegmasia, &c., as consequences of a faulty chylification. His pathogenesis had a further resource in his ingenious error as to the portal circulation. The motion of the blood in the portal vessels was to him independent of the propulsion of the heart. By endowing Glisson's capsule with contractile powers, he created a separate heart for the liver—"cum sinus portarum pariter sit cor hepatis, uti cor dictum universo corpori." Notwithstanding the assistance which he believed must be afforded to this propelling apparatus by the pressure of the abdominal muscles, this machinery was particularly liable to impediments and arrests. Here was the nidus of the humor atrabiliarius—here the doorway by which a host of diseases effected their entrance—"vena portæ porta malorum." One perceives, with astonishment, that a sensible physiology of the liver failed to produce the slightest effect upon pathology, which adopting the worst relics of ancient doctrine, superadded the creations of fancy, and thus became enveloped in a tangle, from which the science of our days has not yet been able to extricate it.

When G. E. Stahl maintained "that no less a share of chyle was conducted to the blood by the mesenteric veins and through the liver, than entered by the lacteal vessels," he opened a new road to pathology. The diseases of the portal vein, to which he referred the disorders of all the abdominal organs standing in communication with that vessel, were classified accordingly. His theory was, however, more the result of a process of thought upon the basis of anatomy, than a deduction worked out by physiological experiment. Less fortunate than the great chemical theory of the illustrious philosopher, this hepatic pathology found few professional followers, but the doctrine of abdominal plethora and congestion became deeply rooted in the minds of the laity. It offered an easy and fanciful explanation of all abdominal ailments, and thereby naturally retarded the progress of their true diagnosis.

There can be no doubt that during the entire century commencing with Stahl, medical practice did everything to neutralize the efforts of medical science. The haughty ignorance of routine found favour and culminated in Kaempf's abdominal infarctus and clysmata.

But rescue was now near at hand, for a hero had gathered his followers and irresistible they were. By dissections and clinical labours Morgagni for the first time investigated, in a clear and comprehensive manner, the seats and proximate causes of diseases, and drew the precise outlines of what we now consider to constitute the morbid anatomy of the liver. The changes in the structure of that organ, and their genesis and consequences, so far as they could be investigated with the naked eye, were for the most part clearly established. To these, sixty years later, were added the results of microscopical study. The store of facts recorded in the works of Lieutaud, A. Portal, Matthew Baillie, Carswell, Andral, Cruveilhier, Rokitansky, is the nucleus round which the modern pathology of the liver has consolidated itself.

The minute structure of the liver, as first recognized by Henle and Kiernan, has in some minor details been further analysed by assiduous labourers. But physiological anatomy brings home the conviction with irresistible force, that structure alone cannot teach function—that form is only one of the elements by which we ascertain the properties of matter—that quality, behaviour under metamorphosing influences, and relative quantity—that is, quantity as it exists during a limited and given time—must be equally ascertained before we can ascribe to any substance, or peculiarly-shaped chemical compound, such as a gland or an organism, its place in the household of nature. The result of this conviction is, that the physiology of the liver has in our days been immensely extended by chemical investigation. Plattner's discovery of crystallized bile, Strecker's lucid researches on the composition of that secretion, Bernard's discovery of sugar and of the dextrine-like material from which it is formed, the discovery by various observers of a series of products of the retrograde metamorphosis, such as uric acid, xanthic oxide, sarkine, creatine, of the changes which fibrine and blood corpuscles undergo during their passage through the capillaries of the liver, and of the changes which take place in fats within the liver-cells, are recent additions to our already rich store of hepatic chemistry.

The pathology of the author of the work placed at the head of this article, purports to start from this physiological basis, and to try it by researches at the bed-side and on the dissecting table. This task is very difficult, not only from the want of unity in the basis itself, but also for various other reasons, such as the inaccessible situation of the liver, the fact that its secretion, being discharged into the upper part of the intestinal canal, cannot be procured unmixed from the living body—its relations to intermediate metamorphoses the products of which do not directly appear in the excreta. Experience, in showing us that affections of the liver are often associated with diseases of the digestive canal, of the spleen, and general disorders influencing sanguification and metamorphosis, exhibits us a long series of complications, which warn us to use the greatest caution in the construction of pathological theories.

The work before us offers many questions without attempting to solve them, and others to which the reply is only fragmentary; but many and important points in the clinical history of the liver are treated of in a complete and satisfactory manner. The author not only investigated the anatomical lesions, but also their physiological influences upon the metamorphoses of matters carried on in the gland. He has combined with morbid anatomy the chemical method of investigation, and has, on a limited field, reaped a harvest which augurs well for the cultivation of the large area that offers itself to the zeal and industry of the rising generation of physicians.

The size and weight of the liver in its diseased and healthy state, with the natural modifications due to age and sex, the influence of food upon the quantity of blood contained in it, form the subject of a chapter, which is full of original observations. It is followed by a clinical chapter, which minutely details the manner in which the anomalies of form, and size, and position of the liver may be discovered during life. This chapter is illustrated by numerous diagrammatic woodcuts.

The fourth chapter is devoted to icterus. As the author does not share the opinion of Budd and Bamberger concerning the existence of a form of jaundice from insufficient or deficient secretory activity of the liver, two modes only remain by which, according to him, an accumulation of bile in the blood can take place, namely, an increased absorption of bile from the liver into the blood, and a diminished disintegration of biliary matters taken into the blood. Thus an excessive production of bile, polycholia, may lead to some forms of jaundice. The whole doctrine is well supported by arguments, observations on man, and experiments on animals; the changes which the tissues of the kidneys undergo under the continued influence of icteric blood and urine are well described and illustrated by some excellent engravings in Plate I. of the atlas. The chapter occupies more than a hundred pages. It is succeeded by a review of bilious fevers and epidemic forms of jaundice; this latter symptom our author, conformably to Annesley and Griesinger, derives from polycholia. An appendix on icterus neonatorum leads us on to acute or yellow atrophy of the liver, otherwise termed malignant icterus. We will give an extract of one of the author's cases, No. 15, p. 212, which is highly interesting as illustrative of his method of investigation and treatment.

A married woman, twenty-four years of age, was received in the clinical ward of the hospital Allerheiligen, on January 21st, 1858. She was well formed, in excellent condition, and in the seventh month of pregnancy. She had for some days been suffering from loss of appetite, constipation, headache, and low spirits. On the day on which she entered the hospital a slight icteric coloration of the face supervened. During the night after her reception she repeatedly vomited a dirty greyish fluid, and then became suddenly delirious. The pulse was 80 per minute, respirations 20. The temperature of the skin was not raised. The pupils of the eyes were not enlarged, reacting slowly upon exposure to light. The conjunctiva had a slight yellow tinge, as also the skin of the face and neck, while the abdomen and lower extremities exhibited no sign of discoloration. The abdomen was soft, and contained only a moderate amount of gas; the epigastric and both hypochondriac regions were tender on pressure. On percussion hardly any dulness was discovered in the hepatic region, except at the axillary line, where it was found for about an inch and a half; at the other places the intestinal sound passed directly into the pulmonary. The patient took muriatic acid. In the night between the 21st and 22d the distress of the patient increased; she screamed and threw herself about; the pulse rose to 112, and the breathing became stertorous. On the morning of the 22d she was delivered of a dead fœtus of seven months, which had no signs of jaundice on it. There was profuse uterine hæmorrhage. After the abortion the excitement of the patient became less, and she was quiet for short periods, but in an unconscious state. The pulse and respiration, and the irritability of the pupil, remained unchanged; but the jaundice became more intense. The hepatic dulness disappeared everywhere. The bowels had been confined for the last three days. The urine, which had been drawn by the catheter, was acid, of a reddish-yellow colour, clear, free from albumen, of specific gravity 1018·5. On the addition of nitric acid the discolorations, characteristic of bile pigment, could not be perceived. On standing, the urine threw down a slight deposit, composed of numerous needles, single and in groups, mixed with yellow epithelial cells from the tubules and the bladder. The patient took some tincture of colocynth, and afterwards jalap. During the night between the 22d and 23d she lay in a comatose state; some of the muscles of the neck and upper extremities exhibited tremulous contractions. The uterine hæmorrhage continued.

On the 23d the jaundice had become more intense. Pulse, 108; respiration, 24, stertorous; temperature not raised, skin dry.

From time to time there was vomiting of grey mucus, mixed with blackish-brown flakes. Notwithstanding the purgatives, the bowels remained costive. The urine became darker, continued acid, and exhibited distinctly the reaction indicating bile pigment, but did not contain any biliary acids; its specific gravity had risen to 1024. On standing in the cold it deposited greenish-yellow light flakes, consisting exclusively of globular masses of needles of tyrosine. A drop of urine on evaporation upon an object-glass left a residue, which, when seen under the microscope, consisted almost exclusively of crystals of leucine and tyrosine of a very striking form, tinged with biliary matter. Fig. 4, Table III., represents a view of this residue. A sample of urine just drawn by the catheter was freed of colouring and extractive matter by basic acetate of lead; the excess of lead being removed, the residue was concentrated and left to crystallise. After twenty-four hours the tyrosine had crystallised in brownish and greenish-yellow spherical groups of needles (Fig. 3, Table III.), which after recrystallisation (Fig. 5, Table III.) were found sufficiently large in quantity to serve for several elementary analyses. These were performed by Städeler, and the results, together with the tests for and the behaviour of the substance, left no doubt of its being tyrosine. The liquid from which the tyrosine had crystallised consisted of leucine and a glue-like substance. Urea was searched for in vain. Ammonia was present, but in such small quantity as was altogether insufficient to account for the absence of urea by decomposition. The urine when first passed (by the catheter) was acid, contained traces of uric acid, left 4·9$\frac{0}{0}$ of dry residue and 0·14$\frac{0}{0}$ of ashes, exhibiting no trace of phosphoric acid and of lime.

The patient died on the third day after her reception into the hospital. The post-mortem examination showed, as the principal lesion, extreme atrophy of the liver, with destruction of its tissue and disintegration of the cells. The weight of the liver to that of the body stood in the proportion as 1 to 68, while in healthy females the proportion is 1 : 28. From the history of the case and its anatomical features it was concluded that the liver must within six days have lost more than two pounds in weight.

The recital of this and other cases is succeeded by a full discussion of the symptoms, anatomy, and nature of the disease. The therapeutical chapter offers but little matter for congratulation. If, however, anything is calculated to inspire confidence that the efforts of medicine may at a future time be crowned with success, it is the mode of analysing disease carried out by the author.

Chapter VI. treats of chronic atrophy of the liver, and contains a variety of interesting cases. Chapter VII. gives an account of fatty liver, as found in post-mortem examinations; for our author, like Louis, candidly avows his inability to diagnose this condition during life. This chapter, although containing many original researches, is rather defective in chemical principles, even in some of those of an elementary nature; for instance, no distinction between neutral fats and fatty acids is set forth, nor is their respective occurrence inquired into. Six distinct chemical compounds are all thrown together under the name of "fat," a confusion for which, indeed, a short time ago analytical difficulties offered a fair excuse, and these may perhaps be pleaded in behalf of our author. It will, however, be fairly demanded of future writers to pay great attention to exact chemical definitions of the substances in question, for we now possess tests for the most important distinctions, and these tests are neither troublesome nor costly.

Deposits of pigment in the liver, and the changes which this gland undergoes in consequence of intermittent fever, are described in Chapter VIII. For this inquiry the author had excellent materials afforded him by an epidemic of ague of the worst kind, which prevailed at Breslau and in the surrounding country of Silesia after an inundation of the river Oder in the year 1854. The Chapter on Hyperæmia of the Liver and its consequences concludes the volume.

On the whole this work is a very practical and learned performance, by which the author will no doubt sustain and increase his reputation. We have been highly gratified by its perusal, and derived much information from its pages. We look forward with great interest to the publication of the second volume, which is promised in the course of this summer. Of the Atlas we can speak in the highest terms of praise. The objects are well selected, seem correctly drawn, and the artistic execution of the engravings is unsurpassed.

REVIEW VI.

1. *On the Prevention and Treatment of Mental Disorders.* By GEORGE ROBINSON, M.D., Fellow of the Royal College of Physicians of London, Fellow of the Royal Medical and Chirurgical Society, Joint Lecturer on the Practice of Medicine and Lecturer on Mental Diseases in the Newcastle-on-Tyne College of Medicine, &c.—*London*, 1859. pp. 228.
2. *A Letter to the Right Honourable the Earl of Shaftesbury on the Laws which Regulate Private Lunatic Asylums; with a Comparative View of the Process "De Lunatico Inquirendo" in England and the Law of "Interdiction" in France.* By EDWARD J. SEYMOUR, M.D., F.R.S., late Senior Physician to St. George's Hospital.—*London*, 1859. pp. 59.
3. *What shall we do with our Lunatics?* By ALFRED ECCLES, Fellow of the Royal College of Surgeons.—*London*, 1859. pp. 16.

WE welcome every additional attempt to throw light upon the mysterious wanderings of the human mind, and to remove the prejudices and fears which so interrupt and frustrate the intentions of those who would show us how best to deal with a difficulty which ignorance makes a scourge; and we augur well for the future when we find so many able contributors coming forward to supply the deficiency of past years; for we cannot but be struck with the fact of how rarely the distinguished physicians whom we have been accustomed to regard as authorities in this department of medicine have appeared in the literary arena. It is encouraging to believe that the true explanation of this is, that insanity has come to be regarded more in the light of an ordinary malady amenable to ordinary treatment, and that it is not thought necessary, as it formerly was, to banish from our sight, and, if possible, from our memory, the sufferers from this grievous affliction. We hope to see the unreasonable dread of insane persons still further dissipated; and we believe that nothing is so likely to accomplish this desirable end as the correct information of the public mind upon a subject which, for the sake of all concerned, necessarily requires secrecy as regards individuals, but not mystery as regards the nature and treatment of the malady. It is much to be regretted that this secrecy operates prejudicially with the public upon those who are concerned in the treatment of the insane; and we must admit that it is the more unfair because it is observed entirely in the interest of the patients and their friends, and not of those upon whom it entails obloquy, suspicion, and distrust. We may say that there is an hereditary predisposition, if not universal, at least very general, to regard all insane persons as necessarily out of the pale of society; and it is to be regretted that some psychological writers have rather strengthened this view of the matter, by maintaining that there can be no degrees of insanity, no partial incapacity, no limited responsibility—that, in fact, a person must either be perfectly insane or perfectly sane, and that there cannot exist any intermediate mental condition which partakes of the nature of both these states. Dr. Robinson says:

> "Nothing has so much tended to confuse the study of mental disease, or to impart harshness to the treatment of the insane, as a forgetfulness of the natural constitution and infirmities of the human mind. It has, until recently, been most unjustifiably assumed that a broad and unmistakeable line of demarcation existed between the sane and insane; that the detection of insanity was therefore always a matter of facility and certainty; that the lunatic was, as it were, a creature of another world, cut off by his distemper from all sympathy or kindred associations with the rest of mankind, and that the latter were consequently justified in treating him with silent neglect, if not with actual cruelty. . . . . Is there not a certain natural range of disorder incident to every mind as regards strength, harmony, and extent of development? And do not the eccentricities, the proneness to vice and crime, the indulgence of evil passions, the follies, the vanities, the weaknesses of daily life demonstrate the universality of this inherent tendency to mental disorders? For as the body, in the creative energies of its original conformation, in the ever varying combinations and mutations of the matter of which it is composed, in its diversified and counterbalancing functions, in the very delicacy and completeness of its arrangements, constantly engenders within itself the elements and germs of disease; even so the mind, poised in a still more sensitive balance, equally composite in its nature, and infinitely more exquisite in its sympathies, and above all, vivified and penetrated by the spiritual attributes of immortality, also carries with it in its marvellous excellences and endowments the weakness of elevation and the fragility of beauty. Philosophy and religion, therefore, alike enjoin humility and humanity in our dealings with the insane. The one forces us to admit that no man is at all moments perfectly or equally rational, and the other, in its doctrine of original sin, ever recalls to our memory the preponderating tendency of the worst and weaker parts of our mental and moral nature."

The question asked by Mr. Eccles: 'What shall we do with our Lunatics?' is a most important one. The social position and means of the patient will, in perhaps the majority of cases, determine how and where he shall be treated. It seems to be agreed by all that home is not, as a rule, the place in which the disturbed mind is most likely to regain its lost balance; and that relatives, by reason often of their affectionate anxiety, are not the best persons to exercise that judicious control which is required.

As, then, in the words of Dr. Seymour, "it is quite impossible that the greater number of lunatics can be treated at home," the question arises, where then? The poorest class of patients can only go to the public hospitals and county asylums, where they are most liberally provided and well cared for, so that the inquiry is almost entirely limited to the upper and middle classes. The course which is least at variance with their accustomed habits, is to place them under the immediate care of a medical practitioner in his own family, and this for many cases we are disposed to regard as the best course where the means are adequate to afford suitable remuneration for such an anxious charge. Next to this, Mr. Eccles advocates the practice of placing insane patients in a private house for themselves, or in lodgings, with a proper attendant, rather than incurring the stigma which doubtless to some extent attaches to those who have been in an asylum—the association with other insane persons being, in his opinion, also objectionable and occasionally prejudicial. Dr. Seymour differs with Mr. Eccles upon this point; as a perfectly disinterested observer, being unconnected with asylums, and having for several years acted as Metropolitan Commissioner in Lunacy, his opinion is entitled to considerable weight. His great objection is that in lodgings the patient is necessarily left very much to the mercy of servants, and that under such circumstances there are too often opportunities for improper treatment, which cannot happen without being known in a licensed house. We are disposed to agree with Dr. Seymour and to differ with Mr. Eccles as to the effect of association in the majority of cases of insanity, for it must not be forgotten that a most essential part of the treatment of insanity consists in presenting to the disordered mind a succession of new ideas in the least exciting form, for the purpose of displacing those which have perhaps been gaining prominence through a prolonged period of neglected self-discipline; the order and regular habits of a well-managed asylum are perhaps better calculated to carry out this treatment than the luxurious freedom advocated by Mr. Eccles, which, though doubtless applicable to a certain class of cases of a mild form and where there are abundant means, is wanting in what we believe to be of paramount importance in the management of a disordered mind—viz., efficient control—whilst it is only the wealthy who can meet the necessary expense.

> "The expense," Dr. Seymour says, "of a private dwelling, medical attendance, and personal attendance, beyond a very short period, is so very great, that only a very few families can afford it, and at length they are obliged to have recourse to a licensed house. In every view, then, of fortune, convenience, or necessity, the great majority of persons of moderate fortune, thus afflicted, must have recourse to a licensed house; the great object, then, surely, is to make the licensed houses—the necessary retreat of the larger number of the afflicted—as perfect as possible. Still the feeling fostered by novel-writers (who never, as far as I know, *really* depict a lunatic case)—the feeling for absolute secresy which pervades society, the idea that when there is secresy there is opportunity for injustice—all these operate on the public mind to decry similar institutions."

After deprecating the unreasoning outcry and hostility manifested against "those who undertake the ungrateful duty of passing a large portion of their lives with these examples of suffering humanity," Dr. Seymour goes on to express his opinion that the law as it exists is amply sufficient if the working staff of the Commission is increased. Dr. Robinson thinks,

> "That the present law of lunacy, and the administrative machinery employed under it, greatly need improvement . . . . and that in any future legislation, the utmost care should be taken to approach this difficult subject in a calm, conscientious spirit, and one utterly removed from passion or prejudice; for it needs the utmost power of intellect and clearness of moral perception to reconcile in a law of lunacy the conflicting claims of personal and public interest, to preserve at the same time the liberty of the subject and the security of society, and to ensure the humane and kind treatment of the lunatic without denying to the persons entrusted with his charge the ordinary protection afforded by English law to English subjects."

Mr. Eccles agrees with Dr. Seymour, that the present law, with some small modifications, would be amply sufficient, though he differs with him as to the expediency of any considerable addition to the working staff of the Commission. His views are thus stated:

> "I trust Parliament will not be led by the parade of a few exceptional cases to legislate hastily. The present law of lunacy, consolidated with some small modifications, would be amply sufficient to protect the lunatic if it were rigidly enforced. Its efficiency, or the reverse, is entirely in the hands of the executive. I do not think the *large* increase of the Commission, or harassing patients with constant visits by ever-changing officials and doctors, desirable. I would rather take stringent measures to secure that *none but persons of character and skill should be allowed to take charge of the lunatics*—would make it necessary that the certificate should bear the signature of one public medical officer, deputed to examine such cases, and of one private practitioner, and that within one month the patient should be visited by a Commissioner, who should countersign the certificate or institute a formal inquiry, and should report on the accommodation, treatment, &c."

It appears, then, from the opinions of those not connected with asylums, though practising in lunacy, that there is but little wanting in the way of change of the existing laws, but it is hinted that the working of them is susceptible of some improvement. It certainly does appear, also, that in considering the claims of this most unfortunate class, there is a tendency to overlook the rights of those whose lives and frequently whose fortunes are devoted to their care and treatment; and against this section of our profession there has been an outcry which all the charges substantiated against them do not seem to justify. If the requirements of society necessitate the existence of licensed houses, it would be impolitic on the part of the legislature and of the executive to make the position of the proprietors in any respect more irksome than the nature of their anxious duties renders inevitable; at the same time, society has a right to expect that its afflicted members should not be made to suffer from the interested motives of the persons to whose care they are committed; it is desirable, therefore, that the responsibility of their detention should not rest upon those who have charge of them, but upon the Commissioners in Lunacy, or other properly constituted and independent authority. If the protection of two medical certificates is not sufficient, by all means increase the protection, and give the public every possible guarantee that no person shall be confined as a lunatic who is not of unsound mind, and whose malady does not imperatively call for judicious control and treatment.

It would lead us beyond our limits to go into the question as to what amount of mental infirmity should justify interference with the liberty of the subject, but we should have a care lest our jealousy of any infringement of personal liberty should lead us to sacrifice the welfare of the individual and prejudice the interests of society, by withholding the proper remedial means, whatever they may be. It is agreed on all hands that the successful treatment of insanity is in the direct ratio of its early adoption, and that this exercises a far greater influence on the result than the apparent intensity of the symptoms as they are manifested in the early stages of the malady. If, then, it is clear that the mental manifestations are becoming more and more disordered, and the belief is unquestioned that delays can only diminish the probabilities of cure, we cannot surely be too prompt in our endeavours to rescue a fellow-creature from the most severe affliction to which intelligent beings are liable, nor should we be thrown off our guard by the apparent reasonableness of the patient's conduct and conversation on particular subjects, for it must be remembered that the most notorious of the class called criminal lunatics are those whom society had neglected, if not refused to consider insane, until the world was startled by some frightful act of atrocity which cleared up all doubts as to their mental condition, when, alas! it was too late. Bearing in mind the greatly increased prospect of cure when early treatment is adopted, it is worthy of consideration whether the patient and his family are not less likely to be prejudiced by the imputation of insanity than by the neglect of proper precautions and management in the first stage of the malady. But the refusal to recognise the evidence of insanity is, as Dr. Seymour remarks, "the paramount feeling of mankind, whether in the rich or poor," and in his experience "there is no asseveration too strong, no trick too great, to hide even from the medical adviser that others in the family besides the patient have experienced this calamity." How, then, can we hope to remove a prejudice whose strength lies in the weakness of human nature? It can only be by diffusing more correct knowledge on the subject, that we can hope to dispel the mysterious awe with which the world regards this particular malady in itself; we must show that it is susceptible of amelioration and palliation, and in the majority of cases of as perfect a cure, as any other bodily ailment, if not too long neglected. With reference to the curability of insanity, which is abundantly shown by statistics, Dr. Robinson says:

> "My own experience is wholly in favour of the idea, that in the great majority of cases insanity is directly or indirectly the effect of sources of mental disturbance originating in the will or feelings of the persons affected, and which may therefore properly be considered as moral in their nature. The mental affection itself may not be immediately produced by the moral disturbing cause, but if the latter excite in the system a series of disordered actions terminating in the former, and which would not otherwise have been in operation, we are surely warranted in regarding it as essentially productive of the attack of insanity. Moreover, in proving that the great majority of cases of insanity arise from moral causes, we prove that it is in general a preventible disease. Many physical agencies and bodily infirmities cannot be altogether prevented. But, inasmuch as sound religion and sound philosophy alike enjoin constant warfare against the evil tendencies naturally existing in mankind, we are justified in believing that mental disorders, arising from those moral weaknesses, are by no means necessary and inevitable afflictions. It is no part of man's duty to bow down tamely to miseries of his own creation, and allow the light of his reason, and the best and holiest of his spiritual attributes to be polluted by the offspring of his own vices

and infirmities, and when educated communities can once be made to understand and feel that the preservation of their mental health is to a very great extent in their own hands, we may expect many of the most fertile sources of mental disorder to be speedily and effectually checked."

In one important point Dr. Robinson differs with almost all the recent writers on insanity, including Esquirol, Pritchard, &c., who have maintained the reality of a morbid condition, to which the designation "moral insanity" has been given, and which, in Dr. Pritchard's words, is characterized by "a morbid perversion of the feelings, affections, and active powers, without any illusion or erroneous conviction impressed upon the understanding; it sometimes coexists with an apparently unimpaired state of the intellectual faculties." Dr. Robinson does not deny that the moral sense and feelings in the partially insane are frequently impaired, but argues that there are few in whom the powers of the will, the reason, and the conscience are so far undermined as to render them altogether incapable of self-control, and consequently irresponsible. This is no doubt perfectly true, but the advocates of the doctrine of moral insanity do not contend, as Dr. Robinson seems to think, that this condition, if ever so satisfactorily shown to exist, should be admitted as a bar to *all* punishment. No doubt the doctrine has been carried too far, and Dr. Robinson has some reason for saying that—

> "This assumption of the possible existence of 'irresistible' and 'uncontrollable' impulses in persons admitted to be intellectually sane, is the root of all the difficulties surrounding the subject."

And we agree with him that—

> "The only effective and legitimate mode of reconciling the requirements of social security with the exculpatory recognition of human weakness and man's natural proneness to crime, consists in the mitigation of our criminal laws, and the substitution of the reformatory for the retaliatory principle, as the ground of the punishments inflicted by them."

But Dr. Robinson recognises the fact, that there are large numbers of persons of unaffected intellect in whom the governing power of the mind is so far enfeebled, that they are "habitually incapable of resisting criminal impulses or vicious cravings," and this surely amounts to something very like "irresistible impulse." He is so well aware that this moral weakness is a fertile source of misery, that he believes in these cases the general interests of the community to be more involved than in those of individuals "morbidly" affected. We are inclined to think that Dr. Robinson's unwillingness to regard moral weakness as a *morbid* condition, depends very much upon his fear that it should be confounded with natural depravity and vice, and admitted as a bar to punishment when pleaded in extenuation of crime. We think there are few impulses which are altogether irresistible, if there be a sufficiently powerful motive to exercise control over the actions, and, therefore, few who can properly be said to be irresponsible; but we think few will deny that the responsibility attaching to individuals differs in degree, and that in many it is of a very limited kind by reason of mental weakness, the result of morbid action in these cases. We would on no account contend for immunity, but if punishment is to be inflicted it ought surely to be modified in proportion to the presumed incapacity to resist criminal impulses.

The question as to the increase of insanity is one upon which authorities differ, and upon which it is very difficult to form an opinion. Statistics are quoted both to prove and to disprove the position, and it is maintained with considerable show of reason, that the apparent increase is in a great measure, if not altogether, owing to the fact that more cases are brought under observation, and more attention is paid to them; but making allowance for these, the weight of evidence seems to be in favour of the opinion that there is a certain increase in the number of insane persons beyond what could be referred to the increasing proportion of the population. The chief cause of this is doubtless to be found in the wear and tear of the great battle of life, too often carried on without regard to any consideration but the gratification of an ever restless ambition, which leads men of every rank and degree to sacrifice present happiness in the fierce struggle for wealth and pre-eminence. We cannot but fear that the increasing civilization of the age of which we constantly hear such complacent commendation, is not without serious drawbacks, and that society pays for its advantages by the greater selfishness of its members and by the prevalence of a less lofty tone of morality. Dr. Robinson has expressed his views on this subject as follows:—

> "Notwithstanding, then, the enormous intellectual advances made during the present age, there probably never was a period in the history of this country when happiness and contentment were less generally diffused throughout the different classes of society. The increasing tendency to the concentration of wealth in the hands of a few, necessarily produces discontent among the many; while the incessant striving after social elevation, as one of the results of the more general equalization of education, the intensity of competition, the love of display, so unnatural to the old English character, the introduction of many foreign vices, and the frequent faithlessness of men holding important trusts, have all united to engender a state of feeling in the highest degree injurious to the mental condition of the community. And in investigating the moral causes of insanity, we shall discover ample evidence of its frequent origin in the vices of a spurious and hollow civilization."

We have derived much pleasure from the perusal of Dr. Robinson's treatise, which is evidently the production of a philosophic mind, and we trust it will be extensively read, believing that his views are generally sound, and knowing that his experience has been considerable. The suggestions also of Dr. Seymour and Mr. Eccles are well worthy of consideration, at a time when Parliament needs to be guided by the unprejudiced opinions of experienced men in legislating upon a very difficult subject, under the influence of a sort of panic created by exaggerated statements, and increased by the fears which have their origin in very imperfect knowledge of the subject on the part of the public.

REVIEW VII.

1. *The Oxford Museum.* By HENRY W. ACKLAND, M.D., Regius Professor of Medicine, and JOHN RUSKIN, M.A., Honorary Student of Christchurch.—*London*, 1859.
2. *A Manual of Qualitative Chemical Analysis.* By A. BEAUCHAMP NORTHCOTE, F.C.S., Demonstrator to the Professor of Chemistry at Oxford, late Senior Assistant in the Royal College of Chemistry, London; and ARTHUR H. CHURCH, F.C.S., of Lincoln College, Oxford, late Assistant to Professor Brodie.—*London*, 1858.
3. *A Handbook of Chemical Analysis, adapted to the Unitary Notation.* Based on the Fourth Edition of Dr. H. Will's 'Anleitung zur Chemischen Analyse.' By F. T. CONINGTON, M.A., F.C.S., Fellow of Corpus Christi College, Oxford.—*London*, 1858.
4. *A Letter to the Provost of Oriel; on a Scheme for making Oxford more Accessible to Medical Students generally.* From C. H. PEARSON, M.A., Fellow of Oriel College, and late of St. George's Hospital.—*London*, 1858.
5. *A Letter to the Rector of Exeter; on Some Proposed Changes in the Residence required by the University for Degrees in Medicine.* By GILBERT W. CHILD, M.B., of Exeter College, Physician to the Radcliffe Infirmary.—*Oxford*, 1858.

SIX years have now passed since we called attention to one out of the many reforms which were then being commenced at the Universities of Oxford and Cambridge. The school of Natural Science, which existed only in embryo, has now, in Oxford at least, attained some growth and organization. Evidence of this is appearing, not only esoterically, in the form of attendance on the professorial lectures, in the increasing number of students of the practical branches of physical science, and in the more general diffusion of appreciative respect for these pursuits among the members of the colleges; but fruit is also being borne of which the outer world can form a judgment.

We have much satisfaction in pronouncing a high estimate of the two manuals of Chemical Analysis, both, wholly or in part, emanating from Oxford men, whose titles are prefixed to this article. Both of them appear to possess accuracy and fulness of detail which would do no discredit to any class of observers; while they are superior to most former works of a like character in the very points in which the advantages of Oxford training ought to be most apparent. Even the casual reader can hardly fail to be struck by a greater completeness of plan, a more accurate adjustment of subject, and a more harmonious subordination of less important parts than is usual in works of this nature. The joint authors of the larger work, by their own statement, seem implicitly to suggest this deficiency in former manuals when they "request the attention of chemists, and all those engaged in instructing pupils in Chemical Analysis, to the following features," of which we extract some of the most prominent:

> "1. In the first part of the volume, at the end of each group of elements or salts, concise tables are given, which show at a glance the most striking properties of the more common substances, as well as their most characteristic reactions.
>
> "3. In describing the salts and reactions of the various acid and basic radicals, *the same order is invariably preserved.* The monobasic salts come first, then the bibasic and lastly the tribasic, the basic elements commencing with those most decidedly positive.
>
> "4. If, in treating of any basic or acid radical, a salt of characteristic properties is described, *the corresponding salt of all basic or acid-radicals subsequently spoken of is invariably noticed.*
>
> "5. The most characteristic compounds of each radical are printed in a conspicuous type."

Indeed, the subsequent arrangement of this work fully justifies the hope expressed by the authors, "That they have attained some degree of unity and simplicity, as well as of completeness, in the present treatise."

Not a little of this clearness and unity of design depends on the adoption in both works of the Unitary Notation. It is somewhat remarkable that a system proposed by Gerhardt sixteen years ago, and possessing such undoubted claims to general employment, should as yet have met with only limited acceptance in England. The chemical school of Oxford has already conferred a boon on the general body of students by setting its stamp of approbation on so manifest an advance in chemical nomenclature.

We sincerely hope these may prove to be only the first of a long series of treatises on the various sections of Experimental and Physiological Science hereafter to emanate from Oxford. For, so far from sharing the old opinion of the unfitness of the Universities as places of study for this branch of human learning, we believe them in every respect well adapted for its successful prosecution. The advantages seem to extend in a twofold manner; not only will the tone of thought and the matter of education provided at these seminaries be improved and enlarged by the addition, but all those classes of the community, whose preliminary training is in some way connected with the Universities, and indeed many who are less closely united to them, will be furnished with new outlets of energy, and fresh means of usefulness.

One of the healthiest signs in the history of modern thought is the increasing turn towards a minute and reverent study of nature. It is in part, perhaps, a reaction from the over-great concentration and individualism which were the characteristics of the end of the last century and the beginning of the present. But, whatever be the essential cause, we cannot doubt that greater use of the observing powers, and a disposition to be more occupied by the facts of the world about us, pervades our literature, our art, our theology, and perhaps, we may add, our science; indeed, it would seem that this progress, commencing from the inner recess of experimental investigation, had not only brought out great results for science itself, but had pervaded the tone of general thought; and that at an epoch of unexampled advance in the discovery and explanation of physical phenomena, the mind of society had, as it were, gained a tinge of this observant and watchful spirit; that where formerly it raised theory and framed system, it is now content to wait in rapt attention for the revelation of fact, for the evidence of purpose, and to labour with an earnest faithfulness to elicit germs of new truth by the closer searching of the means at its disposal.

This is the tendency of the better minds; this gave to a work like the 'Cosmos' of Humboldt its enthusiastic welcome even among classes little read in natural history. The same idea is reflected even in the lighter efforts of fancy; the paintings of the "Preraphaelite" school, the works of Ruskin and of Kingsley, abound in portraits of the common things about us, whose principal attraction is a close fidelity and a reverent admiration for the great design. We look upon the revival of physical science in the Universities as the expression of this general yearning. It is the wakening of a body of men unequalled for power and discipline of mind to the value of even the smallest work of nature about them as an object of study. They had long considered it almost beneath their dignity, as thinking, reasoning men, to use merely their eyes and outward senses in observation of the world round. This was at most the business of a few, specially gifted by caprice of organization, with tastes and faculties fitted for the pursuit. But the same age which speculates with wisdom on the real amount of physical process involved in the action of mind; which estimates the weight of brain substance consumed in thought; which can show almost to demonstration that memory is an actual mark and impression, insensible indeed to our present means of investigation, but none the less material for that reason, stamped on the molecules of the nervous centres; this same age has in part overthrown the false distinction of kind formerly drawn between matter and mind by a sort of unstated axiom. It has shown that reason is only the highest function of our material bodies, and that sight, hearing, and touch, are not far below it in dignity. This age has worthily accepted the practical corollaries of its foregone discoveries, and does not scorn to see its highest intellects, tutored with all the preliminary and ancillary instructions so properly set forth in our school and University course, bending themselves to the watching and "questioning of nature," which can never be a mean occupation for any created being.

Dr. Acland not only points out in a most convincing manner the causes of this change in the current of University studies, but also directs attention to the fact of its being more a return to the oldest methods than strictly a new discovery. The passage is so excellent as to demand quotation:

> "The great tide of human thought had set for centuries, and down even to the close of the Middle Ages, chiefly in the direction of speculative reasoning, poetry, or history. Many circumstances in the condition of our globe tended to repress the outbreak of inquiry and eager interest in external nature, which about the time of the discovery of the New World dawned upon all the educated part of mankind. It is not other than both remarkable and humiliating, that some of those who studied and taught the mental science of Aristotle, or the speculative dogmas of the schoolmen, should have wholly forgotten the successful energy which Aristotle and Galen, in the very dawn of literature, had expended in investigating the laws of organic life. It is probable, indeed, that the very condition of the Church in the Middle Ages, which led men to study the Bible less, and value their own fancies more, did, in fact, close their eyes to the astonishing revelation of the unwritten as well as of the written Word of God. Oxford, 'the ancient seat of learning,' was not exempt from this intellectual one-sidedness. It cultivated chiefly classic lore, and pursued the metaphysical notions of the schoolmen; even these were not always taught in the far-seeing spirit of true philosophy. It has taken some centuries from the epoch of Roger Bacon, followed here by Boyle, Harvey, Linacre, and Sydenham, besides nearly two hundred years of unbroken publication of the Royal Society's Transactions, to persuade this great English university to engraft, as a substantive part of the education of her youth, any knowledge of the great material design, of which the Supreme Master-Worker has made us a constituent part."

While we feel deeply the value of the natural and experimental sciences as an addition and superstructure to the general educational course, we cordially agree with the same writer in rejoicing that there has not been a mere substitution of the one for the other. "The addition," he says, in the work before us, "has been made; the substitution is, I hope, averted. The further my observation has extended, the more satisfied am I that no knowledge of things will supply the place of the early study of letters—*litteræ humaniores*." Obvious as is the soundness to all of this proposition, perhaps there is not any branch of professional labour in which it is so strongly exemplified as in our own art of medicine. There is hardly one of us who will not, at some time or other, have been painfully struck by the absence of coherent and dispassionate reasoning on medical evidence exhibited by men well versed in all the facts, and well practised in all the mechanics of their study. If it be true that in former times we had occasionally to accept the reproach against University medical men, that they were deficient in the knowledge of facts, and in the manipulative skill only to be acquired by a kind of labour particularly repugnant to a cultivated mind; it is still more undeniable that we have very often to wonder that energetic work, and earnest thought on the practice of medicine, aided by a store of all the material information needed for the task, should come, before our very eyes, to so little or so false results, when unsupported by balance and sobriety of judgment, by closeness of logic, and pertinacity in holding to the important point. These powers, unless when given exceptionally to a very few minds, seem only to be acquired by patient and persevering devotion to those studies which the consent of mankind has universally conceded to be the "exercitations" and as it were gymnastics of the reasoning powers.

> "I do not doubt," says Dr. Acland on this point, "the value of any honest mental labour. Indeed, since the *material* working of the Creator has been so far displayed to our gaze, it is both dangerous and full of impiety to resist its ennobling influence, even on the ground that His *moral* work is greater. But notwithstanding this, the study of language, of history, and of the thoughts of great men which they exhibit, seems to be almost necessary (as far as learning is necessary at all) for disciplining the heart, for elevating the soul, and for preparing the way for the growth in the young of their personal spiritual life; while, on the other side, the best corrective to pedantry in scholarship, and to conceit in mental philosophy, is the study of the facts and laws exhibited by natural science."

But besides speculative improvements which result from the addition of physical science to the University course, there are some which will commend themselves to the most practical minds. High among these we are disposed to place the fact that it is converting many men who once lounged through their stated time of residence in almost idleness, into earnest workers. We allude to those young men who have some leaning towards the study of natural history. This is a large class at the Universities; many come up from the country schools with a very considerable knowledge of subjects, which only needs a few sanctifying principles to become philosophical; either they are sportsmen, or fishermen, or learned in horses, cattle and sheep; not uncommonly they have a rough knowledge of botany or entomology, of geology or agriculture. Most persons look upon boys as so alike, so deeply marked with the Latin and Greek stamp of public schools, as to have hardly any individuality. But we speak from some observation, in saying that these biases which often become the ruling passions later in life, are sometimes begun very early. It is, perhaps, to be regretted that the cramped and Procrustean nature of the ordinary routine destroys many of these proclivities before they have had time to ripen into pursuits. All members of this class are alike characterised by a habit of acute observation, and by senses more than ordinarily keen; and to them the schools of natural science open a wide field of occupation. By those who have energy there is much distinction to be gained, and the opportunity of serving the cause of science; and those who, from lack of purpose or of ability, do not attain any eminence, will at least be made more happy and more useful members of society. Many of those who attended the physiological lectures at the Christchurch Museum have been young men of standing and of position in their respective districts; it is undeniable that from their studies in that direction much good has followed; some have since given proofs evident to the world of the value of the lessons they then learned, and of the habits of observation they there acquired; and in the cases where no such proof can be given, there is strong ground for conjecture, since no one can have failed to remark what a vast accession it is to the character of a country gentleman, or a clergyman of some remote district, if his tastes rest on a basis of the study of nature. The merest sports of the former—hunting, shooting, or fishing, and, still more, the more serious business of farming, rearing live-stock, and the like, gain a double interest to himself, probably a higher degree of success; the latter may turn his knowledge to use in a quarter where it is deeply needed—he may show himself a judicious sanitary reformer, as far removed from the charlatan theorists who, at the present time, have been unfortunately too much accepted as representatives of that science, as from the stolid obstructives who protect every time-hallowed nuisance against a day of pestilence and death.

The infusion of a certain tincture of physical science into the Universities shows signs of progress in another direction, from which we hope much. This is what may be described as the border-ground between physics and metaphysics and psychology. There is between these a large space, as yet hardly travelled over. It is true that the world has seen many futile attempts to found systems of physics on a more or less unstable basis of metaphysical speculation. But the opposite course is at once the more valuable and the less traversed. It has long been our hope to see the production of works on mental science by physiologists, and on metaphysics by physicians, for we feel sure that such labours may establish the most valuable results. Indeed, where the attempt has been made, even in a partial manner, it has not been without signal success. Sir B. Brodie's psychological inquiries are a typical instance worthy of imitation. On some of the questions involved there is deep interest of a legal and political character, such as the influence of temperament, imagination, excitement, fanaticism, or disease, on moral and criminal responsibility. Education would receive a valuable contribution in an experimental inquiry upon the physiological problems involved in school discipline. In this department there is much dangerous error abroad, much injudicious treatment sanctioned, which only need a clear exposure and temperate suggestions for their reformation. On many other points—memory, instinct, sleep, and the physical basis of mathematical conceptions—we feel assured that there is much to be yet discovered, to which the surest road lies in a well-digested course of physiological inquiry, carried some steps higher into the regions of first principles than is usually deemed the duty of less transcendental physiologists. It is not unreasonable to expect that such works may emanate from the physical students of the Universities.

Evident as are the advantages which are accruing to Oxford and Cambridge from the addition of natural science to their studies, the consequent gain of the various professions which depend more or less closely upon physics, and specially that of medicine, is even more conspicuous. The main social cause of this was clearly stated in the former article above referred to. It was there enunciated as a fact that "the raising of the medical profession to its due rank depends mainly on the scientific education of the upper classes throughout the country." Subsequent events have only confirmed this view, and although the lapse of time has rendered some modification of the prospects then suggested very necessary, still much has been done, and more is in progress of accomplishment. It is true that the proposed reduction of collegiate expenses, and the establishment of "Affiliated Halls," has not been favourably entertained, neither have we as yet reached what was there looked forward to—"a golden age, when our attorneys, country surgeons, civil engineers, &c., should receive the benefits of the very highest form of education, conjoined with judicious discipline." But the Oxford Museum there spoken of as a vague possibility is now an existent reality. And it is a creation which for grandeur of conception and perfectness of execution is well worthy of the age and the University which have established it. We shall have to speak farther on of its artistic excellences, and of the remarkable manner in which the pliability of a Gothic style has been moulded to requirements entirely modern. Here we are most concerned with the wideness of its design and the completeness of its provisions.

"A few words," says Dr. Acland, "will explain the principles which determined the kind of accommodation. For the illustration of nature the student requires four things,—first, the work-room, where he may practically see and work for himself; secondly, the lecture-room, where he may see and be taught that which by himself he can neither see nor learn; and as an adjunct to these, a room for more private study for each; thirdly, general space for the common display of any illustrative specimens capable of preservation, so placed in relation to the rest of the building as to be convenient for reference and comparison between all the different branches; and lastly, a library, in which whatever has been done or is now doing in the science of this and other periods and countries may be conveniently ascertained.

"The centre of the edifice which is to contain the collections consists of a quadrangle. This large area will be covered by a glass roof supported on cast-iron columns. The central court is surrounded by an open arcade of two stories. This arcade furnishes ready means of communication between the several departments and their collections in the area. The roof springs from above the upper arcade, so that the arcades on both floors are open to the covered court. The arcade on the ground floor is entered from the centre of each side of the court, and ready communication is made from it to every part of the collection.

"Round the arcade is ranged upon three sides the main block of the building. The east is wisely left unencumbered by rooms, to afford ready means for future extension. Beyond, or outside the main block, to the north, because the coolest side, are an open yard for the anatomical and zoological departments, and beyond it dissecting rooms. On the south side are the rooms which require special arrangement for experiments or light, a yard for purposes connected with chemistry and experimental physics, and further still, out-buildings, containing workshops, furnace-rooms, weighing-rooms, and laboratories. Thus all noxious operations are removed from the principal pile, but joined with much convenience to the lecture-rooms, and communicating easily with the central court, common to all the departments.

"The laboratory for the chemical students is the large detached building seen at the south-west angle of the museum.

"On the upper floor are a large lecture-room for 600 persons, intended for occasional use, the entomological collections of Mr. Hope, and along the front, the library and reading-rooms, together 200 feet in length."

The writer then proceeds to explain that it is proposed to transfer the collection of works on medicine from the Radcliffe building to the museum, and to appropriate that fine edifice to the purposes of a reading-room for the Bodleian Library, which is urgently required.

The detail in which we have given these arrangements implies our high opinion of their merits. Indeed, it is beyond all contradiction that the museum just in process of completion presents the most perfect specimen of such a structure that modern ingenuity has devised. In actual size it is unequalled in England; and when the unity of plan and disposition of parts is considered, we may safely challenge Europe to produce its peer. It will be deemed a strong statement that the mere building of such a structure is a boon to science, and a proportionate advance in the social standing of all who are engaged in scientific professions; and yet there is perhaps warrant for the assertion. Even if we could suppose it never to be worthily used, it stands a monument to the dignity of science—an evidence of the value which one of the oldest and noblest Universities in Europe set upon its pursuit—and a tangible proof of the reality of the labours which, proceeding silently in the laboratory and dissecting-room, bring forth results to move nations, and influence the destiny of races. It is the noblest memorial of those giants among men, to whom was given in their generation a deeper insight into the hidden things of nature, the worthiest mausoleum of those whose bodily presence is gone from among us, but whose discoveries live and work only the more widely and energetically as time rolls onward.

Viewing the whole subject, more specially in relation to our own study of medicine, there is much cause for satisfaction. For the recent changes at the Universities are one evidence out of many that it is beginning, as a profession, to occupy the minds not only of the authorities, but also of that class who, on finishing their educational career, have to choose a path in life. It cannot be denied that such a tendency was to be expected, both from the increasing estimation of scientific pursuits and also from that cyclical progress which seems to be a characteristic of mind as of matter. A few years have comprised two sections of the last revolution—the first, in the overwhelming tide of students who, some fifteen years back, hastened to devote their energies to Theology, and its representative, the clerical profession; the second, dating perhaps some eight or ten years back, in the increased study of law and scientific politics, and in the crowd of men adorned with all scholastic successes, who have filled to overflowing the various Inns of Court. There remains but one of the highest professions untried, and we have little doubt that the next decennium will contribute its share to the cycle by swelling the ranks of the medical profession in a proportion far above the previous averages. Hitherto we are bound to confess that the increase, though appreciable, is not very striking;[*] but it would probably have begun before now had not a diversion arisen; had not the moving masses been drawn out of their natural orbits by their heterogeneous attraction. This has been supplied by the sudden throwing open to competition of so much government patronage. We consider that the public offices, the army and the Indian service, are drawing away many who would, in an unchanged state of things, have chosen the medical profession. This view gains some corroboration from the fact that the engineering and other similar departments have given a value to physical knowledge, and to manipulative skill, which were formerly only represented among the highest professions by our own.

It is from considerations of this nature, and from a general sense of inexpediency, that we oppose ourselves to such plans as that proposed by Mr. Pearson, of Oriel, in the pamphlet whose title is prefixed to the present remarks. It was with some satisfaction that we heard it had been negatived by the authorities. The suggestion of any means of rendering more numerous the body of medical graduates at Oxford was so far undeniably good; but the contrivance which he advanced for attaining this end seemed open to much objection. Not only was the reduction of residence almost a confession of social inferiority in those who would present themselves, but the scheme had several other dangers, which Dr. Child well enumerates in his letter to the Rector of Exeter. Dr. Child shows that it would be difficult hereafter to exclude the other faculties of Divinity and Law from the advantages of a measure which, whatever it might appear anteriorly, must in time get to be regarded as a causeless indulgence. Or if this should not happen, a still greater danger would lie in the possibility of a separate *camaraderie* of medical students springing up, not similar in scholastic standing with their contemporaries in the other faculties, and so necessarily isolated from that equality, tempered by competition, which forms so valuable a part of college discipline. Far better than these well-meant attempts to lower the standard of University and professional education to the social level of those who are now excluded, is any method by which some may be attracted into our society, who, under existing circumstances, are inclined to look down upon it. Much will be done towards this end by the daily increasing number of rewards offered for proficiency in science, and much also by the incentives of ambition furnished in the scientific fellowships and professorial chairs; but by far the foremost is the Museum itself; for with a knowledge of the great works of nature comes to the best minds a reverence for them—a longing after a deeper insight, and in our own more special subject an awe-struck consciousness of the physical dangers among which we all thread our way; with a proportionate earnestness in ministering our part of relief and assistance to the countless forms of disease and corruption which are allowed so mysteriously to prey upon our corporeal organization.

There remains one branch of the subject which is by no means the least important. It is represented by the letters of Mr. Ruskin, published with the Lecture of Dr. Acland. It may be generally stated, as the importance of a proper estimate of the dignity of our profession, of its close and intimate relations on the one hand with abstract science, and on the other with the refinements of art. This is a principle which lies at the foundation of all progress in our social and political standing. For it is beyond contradiction that one main reason of our present want of influence lies in the inadequate conception which many practitioners form of their duties and obligations. They view medicine too much as an art, too little as a science; perhaps, also, they allow themselves to regard it as a reputable means of making money, and to forget that this necessary adjunct is its very lowest and most tradesmanlike side. Οὐ γὰρ βάναυσον τὴν τέχνην ἐκτησάμην is the motto we would wish all members of a liberal profession to adopt; and though we might be debarred at times from sources of immediate personal gain, there would infallibly be a large balance of advantage in the end. There is probably no indirect influence which will more tend to bring about this healthier tone of feeling than the humanizing power of art. For though the old sneer at medicine, lately repeated,* that it is atheistical and materialist in its tendencies, is far from universally true; still it has some foundation, for such a result has at times followed from it, but only, we think, when studied by minds originally blunt to delicacy and refinement, proceeding on a technical and unworthy appreciation of the objects of their pursuit. In Mr. Ruskin's view of the case, the contrast is put very strongly before us; we see the light in which the cultivated artistic mind is disposed to view the subject; and while we acquiesce fully in his facts, we can hardly fail to regret that such words should not rather owe their origin to a member of our own society. He says:

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[*] We regret that a publication like the 'Saturday Review,' which has hitherto maintained so judicious a tone on these subjects, should indulge in such unfairness as occurs in an article headed The Republic of Plato (April 30th, 1859. Making every allowance for the hard necessity of introducing a dull topic with sufficient smartness, and for the pungent odour of the rushes of the Cam which pervades the composition, it is difficult to think that the writer seriously believes his own argument; impossible to suppose him prepared to accept its logical consequences.

> "I reverence physical science, more as the source of utmost human practical power, and the means by which the far distant races of the world, who now sit in darkness and the shadow of death, are to be reached and regenerated. At home or far away, the call is equally instant: here, for want of more extended physical science, there is plague in our streets, famine in our fields; the pest strikes root and fruit over a hemisphere of the earth, we know not why; the voices of our children fade away into silence of venomous death, we know not why; the population of this most civilized country resists every effort to lead it into purity of habit and habitation, to give it genuineness of nourishment and wholesomeness of air, as a new interference with its liberty, and insists vociferously on its right to helpless death. All this is terrible; but it is more terrible yet that dim, phosphorescent, frightful superstitions still hold their own over two-thirds of the inhabited globe; and that all the phenomena of nature which were intended by the Creator to enforce his eternal laws of love and judgments, and which, rightly understood, enforce them more strongly by their patient beneficence and their salutary destructiveness, than the miraculous dew on Gideon's fleece, or the restrained lightnings of Horeb; that all these legends of God's daily dealing with his creatures remain unread, or are read backwards into blind hundred-armed horror of idle cosmogony. How strange it seems that physical science should ever have been thought adverse to religion. The pride of physical science is indeed adverse, like every other pride, both to religion and to truth; but sincerity of science, so far from being hostile, is the pathmaker among the mountains for the feet of those who publish peace."

One of the most painful manifestations of the false position taken up by many members of our body, is strongly contrasted with the liberal and comprehensive spirit of the preceding quotation. We mean the sort of antagonism which, especially in country districts, springs up between the medical man and the visiting clergy. Much of this is undoubtedly due to the injudicious meddling of the latter with the special subjects of medicine, on which the wiser course were faith or silence. But as much, or more, is due to the irritable sense of annoyance, far too common among our brethren, at any influence in the sick-room, independent of their own. Such opposition, where it does exist, is the greatest misfortune for all concerned; most directly to the poorer classes, who habitually look up to the doctor and clergyman as the two great luminaries of the place, and who feel in a thousand ways the misery of a divided allegiance. It has already come in our way to show a probable source of amendment on the one side, by a greater diffusion of the general principles of natural science among educated men; and the best hope of an improvement, not less needed on our own side, seems to lie in the re-association of the poetic sentiment, if we may dare to say so, with the sterner matter of professional avocations. It is a pity that the two have ever been so divorced, and the separation is only of these later times; from the days of Hippocrates to those of the Rosicrucians, till the age of Van Helmont, or even of Sir Thomas Browne, medicine was indissolubly joined with a religious and æsthetical element; which, if it was at times degraded into superstition and pageant by vulgar minds, formed, nevertheless, its highest attraction to a superior order of intelligence. Dangerous it truly is, to trust such a guide in the reasonings and in the observation of our art; but the history of our predecessors shows that the closest logical analysis, and the most stringent accuracy in experiment, are not incompatible with a sanctifying reverence, and a fervour akin to enthusiasm for the wonderful objects of inquiry. We may call to mind the fine painting of Andreas Vesalius,* as he stands with darkened windows beside the corpse from which life has not long departed, and pauses before breaking rudely even into the ruins of the temple of life; in the eyes, turned with earnest gaze towards the crucifix, we read that highest flight of man's intellect, which, while it scrutinises with critical niceness the traces of function and contrivance, can still look through these to the beauty, the innate fitness and harmony, which,† like notes of thrilling music, pervade all created things.

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* "Nos médecins sont une classe d'hommes extrêmement éclairée, et, selon moi, la première de la France sans comparison. Aucune autre ne sait autant, ni autant de choses certaines. Aucune n'est si bien trempée d'esprit et de caractère. Mais enfin leur rude éducation masculine d'écoles et d'hôpitaux; leur dure initiation chirurgicale, une des gloires de ce pays; toutes ces qualités ici entrainent un grave défaut. Elles aboutissent en eux à l'extinction de la fine sensibilité qui seule pourrait percevoir, qui prévoit, dévine les choses."—J. Michelet: L'Amour, p. 226.

If, as the preceding example seems to show, the union of such different qualities in one mind is not impossible, it is surely our duty to desire and inculcate it. With this view, we decline to join in the disapprobation so strongly expressed by a few persons at the decorative element in the Oxford Museum. Ornament can only be held objectionable when it interferes with matters of more necessary import. At present such fear is not to be entertained; while we can point to solid and substantial works like the two excellent chemical manuals, of which mention has been made, issuing from the Oxford school, there is no danger of a deficiency in real earnest productive labour, equal to what the meanest and most microscopic conception of the scientific man's mission can give birth to. And we hold that all delicacies of workmanship serve a useful purpose; by entwining an element of beauty about the sternness of the facts, they endear these the more strongly to the inquirer, and, like the sculptured capitals of the museum pillars, crown the rugged representative of science with a repeated remembrance of the fair forms in which science is enshrined.‡

If it be wise at all times to enlist in our service the refining power of artistic sentiment, it is most particularly desirable during the course of the educational process; for the mind of a student is especially open to these secondary influences; he is young and ardent in his new pursuit, his conceptions of his dignity and usefulness are as yet unsettled, every day is adding new facts and fresh ideas to his stores, which will hereafter more or less bear the tincture and colour of the place and time in which they were first gathered. Which of us does not now and then go back in memory to a period long passed by, on opening some old book, some dusty collection of minerals, or of withered botanical specimens, once objects of deep interest and study? All the feelings of that time have faded away and been forgotten, we seem to ourselves other men since then, and yet we are aware that throughout the whole intervening space our conception of that branch of study has been moulded on the associations with which it was then intimately blended. It is perhaps with profound intuition of a half-evolved truth that the present age is making association play so great a part in the educational method; for while we cannot as yet define the exact relation of this power to memory, their resemblance and kindred is indisputable. Much of the success of our great public schools seems due to its operation; and the Universities have always prided themselves on their power, not only of imparting knowledge, but also of moulding individual character to a greater polish and increased fitness for social life, by the force of similar agencies. Few men long resist the combined influence of the *genius loci* so strongly embodied in the institutions and buildings themselves, the refined habits, the powerful and regulated public opinion, and the silent force of the religious element, closely bound up with the collegiate system itself.

We rejoice to see the domain of these forces extended over the students of physical science, and specially over those destined for our profession of medicine. No antidote could be found so efficacious against the vicious lessening of the moral horizon, and the disorganizing laxity of habit, besetting sins of that class, which are perhaps in part due to the nature of the educational course itself. Until recently it might be said that these advantages applied only to a limited number, from which the majority of our brethren are excluded. It is indeed true that only a small numerical proportion would be contributed by the Universities to the ranks of the medical profession, even if the wishes of the most sanguine adherents of the new *régime* were to be fulfilled. But there is another channel opened, by which the University influence is being very widely, and, as we think, judiciously, diffused, and by which no profession will profit more immediately than our own. The recently enacted statutes for the examination of those *qui non sunt de corpore Universitatis*, are already beginning to attract crowds of candidates for the certificate of proficiency in all the large towns. To no class of young men will such a testimonial be so really valuable—by none, as we hope, will it be more energetically striven after—than by those who purpose commencing their career as medical students. For, in the first place, it is fixed at a period of life which very accurately marks off a division between the general educational course and the commencement of more specific professional studies. If, by so doing, it in a measure tends to cut short that most unprofitable portion of the ordinary student's career, which is represented by the apprenticeship in a country town, preliminary to attending hospital lectures in London, it will at least have done a negative good; and will, in return, have entailed some positive advantage, by raising the standard of acquirement in the ordinary branches of liberal education. We would willingly exchange, in the great majority of cases, the scanty, imperfect, and purely empirical information of a private practitioner's surgery, for some better mental training and more intimate knowledge of the great writers of ancient and modern times, for the prominent facts of history, and the higher branches of mathematics. In the second place, these examinations may be made useful as a first winnowing—a preliminary classification of the examinees, which will tend to guide the judgment of friends and relations in determining a young man's future path in life. Medicine is the last of all occupations not absolutely mechanical on which a youth should be thrust hap-hazard, and because, as often happens, his father followed it before him. It needs more definite bias and predilection than any other for its satisfactory pursuit, and it presents more points of distaste and discouragement to an ill-matched aspirant. The mercantile man and the government *employé* can give certain hours of the day to their business, and these over, devote their energies without scruple to other more congenial studies and amusements. Even the barrister, who is probably more called on for severe and continuous mental exertion at times than any other professional man, has usually abundant intervals to recruit his strength; and, except in the case of an overwhelming practice, can throw aside business for a while without damage to himself or his clients. But the medical man must keep himself always in readiness; no time is free from the possibility of calls on his assistance; his practical knowledge, extending as it does over one of the largest areas occupied by any science, must be ever fresh, clear, and ready at hand. Books are but an inefficient auxiliary. His whole life should be one of preparation for emergencies, his temper of mind that of a sentinel on guard.

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* And. Vesalius was one of the first to use post-mortem examination as a verification of diagnosis; in 1564 he was condemned for this by the Inquisition. And yet the religious element of his mind can now be traced in his works.

† *ἐκ πασῶν δὲ ὀκτω οὐσων, μίαν ἁρμονίαν ξυμφωνεῖν.* Cf. Plato, Repub. X., Mythus of Er.

‡ Each pillar in the great quadrangle is formed of a different rock or marble, in classified order. The surmounting capitals are carved into representations of plants, in corresponding botanical series. No absolutely conventional foliage is introduced, each specimen being worked from nature.

Is it not of the last importance that requirements so exceptional should, if possible, be founded on special gifts and predilections? It is our earnest hope that these newly-organized examinations, which we owe to another member of the Acland family,* may in some degree supply this test. They present, indeed, sufficient provision for examination and reward in the domain of science and natural history to enable a fair estimate to be formed of the candidate's fitness for the demands of his proposed profession. In this way much good may be done, not only by the early exclusion of the incompetent and unsuitable candidates, but also by the discovery of latent capacity, and by the means afforded for distinguishing a mere sciolistic whim and fancy for mechanical and physical amusement, so common among intelligent lads, from that steady preference and election of a certain class of subjects, which commences in faculties and bodily endowments adapted for their pursuit, is fostered by friendships and opportunities not the less powerful for being often overlooked, and terminates by developing into a temper which is almost always able to command success for its highest aspirations.

It is perhaps not unreasonable to hope that an extension of the University brotherhood, a dependence more or less intimate from the same ancient *Alma Mater*, may tend to draw closer together the bonds uniting different members of the same profession. Any means which should effect this would be an incalculable boon to our whole body; for if there be a mark of inferiority more painfully characteristic of medical men than any other, it is an utter lack of corporate feeling. "Every man for himself" is almost the motto of many practitioners, and though some may rise above this short-sighted and suicidal selfishness, hardly any attain that measure of *esprit de corps* and of unanimity which ensures esteem. The ordinary hospital connexion seems in most cases utterly insufficient for its production. There is, as a rule, little or none of that legitimate pride of clique among pupils of the same medical school which is so apparent in our best public schools or in members of the same college at either University. And its absence gives just ground for regret, for probably no security is so good against attacks from without and schisms from within. It is not indeed inconsistent with much personal difference of opinion; it necessitates little or no interchange of overt acts of friendship, but it produces nevertheless a closer union, a more conciliatory tone, and a larger share of that tacit freemasonry of thought by which each individual member is a support and helper to the others.

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* T. D. Acland, Esq., of Spreydencote.

Our earnest wish is for the establishment and increase of this good understanding among the scattered disciples of the healing art. It would more tend to raise them than any legislative enactments against quackery, or any mere examination-test, however stringent. And while we see with pleasure faint glimmerings on the horizon of a better and brighter day, it is the bounden duty of every individual among us to gird himself up to help forward the good work which that day is bringing to light. Conspicuous as physicians are and have always been beyond all others for personal earnestness and self-devotion, there is still room for a higher conception of their social responsibilities and of their mutual inter-dependence as members of a world-wide guild and confederation. Let us aim at the attainment of our forefathers' standard, for there is none higher or more worthy, and let each accept the obligation of the oath so nobly tendered by the Father of Medicine himself.[*]

"To reckon him who taught me this art equally dear to me as my parents, to share my substance with him, and relieve his necessities if required, to look upon his offspring on the same footing as my own brothers, and to teach them this art if they shall wish to learn it, without fee or stipulation, and that by precept, lecture, and every other mode of instruction, I will impart a knowledge of the art to my own sons and those of my teachers, and to disciples bound by a stipulation and oath according to the law of medicine, but to none others. I will follow that system of regimen which, according to my ability and judgment, I consider for the benefit of my patients, and abstain from whatever is deleterious and mischievous. With purity and with holiness will I pass my life and practise my art. Into whatever houses I enter, I will go into them for the benefit of the sick, and will abstain from every voluntary act of mischief and corruption. Whatever in connexion with my professional practice or not I see or hear in the life of men, which ought not to be spoken abroad, I will not divulge, reckoning that all such things should be kept secret. While I continue to keep this oath unviolated, may it be granted to me to enjoy life and the practice of the art respected by all men in all times. But should I trespass and violate this oath, may the reverse be my lot."

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[*] Hippocrates, Ὅρκος, Sydenham Society's translation, vol. ii. p. 780

REVIEW VIII.

The Diseases of the Stomach, with an Introduction on its Anatomy and Physiology; being Lectures delivered at St. Thomas's Hospital. By WILLIAM BRINTON, M.D., Fellow of the Royal College of Physicians; Lecturer on Physiology and on Forensic Medicine in St. Thomas's Hospital; Physician to the Royal Free Hospital. *London,* 1859. 8vo, pp. 406.

THIS book can hardly be said to require a very formal introduction to our readers. A great deal of the elaborate material of which it is composed has been contributed to our own pages in the form of Original Communications; and we had formerly the pleasure of reviewing the collected pages of the author on the important subject of the simple gastric ulcer. The present work is the complement and consequence of these former contributions to scientific inquiry; it is the substance of a course of lectures addressed "to the more advanced students of St. Thomas's Hospital;" and professing, as it does, to give a more systematic, as well as a more condensed, view of the author's researches than was consistent with his former plan, it now takes the shape of a treatise addressed to the whole of the medical public. Certainly, no better guarantee could possibly be given for the faithful discharge of the duties implied in writing such a treatise, than the previous career of Dr. Brinton. His book is not, perhaps, one which occupies the entire field implied in the title; but it is one everywhere inspired by the spirit of truth, and is comprehensive enough to prove useful and suggestive. It hardly aims at being brilliant or amusing, but it is everywhere readable, and, without tediousness, it is earnest, solid, and instructive. Among the numerous works which have of late years issued from the press upon gastric pathology, it yields to none in importance; and we feel assured that it will be found to supply a want, even in this crowded region of medical literature.

What has the stomach done to attract to its delinquencies and misfortunes so large a share of public and professional attention? This is a question we often ask ourselves; and it is one easier to ask than fully to answer. In one way, indeed, it is easily enough answered. The poor man has no time to think about his stomach—the rich man makes an idol of it; all its whims and caprices, all its abilities and disabilities must be studied, in obedience to the demands of a luxurious age, with the same solicitude about small matters which Fashion demands of her votaries in everything else. The "diseases of the stomach" are, under this view of the matter, a little field of fantastic sufferings struck out of chaos, to meet the wants of an advanced civilization, and, especially, to attract money into the pocket of the doctor. That respectable gentleman, the fashionable stomach-physician, is, to a great extent, the necessary complement, or rather aide-de-camp of the cook; and, like him, does not come from the same quarter which sends us "good meat," but from a decidedly lower and warmer region than even the kitchen. And so, in more senses than one, that love of money, which we are told is the root of all evil, is the source of innumerable woes to the stomach. A comfortable balance at the banker's counts, not only for a multitude of good things, but for so much gastric misery and so many doctor's fees. Dyspepsia and oxide of silver wait upon our luxurious modern life like the "Memento Mori" at a Roman festival. The rich man soon languishes; he is sick, "not unto death," but only to the extent to require a little delicate attention. The doctor prospers; the cook has his box at the opera; Mammon smiles on the game; and the patient, soothed and flattered with so much attention, is gracefully handed over from the doctor to the cook, and back again from the cook to the doctor, till he becomes a confirmed valetudinarian. And of his endless tales of little errors and little miseries, one half fanciful and the other half exaggerated, the record goes forth, under professional auspices, dignified with the name of "diseases of the stomach," which have the same resemblance to real diseases that the modern expanse of crinoline has to the primeval garment of the first mother.

To any one who has arrived, by dint of much reading (or much physicking), at conclusions like these, we would recommend Dr. Brinton's book, and especially its final chapter, on "Dyspepsia," as an example how much the spirit of truth and soberness can do for a subject which has been twisted and tortured into such an immense variety of forms. This chapter is indeed short—far too short, indeed, if the frequency of the disease, and its importance in practice, be considered, as compared with those devoted to the organic diseases of the organ; and we strongly recommend to the author to keep in view the necessity of a considerable expansion of his ideas on the functional disorders of digestion in any future edition of the book. In the meanwhile, however, we are happy to find in this chapter, and in Chapters I. and II., which may be regarded as the key to the author's pathology of dyspepsia, a pretty distinct recognition of truths which are often present to our own mind, when dealing with, or speaking of, the diseases of the stomach in general. Some of these truths are well expressed in the following sentences:

> "The liability of the digestive organs to functional derangement is a kind of provision of nature against graver and more deeply-seated disease, a provision such as has considerable analogy to the protective function of pain, and to that mechanism of common sensibility of which it forms a part. The position of the alimentary canal (and especially of the stomach) relatively to the food, enables its disturbances to forestall and prevent the mischief that might (and in persons of powerful digestion actually does) gain access to the blood, and through this fluid invade other organs. Unchecked dyspepsia is doubtless not devoid of danger, both by what it thus foreshadows, and by what it can itself produce; just as physiology must own that—*in* and *per se*—pain is an evil. But it stands (as it were) midway between certain deleterious agents and bad habits on the one hand, and the *penetration* of the constitution on the other; and, if its warnings are attended to, is often the means of rescuing its victims from the slower and more dangerous consequences of these errors . . . . . The sufferings of indigestion often call attention to some of these errors of nutrition which, if persisted in, inflict serious or irreparable mischief in the form of tubercle, rheumatism, gout, or calculus; and they thus increase (rather than diminish) the longevity of those whose prudence does not allow this epigastric monitor to warn them altogether in vain." (p. 380.)

We cannot afford space to extract the remarks, condensed as they are, which contain the exposition of the causes of dyspepsia. They are in harmony with the view of its nature embraced in the sentences quoted above. In few words, the causes of this disease are not to be sought in the stomach, unless in the very common case of their being directly put into it in the shape of improper food; in the majority of instances they are complex, and bear far less directly upon the stomach than upon the other functions, and especially upon the nervous system. Thus intellectual exertion, ill-regulated, or producing an excessive strain upon particular faculties; mental anxiety, joy, grief, laziness, apathy, over-fatigue, and *under*-fatigue of body, may all become causes of dyspepsia, because they may all derange the mechanism of that *vivida vis animi*, which is the centre and culminating point of the animal functions, and with which the vegetative life is brought into relation chiefly through the stomach. Hence, as Dr. Brinton well remarks:

> "Our very forms of speech, which, with little exaggeration, represent a person as 'sick' of any person, thing, or topic, show how the chemistry of the stomach is subjected to the least material and palpable agents of our life, to that world of thought and emotion which works within every one of us."

If we are not greatly mistaken, this idea will bear to be pushed further yet; and we commend it to Dr. Brinton as worthy of more elaborate development than he has bestowed upon it. We are disposed to regard the stomach as one of the most ill-used of organs—ill-used, not in the mere material sense that much is put into it which ought not to go there, and much of what is fitting, at unfitting times and seasons—but in the more exalted sense, that of all the organs of the body it bears the greatest burden of sins not its own, and bears them, on the whole, with an equanimity (so to speak) and power of endurance alike admirable and entitled to our gratitude. We are entirely convinced that the nervous system is responsible for the immense majority, we had almost said the whole, of the cases of dyspepsia which do not depend upon errors of diet, or on organic disease; and, further, that in most cases of the latter kind, the stomach is not the real seat of the malady, but only the recording apparatus or danger-signal, as it were, of disorder existing elsewhere. Most thoroughly do we participate in Dr. Brinton's views on this subject, as expressed in the paragraph quoted above; only we desire to see them pushed fairly to their consequences in detail. The course of his researches has led him in this volume to insist at great, almost disproportionate length, on the organic diseases of the stomach. The immense field of its functional derangements demands no less his exact and careful analysis; and it will amply repay the labour. The recent work of Chomel on the subject is, to our apprehension, among the least satisfactory of his contributions to medicine; and among the British authorities, we know not one who has not left ample room and verge enough for a new and enlarged consideration of the subject. The single topic of hypochondriasis and its connexion with gastric symptoms would of itself form material for a chapter of some length. Yet so little have our recent authorities on diseases of the stomach found it to be within the somewhat narrow range they have prescribed to themselves, that there is hardly a hint to be found in any of them upon the subject. So important a morbid relation should not be given over to the quacks.

And here we are tempted to make one remark tending to a less inglorious view of the connexion of dyspepsia with an advancing civilization than that we have put forth, half in jest, at the beginning of this article. May it not be that, with the growth of man in intelligence and moral elevation—in proportion as spirit rises supreme over matter—the sensibility of the whole nervous system is exalted, and its diseases, or functional derangements, are rendered proportionately important? It is the characteristic of an intellectual age to suffer the penalties of an over-worked or ill-worked brain. But among these penalties a restricted digestive faculty is perhaps first in rank and in frequency. It is the inexorable fate which binds the intellectual workman to the earth, and reminds him that he must not follow his aspirations to the neglect of the machinery that controls the very power of thought. The lowest of his appetites holds him in check, as if to remind him that true wisdom is not in rising above human nature, but in submitting gracefully to all its limitations.

But we must not linger on this subject. Suffice it to say that the entire sketch of dyspepsia, considering it only as a sketch, is excellent; the analysis of its multiform symptoms and of their combinations being at once simple and comprehensive, and the treatment equally judicious and clearly stated. In the general description of the symptoms of gastric disease, the author is not less successful in producing a trustworthy picture of a difficult subject; and the ideas, even if not absolutely novel, are often so strikingly enunciated as to have the effect of novelty. The following passage on gastric pain is important. After showing that a high degree of tactile sensibility is "unnecessary" (he might say "would be positively injurious") to the stomach in the discharge of its function, and that therefore no such sensibility has been accorded, the author proceeds:

> "But we are not, therefore, to imagine this important organ really insensible to stimulation, or to suppose that, because it is not every moment arousing the brain of its master, and demanding his forethought or exertion, it remains unimpressed and inactive. On the contrary, we must rather conclude that it has a special sensibility of its own, not one whit less marvellous (but surely more so) for possessing a certain independence of the cerebro-spinal centre. Closely related to this centre by the feelings of hunger and satiety—nay more, dictating to it (so to speak) those exertions which the proper alternative of these two states imperiously demands from the mass of mankind—it has a sphere of action altogether its own. And the study of digestion has shown us how admirably and silently the stomach fulfils its various and complex tasks; and how, incidentally to these, the unfelt particle of food no sooner touches its mucous surface than it excites the flow of a variety of secretions, both far and near, and provokes movements in the muscular substance of its walls and vessels, as well as in the analogous structures of neighbouring parts. To these acts, which respectively constitute the sensation and motion of the healthy stomach, its morbid states afford an instructive parallel. And just as the kind of sensibility specific to a healthy muscle—the feel-

> ing of its strength, its equilibrium, its measurable force—seems to be traceable by gradual modifications, through healthy fatigue to the feverish soreness of over-exertion, and through this to the universal muscular pain and prostration of various grave general ailments, so the indistinct sensation of the healthy stomach affords us the best clue to the acute sensibility of the diseased one, and allows us to trace a scale of a similar kind—from satiety to repletion; from repletion to distension and weight in the epigastrium; and from hence to the dull heavy aching of dyspepsia, the gnawing or burning pain of ulcer, and the sharp agony of cancer of the stomach." (pp. 48, 49.)

Again, a few pages further on, we find the following:

> "The pain of gastric disease is grave in proportion not only to its severity, but also to its concentration and fixedness. In other words, a severe and continuous pain, confined to a single spot of small size, is a more serious indication than one which at times of equal (or nearly equal) severity, fluctuates in its different attacks, and ranges the epigastrium, of which it habitually occupies a wide area. Pain is graver in or near the median line, not only because (for many reasons which will readily suggest themselves) it is, *cæteris paribus*, more certainly gastric here than elsewhere, but because this situation (at least, such is my opinion) indicates a more serious derangement of the innervation of the organ than when the pain has a less exact correspondence with the solar plexus. Lastly, of all situations, a median and dorsal one (in anatomical language rachidian, and ranging from interscapular to lumbar), which is usually an addition and complication to a previous epigastric pain, is the most serious—so much so, that it will rarely be found associated with any but the graver gastric dyspepsias, and belongs chiefly to deep ulcers, or to cancerous lesions of the stomach, involving all its coats." (p. 53.)

There is excellent matter for thought in all this; and it suggests, as does the whole book, the idea that more has been thought than is written. We would, however, interpose a caution which arises to our minds in copying these passages. Let the author jealously guard against his besetting sin, which is the over-use, sometimes amounting to abuse, of parentheses, commas, dashes, and all the other means invented by modern typography, for presenting sentences to the eye in the highest state of literary perfection. The necessity for employing to the utmost these arts of the intellectual *cuisine* is not always consistent with that robustness and vigour of style which is to be cultivated as the highest type of health; it rather indicates a kind of mental dyspepsia, not seriously affecting the vitality of the author, but interfering somewhat with his comfort and with the satisfaction of his readers. Let him take in good part this hint from a fellow-sufferer, who has had to make great exertions, perhaps only in part successful, towards overcoming this malady. It is only a "danger-signal," and possibly prophylactic of graver evils; but it requires watching.

In speaking of vomiting, the author advances the view that it is essentially an act of the cerebro-spinal centre, whether produced by reflex transmission of nervous influence, or by direct irritation. Even when the cause of irritation is introduced into the stomach, he holds that it operates not always through the special nervous systems of the abdomen, but (at least in the case of tartar-emetic) through a multitude of disturbed functions connected with the absorption of the poison into the blood, and telling their tale of distress upon the cerebro-spinal centre. This view is no doubt correct, at least as regards those poisons which, like tartar-emetic, produce the same results when introduced into the stomach and into the blood. We think that an important distinction is to be drawn, in this point of view, between different kinds of emetic substances. Some, like mustard, and, perhaps, sulphate of zinc, appear to act by direct irritation. Their operation is sudden, quick, complete as regards evacuation, but attended by little nausea, and no more permanent result than emptying the viscus. Others, like arsenic and antimony, are really very uncertain in their emetic effect (dose for dose), as compared with the preceding; but they produce grave constitutional disturbance, and the nausea, prostration, and death, which is their maximum result, are induced fully more readily by moderate than by large doses, because the latter tend at once to emesis, and, therefore, to removal of the poison. In connexion with this elimination of irritant poisons by vomiting, Dr. Brinton has performed a remarkable experiment, which seems to assign to the stomach a higher office than that of merely resenting the presence of a deleterious substance. It would appear probable that the stomach is really an excretory organ of the first importance, as regards tartar emetic; for that poison, when injected into the blood of a dog, is soon found in the stomach "in a state of concentration much exceeding that in which it must have been mingled with the mass of the blood."* This is a very important doctrine in relation to general pathology. Dr. Brinton points out that vomiting in disease must probably often be considered as an eliminative act; and he refers, in illustration, to the vomiting at the commencement of fever. It is certainly a remarkable fact, that among the various means which have been proposed for cutting short the attack of contagious fevers, the only one which, to our mind, has any good authority, or any considerable amount of evidence in its favour, is that of an emetic administered during the first few days; and it is worthy of notice, that the class of emetics always recommended for this purpose consists of those which in all probability enter into the blood, and shake the nervous system to its foundations previous to their action on the stomach. These facts are strongly corroborative of the author's ingenious theory; which, moreover, derives additional interest, if not strictly scientific support, from the evident manner in which it would demonstrate the "final cause" of that kind of vomiting which accompanies poisoning by arsenic and antimony, when introduced into the system by other channels than the stomach. There is a purpose, too clear to demand explanation, in the selection of the stomach as the leading channel for the elimination of injurious substances, which find by that organ their usual entrance into the economy.

In regard to hæmorrhage, Dr. Brinton of course repudiates the old doctrine of hæmatemesis "by exhalation." It is certainly true that some cases of fatal gastric hæmorrhage are very puzzling to the morbid anatomist; but, as the author remarks, it is not surprising that "amongst the myriads of these minute tubes present (in the mucous membrane), the eye often fails to detect the exact vessel or vessels involved in the lesion." Dr. Brinton believes, and we think correctly, that the *inter-tubular* vessels are rarely the seat of hæmorrhage as compared with the superficial, or subra-tubular network. We believe, in fact, that the majority of what are called the "hæmorrhagic erosions" of the gastric mucous membrane are the result of direct irritation, acting perhaps upon a mucous membrane accidentally denuded of its protective covering. And, if we mistake not, a very considerable proportion of the minute microscopic lesions described by recent authors are, in like manner, mechanical or chemical in their origin, whatever their importance or insignificance in regard to gastric pathology. This is evidently Dr. Brinton's view in so far as he goes into detail. Though he has apparently not been inclined to make a very pointed critical analysis of the observations of Dr. Handfield Jones on this subject, we gather from a few hints his opinion that an exaggerated importance has been assigned by that able pathological anatomist to appearances, the result, in many instances, of decomposition and manipulation of the mucous membrane. The recent researches of Dr. Wilson Fox will probably bring the subject once more under consideration, but we cannot stop here to enter upon it; nor, indeed, do we feel that we have a right to assume a tone of authority in relation to a matter depending on such delicate investigation. Certain it is that no intelligible relation has as yet been pointed out between the multitude of post-mortem changes, and the functional state of the organ during life; and, till this is done, we must decline to admit into the rank of diseases what are, at best, only problematical appearances of disease.

Dr. Brinton's doctrines on flatulence and on the symptoms connected therewith, are in some respects peculiar; and here we shall not say that he absolutely carries our convictions, but unquestionably he makes good show of weighty argument in favour of his own views. He entirely rejects the idea of secretion of air into the intestinal canal as a cause of this symptom; pointing out that the gas of flatulent intestines, even when not containing elements which cannot be procured from the blood in appreciable quantity, differs altogether in composition from the usual gaseous products of the system as evolved in expiration. "It contains," he remarks, "ten times the quantity of carbonic acid, and two hundred times the quantity of nitrogen found in expired air;" and he argues, not without force, that gas of this composition, is much more likely to be evolved from the food than from the process called "secretion," which has only been hypothetically called into existence to account for cases of enormous flatulent distension occurring within a very short period of time. Still, this is only, after all, a question of probability, and it does not appear impossible that the gas of secretion may, when separately procured, present a different composition from that which has hitherto been analysed as intestinal flatus. Dr. Brinton lays great stress on the fact that in sudden and complete starvation the gases disappear from a great part of the canal; but the stoutest advocate for secretion will not deny, now-a-days, that gases are evolved in large quantity from the food; and, further, that the absence of food from the system diminishes, as respects the blood and the tissues, the sources from which waste gaseous matters are to be drawn, according to the hypothesis. The experiments of Magendie and Girardin (incidentally repeated by Frerichs), according to which gas is found in a coil of intestine for some time isolated by ligatures, and previously emptied by pressure, is objected to by Dr. Brinton on the ground that it is impossible by pressure entirely to empty the intestine of its contents; and that a single grain of starch or sugar remaining would, under decomposition, evolve gases equivalent to eight cubic inches of space. Surely this experiment is important enough to justify its repetition with additional precautions. There could be no difficulty, one would think, in washing out every particle of starch or of sugar from a coil of intestine, previously to the application of ligatures; besides, the character of the gases evolved would, in this case, at least, be ascertainable, and would be of great importance to the inquiry. This experiment is really crucial as respects the doctrine of secretion; and we cannot consider the negative of Dr. Brinton as carried until it has been thus tested.

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* See p. 61 of this work, and the author's article, Stomach, in the 'Cyclopædia of Anatomy;' also the 'Lancet' for 1858, vol. ii. p. 599.

Dr. Brinton insists much on the pressure to which the intestinal gases are subjected, as modifying their bulk, and believes that many of the cases of apparently sudden evolution of gas are really due to the expansion of the gas already in the intestinal canal by the temporary paralysis or at least relaxation of part of the wall. He explains the tympanitic distension of peritonitis in this way, and also that gastric flatulence which is the rapid result of taking a few mouthfuls of improper food. In the latter case, he believes that the irritation induces,—1st, an abnormal sensation, sometimes amounting to pain, or at all events to an impression on the nerves of the part, which causes, 2d, a relaxation in the coats of the stomach, and thereby, 3d, expansion and rarefaction of its contained air; 4th, a secondary rush of air from the duodenum through the relaxed pylorus; 5th, increased sense of pain or uneasiness by over-fulness of the stomach; 6th, expulsive efforts of eructation. This view, which undoubtedly is favoured by many facts in the symptomatic history of colic and other flatulent affections, will bring to the mind of the reader the late Dr. Abercrombie's observations on ileus, which led him to the doctrine that spasm, commonly so called, was no part of the pathology of that affection, but rather a paralytic relaxation leading to over-distension of the bowel and undue retention of its contents. The most important novelty, perhaps, of Dr. Brinton's view is the idea that the pain of such attacks is not the effect of the accumulation so much as its cause, or at least its antecedent. The pain is the expression of an outraged sensibility, so to speak, of the mucous membrane; the paralysis and the distension follow after the nerves have reflected their abnormal impressions on the muscular apparatus of the organ. There is much in this view that commends itself to the practical experience of the physician, but we prefer to leave it, as Dr. Brinton does, without further development.

In all that the author says upon Acute Gastritis we cordially concur. He adopts the statement of Abercrombie, that acute inflammation of the gastric mucous membrane hardly occurs, except as a consequence of irritant poisoning; and he treats the idiopathic acute gastritis of nosologists as a practically non-existent disease. We are disposed to go beyond our author in this point, and to assert very nearly the same thing of subacute and even chronic inflammation, for we have never seen anything coming distinctly within the range of inflammation at all in the gastric mucous membrane generally, which was not to be accounted for on the supposition of mechanical or chemical irritation. In general terms we would say that according to our pathological experience, the gastric mucous membrane, considering the number and variety of the stimuli to which it is constantly subjected, enjoys the most remarkable immunity which it is possible to conceive, from serious inflammatory disease, and has the most marvellous power of recovery after even serious injury. And in no point is the stomach more wronged than in the facility with which dyspeptic and feverish symptoms are set down as the result of "chronic gastritis." That the exposed stomach of Alexis St. Martin, tortured from day to day by the direct application of unmasticated food, habitually irritated by curious physiologists in search of gastric juice, and occasionally by strong alcoholic drinks, should only now and then have resented this treatment so far as to cease to give forth gastric juice, and to undergo a temporary congestion and partial extravasation of blood, seems certainly to us rather to disprove than the contrary the asserted liability of the organ to "chronic inflammation." Consider that there is not a trace of evidence that *pus* was ever formed on this mucous membrane so irritated, and then consider the facility with which this product is evolved under the slightest forms of external irritation on all other mucous membranes excepting those of the alimentary canal, and it will be apparent, we think, how small a share is taken by general inflammation in the diseases of the stomach. The utmost that we can admit as at all common is the existence of transient congestions, resulting perhaps in an excess of mucous secretion and a temporary arrest of the proper digestive process. We agree with Dr. Brinton in doubting even the existence (considered as a disease) of the so-called "chronic catarrh of the stomach," but if it be really a disease, it is quite clear to our convictions that it bears no resemblance to inflammation.

It is more difficult to speak with confidence of the partial affections of the mucous membrane. Beyond all doubt the stomach is subject to disorganizing processes, limited to particular spots of the mucous surface, and the pathological character of these may reasonably be supposed to be inflammatory. Such are the simple perforating ulcer and the hæmorrhagic erosion. But the relation of these to the ulcerations of other mucous membranes is not a little obscure. The chronic simple ulcer of the stomach is an isolated fact in pathology. Nothing in its history, in its antecedents, in its consequences or accompaniments, throws the least light upon it; and except that the entirely normal character of the general mucous membrane, and the absence of all inflammatory products on the surface of the ulcer itself, seem to belie the supposition, we might as reasonably refer it to inflammation as to anything else. But the destruction of an accurately limited space of mucous membrane, bearing no relation to any special glandular structure, and this without sloughing or suppuration having ever been observed at any stage of the process, is a fact too anomalous to remain securely under this all-comprehensive pathological denomination. It is wiser, surely, to admit ignorance than to speculate in such vague fashion as this. And here again we find ourselves at one with Dr. Brinton, who declines to pronounce on the causes or mode of origin of the gastric ulcer.

We refrain from observations on the rest of the book, which is, notwithstanding, its major part. We have been chiefly anxious to bring before our readers such portions of the author's labours as have not been known to them from our own pages. It is unnecessary to enlarge here upon the exhaustive and admirable manner in which Dr. Brinton has treated of the Chronic Ulcer and on Cancer of the Stomach. The results of both these inquiries are given in this book as fully as is necessary for the busy practitioner, and in a form better adapted, perhaps, for his perusal than the original papers. The same conscientious care for truth has guided the author through every part of his researches, as is apparent in the ground we have now gone over; and indeed not one sentence or phrase from beginning to end of this work will bear the construction that it is written at random, or without the most serious reflection. We sometimes differ from the conclusions adopted; but we are obliged to do so with the respect which is due to well-considered opinions. The practical man will miss some of those vague and dashing generalizations to which he is accustomed in works of this kind. But he will miss them greatly to his advantage if he imbibes in any degree the author's earnest and sober spirit of inquiry.

There is an introductory chapter, to which we have not alluded, on the Anatomy and Physiology of the Stomach. It is of the same character as the rest of the work, but does not aim at putting forward any new views or observations on the subject.

In the preceding remarks, it is to be observed, we have not attempted anything like complete analysis. Were we to have done so, we should have been obliged to re-write the book, which is, especially in the parts on which we have touched, far too condensed in expression to admit of further reduction of its argument. Indeed, we are sensible at some points of having even expanded the author's ideas in referring to them. We trust we have done them no injustice in thus handling them, and in venturing to indicate further points on which we would willingly see the author engaged. The enlargement of this volume by a chapter or two in a future edition would, we think, make it still more serviceable to the public and to the reputation of Dr. Brinton.

Review IX.

Practical Midwifery: comprising an Account of 13,748 *Deliveries which occurred in the Dublin Lying-in Hospital during a period of Seven Years, commencing November,* 1847. By Edward B. Sinclair, A. B. F. K. and Q. C. P., &c. &c., Ex-Assistant-Physician to the Dublin Lying in Hospital; and George Johnston, M. D., L. K., and Q. C. P., &c. &c., Ex-Assistant-Physician to the Dublin Lying-in Hospital.—*Dublin*, 1858. 8vo, pp. 574.

The above volume, although purporting to be a treatise on practical midwifery, is essentially a statistical record of the deliveries which occurred in the Dublin Lying-in Hospital during the seven years' mastership of Dr. Shekleton, interspersed with some general observations on practice. We say essentially statistical, for there is scarcely a fact or an incident recorded in the volume which is not given in a statistical form; and it is curious to observe in how many ways the same fact may be numerically stated. As a mass of statistics, therefore, we regard the work as a valuable contribution to obstetrical literature, and those who attach more importance to such data than ourselves will doubtless accord to it a high tribute of praise. For ourselves, however, we cannot help thinking that throughout the work the practical has been too much sacrificed to the statistical—that an immensity of labour has been expended in stating facts numerically which are of little clinical importance, and that a too servile deference has been paid to the arbitrary rules of an artificial nosology. Thus, instead of the history of Face Presentations being given in a consecutive series, we find them scattered among different chapters of the book, in deference to nosological formulæ; so also is it with Puerperal Fever and many other of the subjects treated of. We concede that a large array of facts and figures gives an imposing character to a work, and that where upwards of 13,000 deliveries are recorded, there exist ample materials for such a purpose; but we nevertheless think that the volume would have been more generally useful and available if less encumbered with figures and calculations, and as we do not think we could interest our readers by a merely statistical abstract, we shall rather endeavour to enucleate from its pages the spirit or principles which would appear to have guided the practice in the Dublin Lying-in Hospital during the period over which its history extends.

Let us, however, premise a few generalities culled from the introductory observations. The report, as already stated, contains an account of the cases which occurred in the hospital during the seven years' mastership of Dr. Shekleton, which commenced in November, 1847, and terminated in November, 1854. During this period, 13,748 women were delivered, and gave birth to 13,933 children. Of these children, 7177 were males and 6756 females, whilst the still-born, including those born putrid, amounted to 968. Of the 13,748 women delivered, 4535, or one-third, were primiparous, 233 had twins, 1 had triplets, and 163 died in childbirth from puerperal and other causes. From these latter, however, it is right to deduct 17 who were admitted into the hospital in a dying state, leaving a balance of 146 deaths from all causes, or 1 in 94. But of these it would appear that 70 deaths were due to puerperal epidemic disease, which subtracted, would reduce the mortality to 76, or 1 in 180⅔, and of these it is further to be observed that 40 died of other than puerperal diseases, such as apoplexy, bronchitis, pneumonia, phthisis, &c., leaving a total of 36 deaths only which originated in labour, or 1 in every 381½ of those delivered.

From the same source we learn that the greatest number of monthly deliveries was respectively in the following order:—May, March, June, April, July, August, February, November, December, September, October, and January, whilst inversely as regards mortality, the fewest deaths occurred during the month of May, and the greatest during December, the May series for the seven years presenting the least amount of mortality and the greatest number of deliveries, whilst the December gave the smallest number of deliveries and the greatest amount of mortality. Taking the series of months during the seven years in the order of their salubrity, commencing with the most healthy and terminating with the most fatal, we find them to run thus:—May, June, September, October, August, March, January, November, April, July, February, and December.

It is worthy of remark that the general fact thus indicated as to the salubrity of different seasons in regard to parturient females accords very strikingly with that which has been observed elsewhere, and the reader will find an interesting exemplification of it in a note published in the appendix to Dr. Ferguson's essay on Puerperal Fever. From it we learn that the most injurious months in Paris are respectively, November, October, and February; in Geneva, January, March, and November; in Aberdeen, October, December, and November; in Edinburgh, November, December, January; and in London, January, March, February, December, and May. As a general rule, the cold months are the most fatal, and it is surmised that this increased mortality is partly due to the want of ventilation. Nothing, it is remarked, will induce the patients in winter to allow a window or door to be opened; hence the whole ward is hot and close; while in the month of July every door and window admits fresh air day and night.

We shall next proceed to give an outline of the practice pursued in the 13,748 deliveries recorded and tabulated in the work, premising that the arrangement of labour into four classes—Natural, Preternatural, Difficult, and Anomalous, as proposed by Denman, has been adopted throughout, each, however, having certain subdivisions, to which we shall refer in the progress of our analysis.

I. Natural Labour.—This is understood to signify labour where the head presents and the delivery is completed within twenty-four hours. It is divided into purely natural labour and varieties of natural labour, the latter being subdivided into face, face to pubes, and arm-with-head cases.

Commencing, then, with purely natural labour, we have to express our approval of the careful manner in which the duties to be discharged in the management of these cases are laid down; the necessity of investigating the state of the os uteri, that of the membranes, the presentation and its relative position, the condition of the pelvic strait, that of the soft parts in the pelvis, their surfaces and secretions, the state of the bladder and rectum, the existence of morbid growths and tumours, are successively indicated as points for careful investigation. Upon this subject, however, we have no time to enlarge, and passing over the management of the first and second stages of natural labour with the single observation that we object altogether to the practice of puncturing the membranes in the first stage, as recommended by our authors in certain exceptional cases, we proceed to the consideration of the treatment recommended in the third or placental stage of labour.

We believe, notwithstanding the difference of opinion which exists, that the practice recommended by our authors is essentially the best—viz., the fundus uteri having been steadily compressed manually, and thus followed down during the descent and birth of the child, and the charge of maintaining its contraction having been intrusted to the midwife, whilst the necessary attentions are paid to the child on its birth, the medical attendant resumes the charge of maintaining uterine contraction until after the expulsion of the placenta. We believe this practice is better than the ordinary plan of applying the binder immediately after the birth of the child. We concur with our authors in thinking, that by this means the liability to hæmorrhage is lessened, the state of the uterus as to flaccidity, distension, or contraction, is more certainly determined; the detention of clots within the uterus prevented, and the abdominal parietes generally better supported. As compared, moreover, with the immediate application of the binder, we conceive that it has the advantage that it obviates the chance of overlooking insidious or internal hæmorrhage, and that it thus does away with the necessity and consequent delay of having to undo the bandage, and seek for and restore the contraction of the uterus in cases in which internal hæmorrhage has occurred.

For after pains opium is recommended, but we should prefer giving a full dose of castor oil, either with or without turpentine, when they are troublesome. We believe, however, that when care has been taken to maintain the contraction of the uterus in the manner described, they will seldom be severe, and when they do occur under such circumstances they will generally be found to be occasioned by a clot which is expelled under the action of the purgative. We hold that the employment of opium, after labour, should be as much restricted as possible, tending, as it frequently does, to constipate the bowels, disorder the secretions, impair the appetite, and disturb the sensorium.

Of the sequelæ of natural labour one of the most troublesome is sore nipples, for which a variety of applications are proposed, the favourite being a wash composed of borax, chalk, and diluted spirits. Sometimes gutta percha collodion was used so as to form a kind of flexible shield round the nipple, and sometimes weak citrine ointment; but as it is stated that every kind of unguent and lotion was used with occasionally little or no success, we venture to recommend a formula which we have employed with the greatest advantage—viz., a solution of tannic acid in glycerine, in the proportion of a scruple of the former to a drachm of the latter. With cleanliness and a frequent application of this lotion by means of a camel's hair pencil, we have seldom failed to cure the most troublesome affections of this kind.

Varieties of Natural Labour.—These comprise cases in which the head presents, and delivery is accomplished within twenty-four hours; it is subdivided, as already intimated, into face presentations, face to pubes, and arm-with-head cases.

The rule of practice in *face presentations* was to leave them as much as possible to nature, and no attempt was ever made to rectify them or to change them from face to vertex presentations. The chief care was to guard the perineum during the preternatural distension it had to undergo, so as to avert the consequent liability to rupture. Thus treated, of 40 cases in 31 the labour terminated naturally.

Face to pubes cases were treated much in the same manner, and were left entirely to nature unless delay compelled interference. Though often tried, it was not found that these positions could be rectified by manipulation, and, as in face presentations, the great point aimed at was to guard the perineum from rupture, and under this treatment of 33 cases 20 were terminated by the natural efforts within twenty-four hours.

Arm-with-head cases were not of frequent occurrence, amounting only to 16 in the 13,748 deliveries. In these cases when the arm descended before the head, which was always discovered early, when circumstances permitted, efforts were made to support it while the head descended, but never in the first stage of labour. When these efforts were not successful, and this was found to be generally the case, the labour was allowed to proceed until a delay occurred demanding interference.

II. PRETERNATURAL LABOUR.—This, we need scarcely observe, comprises all cases in which any other part than the head of the fœtus presents, and is divisible into two varieties—1st, cases in which some part of one or both inferior extremities presents; and 2nd, those in which some part of the superior extremities presents. Breech, foot, and knee presentations constitute the former; shoulder, arm, elbow, and hand, the latter.

As a general rule, it is observed that these cases require manual assistance, and adverting to them in the order of sequence above stated, it is very justly insisted upon that the diagnosis of breech cases requires much care and consideration. They may, for instance, be mistaken for cases in which the shoulder, the face, or even the occiput, when the scalp is much swollen, presents, and hence, whenever a doubt exists, great care, it is insisted upon, should be taken lest in the necessary examinations some injury should be inflicted upon the child. We will not, however, enter upon the diagnosis of these cases, as we believe it to be generally well understood; and we do not find any information given which is not to be found in our ordinary text books.

As to treatment, the following appears to have been the practice generally pursued. The patient having been placed in the same position as in natural labor, the position of the breech relatively to the pelvis was first ascertained, and if found to be presenting in the first or second position—i. e., with the back of the child to the right or left acetabulum, no interference was deemed necessary beyond attention to the perineum while the nates were passing through the vulva. This was carefully guarded whilst the breech passed, and when the feet of the child came to touch the posterior fourchette, their too sudden egress was prevented by the hand of the attendant which was unoccupied. Pursuing the practice that is usually adopted until the arms are delivered, an important rule is laid down for the purpose of averting the mal-position of the head, which often results from traction of the body of the child—viz., the separation of the chin from the sternum, and the consequent substitution of the occipito-mental for the occipito-bregmatic position of the head in the pelvis. To obviate this difficulty and the consequent locking of the head, the following practice was pursued:—with one or two fingers of the right hand against the occipital bone, the child's occiput was pressed upwards so as to cause the chin to descend lower into the pelvis. By this means the mouth of the child was brought within reach, into which the finger of the left hand was inserted, and by simultaneously depressing this and elevating the occiput, the occipito-bregmatic was made the opposing diameter, instead of the occipito-mental. Reduced to this position the delivery was readily effected by traction, solely upon the shoulders by the right hand.

We do not observe any direction for the management of breech cases when the fœtal abdomen is directed forwards, beyond those generally enforced. The principal point insisted upon is the necessity of rotating the sacrum forwards just as the breech is being born; and this rule would appear to be the more necessary, inasmuch as our authors inform us, that although the peculiar changes in the presentation described by Naegelè as occurring naturally were looked for, yet that they had not the good fortune of seeing them. We presume this refers to the supposed spontaneous rotation of the body, as of the head of the fœtus, when presenting in an anterior position; and inasmuch as this has been much insisted upon by some writers of late, it may be well to bear in mind that clinical observations do not support the accuracy of the rule.

For assisting the delivery of the breech when unusually protracted, the finger in the first place, and the blunt hook in the second, were exclusively resorted to. Never was the fillet employed, and when it happened that the head became impacted in the progress of the birth, the forceps were invariably tried before resorting to the perforator and crotchet.

In presentation of the superior extremities three indications are insisted upon:—the first being an early diagnosis, the second to turn and deliver at the proper period, and the third to eviscerate when turning is impossible. The general diagnosis of these cases need not be dwelt upon; but a circumstance is stated which may be usefully quoted—viz., that when the woman is at full term, it is often easy to diagnose a cross birth by the peculiar shape of the abdomen, which is neither so high nor so prominent as when the long axis of the child is directed from above downwards. In this case the uterine tumour is broader and flatter, not reaching much higher than the umbilicus, the fundus uteri being transversely long and straight, instead of being rounded, as it is ordinarily.

In connexion with presentations of the upper extremity, we find that fœtal evolution occurred twice; the first instance having been a male which was born at the sixth month, dead and putrid—and the second a female at the seventh month, which was also born dead. The description of the process of spontaneous evolution, as given by the late Dr. Douglas, was found to be strictly correct. The arm protruded, and the shoulder having become fixed under the pubic arch, remained in that situation until the breech of the child was born; the arch of the pubes having been the fulcrum upon which the body of the child was made to turn. All the uterine efforts would appear to have been expended in forcing the inferior portion of the trunk into the pelvis until the nates were expelled beyond the vulva.

III. Difficult Labour.—This class of labours is divided into tedious and instrumental deliveries, which we shall treat of in the order in which they appear in the volume.

Tedious Labour is described as labour at full term, in which the head presents, and the child is delivered without the assistance of instruments after the expiration of twenty-four hours. It is treated of under two heads—the first comprising those cases in which the cause of delay originates in the first stage of labour, and the second those in which it originates in the second.

Delay occurring in the first stage of labour, although less important than delay originating in the second, is yet regarded as serious, inasmuch as a lengthened first stage has always a tendency to render the second tedious. Hypersecretion of the liquor amnii, rigidity of the uterine tissue surrounding the os uteri, nervous irritability and despondency, inertia from constitutional disease, and want of power, the origin of which could not be traced, were found to be its most frequent causes; and puncturing the membranes, venesection and tartar emetic, the administration of chloroform or opium, and frictions of the abdomen, were respectively found to be the most successful means of treating it.

Delay in the second stage of labour was regarded as a matter of infinitely more importance than that in the first. It was often induced, first, by inertia occasioned by a prolonged first stage, especially if the membranes had been early ruptured; secondly, by nervous irritability; thirdly, by over-distension of the urinary bladder; fourthly, by simple rigidity of the soft parts of the pelvis, unconnected with that of the os uteri, in which case emollient enemata were found most useful; fifthly, by enormous distension of the bowels from flatus; sixth, by pendulous states of the uterus and abdominal parietes, for which the application of the binder was found useful; and seventhly, by vaginal bands and cicatrices, which, if not overcome by the uterine efforts, were divided by the scalpel. The majority of these cases, however, are treated of under the head of instrumental labours, to which we shall next proceed.

Instrumental Deliveries.—These comprise all cases in which the labour, either long or short, necessitated the employment of instruments. It is important to remark that during the mastership of Dr. Shekleton the vectis was never resorted to, and hence this variety of labour is divided into Forceps Deliveries and Crotchet Deliveries.

Forceps Deliveries.—With regard to these cases the following circumstances were considered to render the application of the forceps necessary:—first, absolute or impending danger to the mother's life; secondly, the likelihood of injury to the mother's structures; and, thirdly, the threatened failure of the fœtal circulation. Time, it may be remarked, was never taken so much into consideration when deciding upon the employment of the forceps, as the existing state of the woman's constitution and that of the child's circulation. These furnished the chief indications for their use, and whenever the case was doubtful, it was deemed the safest practice to complete delivery by their aid.

The mode of applying this instrument need not be dwelt upon; but it is important to remark that, as a general rule, the patient was first brought into a state of complete anæsthesia. Any attempt to apply the forceps in a state of *partial* anæsthesia was found to be not only difficult, but dangerous. They were first attempted to be applied in an obliquely lateral position, and if the effort at extraction failed, they were then withdrawn, and reapplied in an antero-posterior direction. Should this mode of application also prove abortive, another attempt was made in the direction in which they had originally been applied; and it is remarked that by persevering in this plan of proceeding many lives were saved, which would otherwise have been sacrificed to the crotchet. Contrary to general directions, it was by no means felt necessary to feel the ear before resorting to their use—indeed, it was seldom or ever sought for, except for the purpose of determining the position of the fœtal head; and in many cases they were employed when the head had only barely entered the pelvic cavity. Once applied, the only movement permitted was traction in one direction—viz., that of the curve of the pelvis, in which the head was placed. No seesawing or twisting movement was allowed, and it is said, that when the one steady direction by traction was maintained at intervals, the forceps never failed to complete delivery, if by any possible means the presentation could be moved through the pelvic space.

Thus employed, we may conclude the subject by observing, that of the 13,748 women delivered—irrespectively of twins—200 were delivered by the forceps. Of the children so born, 118 were males, and 82 females; 17 of the former were still-born, and 12 of the latter, whilst of the 200 subjects of forceps delivery 11 died.

Craniotomy Cases.—The circumstances which were considered to require the use of the perforator and crotchet were, first, pelvic deformity more or less; and second, disproportion between the fœtal head and the pelvic space; third, mal-position and consequent impaction of the fœtal head; fourth, bands or cicatrices in the vagina from former labours; fifth, great rigidity of the soft parts which did not yield to treatment; sixth, apoplexy of the mother; seventh, excessive action of the mother's heart; ninth, hæmorrhage; tenth, rupture of the uterus and inability to complete delivery after version.

The general rule as regards the employment of craniotomy would appear to be the following. When the diminution of space at the brim was such as to prevent the entrance of a full-sized fœtal head, as judged of by the readiness with which the sacro-vertebral angle could be reached by the examining finger, and the os uteri being sufficiently dilated, craniotomy was resorted to as soon as it was found that the pains made no impression upon the presentation, and more especially when it had been ascertained that the woman had been previously delivered by means of the crotchet.

On the other hand, should the head have entered and become impacted in the pelvis, the forceps were first introduced; and if the attempt to adjust them failed, the operation of lessening the head was undertaken without hesitation. The same course was adopted when attempts at extraction failed after the forceps had been adjusted—when the action of the fœtal heart was known to have ceased, when imminent danger occurred to the mother at any stage of labour from any cause—when delivery was impracticable by other methods, and when fibrous or bony tumours obstructed the pelvic cavity.

Of 130 mothers who were the subjects of craniotomy, 104 recovered and 26 died.

IV. ANOMALOUS OR COMPLICATED LABOUR.—This class comprehends the following varieties: first, Plurality of children; second, Prolapsus of funis; third, Hæmorrhage; fourth, Convulsions; fifth, Rupture of uterus, vagina, or both; sixth, Inversion of the uterus; seventh, Premature labour; eighth, Retained placenta; ninth, Labial thrombus, &c. We shall very briefly touch upon the practice pursued in these cases in the order here stated.

A. *Plural Births.*—Various circumstances are referred to as affording diagnostic evidence of twin cases, but all are regarded as more or less equivocal. The stethoscope would appear to be the most reliable when by its means a distinct fœtal cardiac sound is heard at opposite or distant parts of the gravid uterus. It was, however, considered but of little importance to have discovered the existence of twins before labour, inasmuch as the treatment up to the birth of the first child was precisely the same as though it were a simple case. But when the first child was born pressure was immediately made by the hand over the fundus uteri, and everything was done to accelerate the birth of the second. Hence, after a short interval, the membranes were punctured, and the fundus uteri gently rubbed. By this means uterine action was for the most part quickly restored, and the second child was speedily expelled. If, however, inertia of the uterus supervened, ergot was exhibited, and if this failed, either the forceps were applied if the head were below, or turning resorted to if above the pelvic brim.

B. *Prolapsus of the Cord.*—These cases were treated with reference to the following varieties: first, when it was found presenting previous to the rupture of the membranes; second, when it was found prolapsed in the first stage; third, when the prolapse was discovered at the commencement of the second stage; and fourth, when it was discovered during the progress of that stage.

In the first case the patient was maintained in the horizontal position until the os uteri was sufficiently dilated to admit of the operation of turning previously to the rupture of the membranes. In the second, version was also the remedy, and this was resorted to much earlier in the case of pluriparous females than primiparous females. In the third, if the head had not descended into the pelvis version was also attempted, but if it had, labour was either accelerated by the application of the forceps, or an attempt was made to raise the cord above the sphere of pressure. In the fourth, the same practice was pursued, and it was either attempted to elevate the cord above the head, or to accelerate delivery by the forceps. In short, it is observed that two rules mainly regulated the management of these cases. The first was the delivery by version in the early stage, and the application of the forceps in the later; unless, indeed, the funis was pulseless, in which case the labour was left, as much as was consistent with the mother's safety, to nature.

C. *Hæmorrhage.*—We need scarcely premise that this is treated of under the two forms of accidental and unavoidable hæmorrhage. The causes of the former were generally difficult to discover, but when discoverable were found to consist of falls, blows, over exertion, strong mental emotion, or some indiscretion. When cases came under treatment, before the os uteri had commenced to dilate, the horizontal position was enjoined, and a cool apartment, cold applications, cold acidulated drinks, and acetate of lead and opium were prescribed.

The diagnosis between this and the unavoidable form of hæmorrhage was mainly determined by the constant oozing of blood during the intervals of the pains and its cessation upon their accession, and by the absence of the placenta from the neighbourhood of the os uteri, the cervix uteri in these cases conveying to the finger a peculiar feeling of elasticity, which contrasted strongly with the soft and doughy feel which was experienced when the placenta was attached to a greater or less extent of its surface.

As to the treatment of these cases, the plan was to rupture the membranes as soon as possible, and if the os uteri were not sufficiently dilated to admit of this operation, ergot was given. Generally on the discharge of the liquor amnii the hæmorrhage ceased, and the pains set in with great vigour and frequency, otherwise ergot was again given, and if the bleeding continued after the head had entered the pelvis, either the forceps or the perforator were resorted to, according as the child was believed to be living or dead. In one case only was the tampon introduced for the purpose of hastening the dilatation of the os uteri so as to admit of the rupture of the membranes.

Unavoidable hæmorrhage is subdivided into two varieties, according to the greater or lesser extent of the placental attachment over the os and cervix; the one being called complete, the other partial placenta prævia—the former of course being infinitely the more dangerous variety.

The diagnosis of these cases was always easy—the occurrence of hæmorrhage during the period of the pains rather than during their cessation—the sensation given to the examining finger by the spongy placenta as distinguished from the elasticity of the amniotic fluid, and the inability to distinguish the fœtal presentation in cases of placenta prævia, afforded adequate data for diagnosis.

In the partial variety of unavoidable hæmorrhage much the same practice was pursued as in cases of accidental hæmorrhage. The membranes were early punctured, and the case was permitted to proceed as naturally as possible; but when the os uteri was undilated the tampon was applied with the best results. In the complete variety of unavoidable hæmorrhage the tampon was at once introduced and retained until the os uteri was sufficiently dilated to admit of the introduction of the hand and the operation of turning. Subsequently we learn that the tampon usually employed was a sponge wrung out of warm water and smeared over with an unguent.

D. *Convulsions* are divided into apoplectic and hysterical, the latter, it is stated, being extremely rare. The treatment of the former is considered with reference to, their prevention as well as cure, and when patients were found labouring under anasarca, albuminuria, headache, or dizziness, before labour, the practice was to purge them freely and repeatedly with hydragogue cathartics; maintain a horizontal position in a cool ward, and allow none but the mildest and lightest nutriment.

With reference to the relations of albuminuria to convulsions, it was found that in nearly all the cases that came under treatment anasarca and albuminous urine were present to a greater or less degree, yet that convulsions did occur, and even in the most violent form, when neither anasarca nor albuminuria could be discovered.

The curative treatment consisted in free bleeding and purging as soon as the convulsion ceased, and during its continuance preventing the patient, as much as possible, from injuring herself or biting her tongue. Enemata of turpentine, or castor oil and assafœtida were found useful, and if the convulsions returned, shaving the head, cold lotions, and tartar emetic, with opium, were prescribed. Chloroform was seldom given, and consequently no inference could be drawn as to its efficacy. If the head were within reach the forceps were applied, but whenever the severity of the case was great perforation was unhesitatingly resorted to. Of the 63 so treated 13 died, or about 1 in every 5.

E. *Rupture.*—Including under this head cases in which the breach of continuity was confined either to the uterus, to the vagina, or both, 17 occurred, of whom 1 only recovered. In these the symptoms of impending rupture laid down in books were not always observed, and in one the event occured without any premonitory symptom whatever. Vomiting during the second stage of labour was always considered a suspicious symptom, especially when this had been severe or prolonged, and whenever pain, fixed and increased upon pressure, was referred to the pubes during the expulsive stage, that stage having been severe and protracted, the indication was to deliver as soon as possible by the means best suited to the particular case. The symptoms of the actual occurrence of rupture were more constantly those which are usually observed, but the collapse varied much in degree, and the sensation of something having given way was not always observed. The treatment, after due attention had been given to the state of prostration and to the delivery, was directed to the subsequent peritoneal inflammation certain to take place, and mercury and opium were chiefly employed, the latter very freely.

F. *Inversion of the Uterus.*—This accident occurred but once during the seven years. It occurred in a primiparous patient, nineteen years of age, after an easy labour of six hours' duration. The gentleman in attendance, after having tied and separated the funis, had maintained the contraction of the uterus with the hand above the fundus for a quarter of an hour, when, finding a tendency to draining, he increased his pressure, but not more than was usual. Whilst doing so the uterus was felt suddenly to yield and recede from his grasp, and he immediately saw it expelled from the vagina an inverted mass, with the placenta still attached. The organ was immediately replaced, the placenta having been previously detached, and the patient made an excellent recovery.

G. *Premature Labour.*—The chapter devoted to this subject is chiefly of a statistical nature, but contains a few practical observations upon the treatment of abortion, from which we learn that when a patient entered the hospital with hæmorrhage in the early months, two indications were kept in view—first, to endeavour to preserve the ovum, and secondly, finding this impossible, to effect its complete discharge as soon as practicable. The first indication was fulfilled by rest, a cooling regimen, acidulated drinks, acetate of lead with opium, and the other means generally employed for restraining hæmorrhage; the second, by the introduction of the sponge tampon, as already described, and the administration of ergot in ten-grain doses every two hours. We find no reference to the use of sponge tents in the treatment of abortion, and the patients would appear to have all done well without them.

H. *Retained Placenta.*—According to established practice, this is treated of under three heads having reference to the cause of the retention—viz. simple inertia, irregular contraction, or morbid adhesion. For the first, the practice was, if possible to make the uterus expel its contents before introducing the hand for the purpose of removing them; to effect this, cold applications, pressure, and frictions over the fundus were resorted to, and these failing, the hand was introduced. When the placenta was retained from irregular uterine contraction, whatever the character or variety of the contraction, such cases were all treated by the introduction of the hand. As regards morbid adhesion, artificial detachment was in all cases practised, and great care was taken to detach the placenta as completely as possible; but if this were found impracticable without the exercise of undue force, as much was removed as had already yielded, and the remainder was left behind.

We have thus passed over in rapid review some of the more practical portions of the work, and have endeavoured to give in a condensed form the leading rules of practice which are at present observed in the Dublin Lying-in Hospital. Regarding it as one of the great centres of obstetric learning in this country, we have, in deference to this consideration, ventured upon little more than an analytical notice of the volume, and if in the analysis we have submitted to the reader he is enabled to discern but little that is original or novel in the practice, we would venture to observe that an important inference is deducible from the fact—viz. that it is not from such institutions that the many innovations and reputed improvements upon established practice which are to be found in the current literature of the day for the most part date their origin. He who has the responsible management of a lying-in hospital in which nearly 2000 women are annually delivered, can have little time or taste for engaging in abstract or speculative disquisitions, and must see the operations of nature conducted upon too large a scale to seek to control them by unnecessary artificialities. We accordingly do not find in the work any proposal for the conversion of natural into preternatural labours—no proposition for the abolition of craniotomy from obstetric practice, no rules of treatment founded merely upon speculative considerations. Hence, indeed, the great value of such works as the present: they reflect, as it were, a faithful image of the realities, dangers, and responsibilities of obstetric practice, as distinguished from its idealities—they show how these difficulties are capable of being met, how far successfully, how far otherwise, and they offer equally encouragement and consolation to all who are engaged in this arduous undertaking—encouragement, by showing how often the greatest difficulties may be successfully overcome, and consolation under failure, by showing how often the best directed efforts may prove abortive and unavailing.

We have ventured in the introductory observations of this article to point out what appear to us to be two great drawbacks to the practical value of this work, the one being a too great deference to artificial classifications, the other a redundancy of statistical calculations in reference to facts of little practical import. The former deprives many of the subjects treated of of their full value and interest, the latter sorely perplexes and confounds the reader who seeks to elicit from its pages the *point* and *spirit* of the work. We trust that the future historian of the practice of the Dublin Lying-in Hospital will guard against both these tendencies, and that he will bear in mind the excellent admonition of the late Dr. Gooch to those who are engaged in the study of midwifery—advice which appears to us to be as applicable to authors as to students :—"Watch cases attentively, and take notes of their important particulars, and not lengthy notes containing *a diffuse description of unimportant trifles*, which from the time that they occupy, will soon cease to be written, and if written, are sure never to be read; but a short description of the leading circumstances, with an equally short mention of the reflections which they suggest."

REVIEW X.

The Works of John Hunter, F.R.S.; with Notes. Edited by JAMES F. PALMER. Four Vols. 8vo. Illustrated by a Volume of Plates in Quarto.—*London*, 1835.

THE late re-interment of the remains of John Hunter has naturally recalled his memory to the public mind, and more especially and more vividly to the mind of that profession which he did so much to enlighten and to elevate. The publication of a collected edition of his works, now more than twenty years since, accompanied with introductory essays and explanatory notes, by men eminently qualified for the task, might well have been supposed to have made his labours thoroughly familiar to the profession, and to have rendered superfluous any further attempt to exhibit the greatness and the peculiarity of his genius, to vindicate his opinions from misrepresentation, or to point out the benefits which he has conferred on various departments of medical science and practice. According to our observation, however, these effects have been slow in following. In a general way, the name of Hunter is invested with all imaginable honour; but his views are still very imperfectly understood; several important results of his inquiries are frequently ascribed to others; he is praised for discoveries that he did not make, and censured for tenets that he never held; and, on the whole, if the broad question were proposed, "What did John Hunter achieve for science to entitle him to the lofty position which is so universally conceded to him?" we apprehend that few even of his professed admirers would be prepared to give a categorical answer. Under these circumstances, we are induced to deviate somewhat from our ordinary track as reviewers of cotemporaneous authors, and to give as distinct an outline as we are able of Hunter's doctrines and opinions, and of the scope and results of his researches.

The particular discoveries of Hunter in anatomy, physiology, zoology, pathology, and surgery, would afford sufficient ground for assigning to him a very high rank among the contributors to each of those branches of science; and there certainly never was any one man who contributed so much to them all, taken collectively. But it was not on his particular discoveries, great as they were, that his claims to our admiration and gratitude principally rest; his vast and profound generalizations from the observation of innumerable facts, throw both his own and all other individual discoveries into the shade, and entitle him to rank as one of the greatest of inductive philosophers, and as the first who brought the inductive philosophy to bear efficiently and profitably on the science of life and the study of disease. There is one great department of his labours which, though in extended and beautiful relation to all the rest, and shedding over all a broad and majestic light, stands in some measure apart, as a thing altogether peculiar in the history of human research and human industry, and as the work which bears the strongest impress of his individual hand, and in which the character of his mind is most singularly manifested in all its power and all its comprehensiveness—we allude to his museum.

We propose, therefore, to dwell briefly, first, on Hunter's general doctrines and opinions; secondly, on his particular discoveries; and, thirdly, on his museum.

In order to arrive at any just estimate of Hunter's general doctrines, we must have acquired an accurate conception of his views of life. These views were for a long time much misrepresented and misunderstood; and even to this day we too frequently find the name of Hunter associated with a mysterious hypothesis of the vital principle, which is supposed to have pervaded, and in some degree obscured, all his reasonings. Such a notion, however, can be entertained only by those who have no comprehensive acquaintance with his writings, and the fact of their prevalence shows how little familiar those writings are to a majority of our profession. Hunter, though he would himself probably have laughed heartily at the suggestion, had in him several of the elements of a poet, and among these, a strong tendency to figurative modes of expression; but there was another element of the poetic character which he certainly did not possess, namely, a felicitous flow of language; on the contrary, his style of writing, generally more or less involved and embarrassed, was sometimes so obscure as to be barely intelligible. We quote a curious passage in which he falls into this peculiar kind of phraseology, and at the same time endeavours to account for and to excuse it:—

> "This disposition to coagulate when out of the vessels, or when retained in them without motion till the consciousness of the use of motion, and of course of fluidity, is lost, is one of the effects of the life of the blood. I have used the word *consciousness* because we have no language existing answerable to all my views of the animal economy, and to coin words would not answer the purpose, because then I must have a dictionary of my own. I have not a word for expressing the cause of those actions which take place in the body, as if it was conscious that such and such things were going to take place. There are actions in the body which come nearest to consciousness of the mind of anything that I can conceive, and therefore I make use of this word, but it is commonly applied by philosophers only to the mind."[*]

Taking into consideration these unfortunate peculiarities in Hunter's manner of expressing his thoughts, it is not to be wondered at that the analogical illustration of his meaning should sometimes have been mistaken for the meaning itself,[†] and that many should have derived from a partial and inattentive perusal of his works, very erroneous impressions of his opinions. Of all his writings, the 'Treatise on the Blood, Inflammation, and Gun-shot wounds,' is the one that has been most generally read, and from which, therefore, the prevalent notions of his opinions have been chiefly derived; but this abounds, more than any other, in the figurative expressions and obscurities of style just adverted to, and is therefore especially liable to be misconstrued by the reader who is unacquainted with the general scope of his writings. Again, the 'Lectures on the Principles of Surgery,' in which he enters more explicitly than elsewhere into his views of the nature of life, were not published at all till within the last thirty years, and have probably never yet been perused by many who affect to be offended at his "hypothesis." In the introductory portion of these lectures, there is a passage which we think it well to quote, because it is very directly to the purpose, and shows at once how widely those have erred who have attempted to father upon John Hunter a doctrine in which the vital principle is endowed with a kind of personality and intelligence; though in truth if he had entertained such a notion, any one who is at all conversant with physiology will not require to be informed, that it would have been no peculiar notion of Hunter's, but one that had floated through the speculations of physiologists from the days of Aristotle to those of Stahl.

> "Animal matter is endowed with a principle called in common language, life. This principle is, perhaps, conceived of with more difficulty than any other in nature, which arises from its being more complex in its effects than any other; and it is therefore no wonder that it is the least understood. But although life may appear very compounded in its effects in a complicated animal like man, it is as simple in him as in the most simple animal, and is reducible to one simple property in every animal. I have observed that animal matter may be in two states; in one, it is endowed with the living principle, in the other it is deprived of it. From this it appears that the principle called life cannot arise from the peculiar modification of matter, because the same modification exists where this principle is no more. The matter, abstracted from life, appears at all times to be the same, as far as our senses and experiments carry us. If life arose out of this peculiar modification, it would not be destroyed until the modification was destroyed, either by spontaneous changes or by some chemical processes; and were it destroyed by the last, it might sometimes be restored again by another process. Life, then, appears to be something superadded to this peculiar modification of matter; or this modification of matter is so arranged that the principle of life arises out of the arrangement, and this peculiar disposition of parts may be destroyed, and still the modification from which it is called animal matter remain the same. If the latter be the true explanation, this arrangement of parts, on which life should depend, would not be that position of parts necessary to the formation of a whole part or organ, for that is probably a mechanical, or at least organical arrangement, but just a peculiar arrangement of the most simple particles, giving rise to a principle of preservation; so that matter so arranged could not undergo any

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[*] Lectures on the Principles of Surgery, p. 236.

[†] Even Abernethy, from whom such mistakes were little to have been expected, has been at the trouble of defending and enforcing what he considered as Hunter's view of the identity of the vital and electric forces; whereas Hunter assuredly never entertained any such view.

> destructive change till this arrangement were destroyed, which is death. This simple principle of life can with difficulty be conceived, but to show that matter may take on new properties without being altered in itself as to the species of matter, it may not be improper to illustrate this idea by such acquirements in other matter. Perhaps magnetism affords us the best illustration we can give of this. Iron appears at all times the same, whether endued with this property or not; magnetism does not depend on the formation of any of its parts. A bar of iron without magnetism may be considered like animal matter without life; set it upright and it acquires a new property of attraction and repulsion at its different ends. Now, is this any substance added, or is it a certain change which takes place in the arrangement of the particles of iron giving it this property?"*

It will be observed that there is only one positive affirmation respecting life in the whole of this passage—namely, that it is universally reducible to one simple property—a position which, in the present day, will be questioned by many, but hailed by some as the foreshadowing of a great generalization to which we are gradually approaching. All the rest is here advanced by Hunter as mere conjecture. To this we attach no more importance than to any other conjecture; we merely adduce the passage as conclusive evidence that Hunter entertained no definite opinion as to the essential nature of life, but was content to acknowledge his ignorance upon the subject.

There is another point of view, however, in which this passage is not without interest. Hunter here expresses the notion that matter may acquire new properties in virtue of molecular changes taking place within itself, apart from any perceptible alteration in its composition; in other words, the notion of what is now called "allotrophy;" and he conceived that there might be a kind of vital, or rather vitalizing, allotrophy, by dint of which animal matter became living matter.

Hunter followed Harvey in the belief that the principle of life existed in the impregnated ovum prior to the formation of any part of the future organism, and they both attributed the power by which the impregnated egg resists putrefaction to the presence of this principle. But Hunter, still making the egg the subject of experiment, discovered a new power, at the same time that he obtained an additional confirmation of the preceding. He found that the impregnated egg would resist a greater degree of cold than the unimpregnated, without being frozen; but that, when once frozen, and thus deprived of its vitality, it had no greater power of resisting putrefaction than the unimpregnated egg. Like Harvey, also, Hunter believed that the living principle displayed its activity more especially in the blood; and this led him to a train of investigation on the vital properties of the blood, to which we shall presently have occasion more particularly to advert. There is another point which we have to notice respecting Hunter's general views of life. He was fully aware of a very important truth which had escaped preceding physiologists—namely, that life was not a self-acting power, but one which is called into action by appropriate stimuli.

> "I shall carry my ideas of life further than has commonly been done. Life I believe to exist in every part of an animal body, and to render it susceptible of impressions which excite action; there is no part which has not more or less of this principle, and consequently no part which does not act according to the nature of the principle itself, and the impressions thence arising, producing thereby infinite variety both in all natural and diseased acts."†

The truth thus plainly announced by Hunter was, however, lost sight of by succeeding physiologists, or remained without any general application, till it was taken up by Tiedemann and others, and brought to bear luminously on every department of physiology.

On the whole, then, it appears that the positive tenets of Hunter respecting life in the abstract, were few and simple, and that of these the only two peculiar to himself were, that relating to its power of resisting temperature, which he proved by direct experiment, and that relating to its power of reaction on the application of stimuli, which is now universally recognised as one of the most important principles in physiology.

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* Lectures on the Principles of Surgery, chap. ii.

† Treatise on the Blood, &c., p. 2–3.

In his inquiries into the effects of vital causes, Hunter discarded all hypothesis, and starting from the plain and intelligible postulate, that as the vital phenomena are not explicable from the ordinary laws of matter, they must acknowledge some laws different from these, he devoted his energies to the elucidation of the unknown laws of life. In entering on this pursuit, his sagacious and comprehensive mind soon perceived that there were as yet no sufficient data for his investigations, and that general laws were not to be unveiled by studying only their partial applications. The common properties of life being manifested through such an infinite variety of organic forms, he felt the necessity of becoming acquainted with all these forms, or at least with as many of them as possible; and as the phenomena of life are exhibited as modified by an endless variety of external conditions, he felt the necessity of becoming acquainted as far as possible with the nature and influence of those conditions. The profound recognition of such truths as these was, with Hunter, the starting point of all his laborious thinking, and endless observation, and diversified experiment. Inspired with a genius altogether peculiar and original, he entered on the domain of life as on a region almost unknown, which it was his mission to explore, and with unwearied step he tracked the manifestations of the vital force through all the developments and activities of organized bodies, through all the apparent anomalies of organization, and through all the modifications of function in disease, as well as in remedial efforts and processes.

Great discoveries in physiology had been made before the days of Hunter; but they all stood aloof and isolated for want of general principles to connect them. Even the grand discovery of the circulation, though its influence is now everywhere so perceptible, had remained comparatively barren of results. Several hypotheses had been maintained respecting the vital power; but they all aimed rather at discovering what it *was* than what it *did;* and their authors accordingly wasted their energies in vain speculations. It never seems to have occurred to any of them, that the laws of life might be determined, though its essence should for ever remain a mystery. Now, this is precisely what did occur to Hunter. One step in the right direction had indeed already been made by Haller, who had established the existence of two distinct modifications of vital power—namely, sensibility resident in the nervous, and irritability in the muscular fibre; thus suggesting the method of investigating the phenomena of life, as influenced by the tissue in which they are manifested. But nothing had yet been done in the way of comparative physiology, which was a name and a thing unknown until the genius of Hunter called it into sudden and gigantic existence, and in the space of one short life enriched it with such an accumulation of facts and inferences that even now, more than half a century after his death, we are far from having arrived at the full understanding and appreciation of his labours in this department.

As before observed, Hunter was not the first physiologist who held that the vital principle resides in the blood; though, in the limited extent of his own reading, he doubtless believed himself to be so. Harvey had entertained the same belief, but he merely speculated upon the subject, and bewildered himself in a maze of confused and contradictory conjectures. Thus, at one time he considers the blood as the instrument of the soul; at another, he makes the soul an act of the blood; then the blood itself is the soul; then the soul is the *calidum innatum;* and, lastly, the soul is held to be pre-existent, and to create both the blood and the *calidum innatum.*

Hunter went to work in a very different spirit. Convinced of the fundamental truth of the life of the blood, he engaged in a most comprehensive and searching experimental inquiry into the properties of this fluid, whether in health or disease, in motion or at rest, contained within the vessels or extravasated from them. But the time was not yet ripe for many of the conclusions at which physiologists have since been enabled to arrive. Organic chemistry was in its infancy, if, indeed, it can be said to have existed at all; and the microscope was yet a rude and imperfect instrument, available but to a small extent for the purposes of scientific research. Hunter's view of the coagulation of the blood as a vital act has never been substantiated, but is rather discountenanced by more recent inquiries. His belief that the coagulated blood on the surfaces of wounds might become, like the lymph, the seat of the formation and inosculation of vessels, doubtless tended to confirm him in the notion of the vital character of the coagulating process; and this supposed fact would, if true, sufficiently prove the accuracy of his opinion; but his belief in this respect seems to have been the result of imperfect observation, nor does there appear any reason to suppose that such formation of vessels in coagula ever really takes place.

One very important conclusion was certainly arrived at by Hunter with regard to the blood—namely, that its vital properties reside principally and primarily in the colourless portion. This he established by the observation, that in the embryo of mammiferous animals the red particles are entirely absent, and that the blood is consequently in the same apparent condition as in the invertebrata when fully developed. So late as the year 1832, this fact was brought forward by MM. Delpech and Coste as a new discovery, doubtless in ignorance of Hunter's priority.

On the whole, Hunter's researches into the vital properties of the blood are perhaps more valuable as suggesting important points of inquiry, than as establishing valid conclusions respecting them; to which, as already remarked, it was simply impossible that he should have attained, in the absence of adequate implements and means of investigation. But in this their suggestive tendency, they cannot be too highly estimated; and, whether considered in relation to their immense extent, the penetrating sagacity with which they were conducted, or the important pathological inferences immediately deduced from them, they must be admitted to have formed one of the most remarkable series of investigations in which any single inquirer was ever engaged. Hunter, then, is unquestionably entitled to the merit of having been the first to promulgate a rational and consistent doctrine of the life of the blood, and to lay a broad and sure foundation for future inquiry into its vital properties and uses.

Among the vital phenomena connected with the blood, and the vessels which contain it, those of inflammation naturally presented themselves to a mind like Hunter's as a highly interesting subject of inquiry. On this subject all had been till his time mere conjecture, and scarcely a single really philosophical observation had been made upon it. Hunter, as is well known, referred inflammation mainly to an increased action of the vessels of the part; but this, not as many have supposed, in the sense of an increased contraction, but in the sense of an action the result of which is an increase of their capacity.

> "We must suppose it an action in the parts to produce an increase of size to answer particular purposes; and this I should call an action of dilatation, as we see the uterus increase in size in the time of uterine gestation, as well as the os tincæ in the time of labour, the consequence of the preceding actions, and necessary to the completion of those which are to follow."*

It was probably his notion of common, or, as he first called it, healthy inflammation, as being essentially a restorative and not a morbid process, that led him to this view of the action of the vessels. But whether Hunter were right or wrong as to the nature of the action going on in the vessels, and whether we regard their dilatation as an active state induced by an effort of the vital force, or a passive one resulting from diminished contractility, the fact of their dilatation admits of no dispute. Nor is there so much practical difference as might at first be imagined between the views of Hunter and those of Vacca, Allen, and their followers; for Hunter distinctly states his belief that both the muscular contractility and the elasticity of the vessels are diminished.

> "The power of muscular contraction would seem to give way in inflammation, for they certainly dilate more in inflammation than the extent of the elastic power would allow; and it must also be supposed that the elastic power of the artery must be dilated in the same proportion." †

This is very badly expressed, but Hunter evidently means to say that the muscular power is diminished, and that the elastic coat of the vessels is dilated beyond what its usual elasticity would admit of. All the hypotheses seem, therefore, virtually to agree as to a diminution of contractile power in the vessels; only those based on Vacca's seem to regard this in the light of a sort of paralysis and diminution of vital power, while that of Hunter refers it to "an action of dilatation," which appears to us merely a strange mode of expression for an organic increase of size, or actual growth of the vessels.

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* Treatise on the Blood, &c., p. 394.

† On the Blood, &c., p. 394.

On the question, whether there be an increase in the number of the vessels of the inflamed part, or whether the appearance of increased vascularity depends merely on the dilatation of the ordinary vessels, and the unusual quantity of blood they contain, Hunter professes himself uncertain. But in considering the state of the vessels, he did not lose sight of the state of the blood itself; and he arrived at the conclusion that there is a tendency to separation in its constituent parts, and a disposition among its red particles to cohere and separate themselves from the general mass.

> "In all inflammatory dispositions in the solids, whether universal or local, the blood has an increased disposition to separate into its component parts, the red globules becoming less uniformly diffused, and their attraction to one another becomes stronger, so that the blood, when out of the vessels, soon becomes cloudy or muddy, and dusky in its colour, and when spread over any surface it appears mottled, the red blood attracting itself and forming spots of red. This is so evident in many cases that it is hardly necessary to wait till the whole coagulates to form a judgment of it. When the blood has not an inflammatory disposition, the stream has a degree of uniformity and transparency in its appearance; but it is only an eye accustomed to it that can make this distinction."*

The accustomed eye of Hunter here led him to conclusions which the microscopic eye of more recent observers has confirmed. The only point connected with the phenomena of inflammation in which Hunter was in error as to the facts of the case, was his belief that the blood moved quicker than usual in the inflamed vessels; whereas it is now well known that it moves much slower, and sometimes ceases to move altogether. The fact, however, that the velocity of the blood is really increased in the vessels immediately adjacent to those which are the seat of inflammation, renders this error of the less importance.

Setting aside this one error, then, it would appear that Hunter's actual conclusions respecting the proximate cause of inflammation were, that the vessels of the part are dilated, and contain more blood than usual; that the blood has a tendency to resolve itself into its constituent parts, and that the red corpuscles acquire an increased attraction for each other. On the whole, we believe this will be found to be pretty nearly the amount of our positive knowledge on the subject up to the present time.

A very important feature in Hunter's view of inflammation, consists in his division of it into the *adhesive*, the *suppurative*, and the *ulcerative*, a division founded on the most scrutinizing observation of its phenomena, whether in the progress of disease, the limitation of morbid processes, or the reparation of injuries.

It seems not a little singular, and it is moreover much to be regretted, that Hunter did not include mortification as one of the terminations of inflammation, and that a subject so congenial to the general tenor of his inquiries as the state of the vessels in gangrene, and the processes instituted by nature for the separation of sphacelated parts, should have received so small a share of his attention.

But Hunter's discoveries were not confined to placing adhesion, suppuration, and ulceration in their true practical relations to inflammation. He investigated each of these processes minutely, and entered fully into their pathological details. His observations on suppuration were regarded at the time, and for a long time after, as proving that pus was a secretion from the inflamed vessels; but the recent progress of inquiry is unfavourable to this conclusion, and we have been led back, by one of those singular retrogressions which sometimes occur in the history of science, to a view of the subject which approaches more nearly to that of Boerhaave and some of his followers, than to that of Hunter. But the observations of Hunter on the organization of lymph in the adhesive process stand fast, allowance being of course made for an unacquaintance, which was then universal, with the intervention of cells in the earlier stages of the formation of parts. And he was the first to establish the true nature of the ulcerative process, which, from the times of Galen to his own, had been regarded as an erosion of the solids by the fluids of the part; but which he proved to be effected by the action of the absorbent vessels. He was the first, also, to show the manner in which nature operates in the formation of granulations, and the filling up and cicatrization of abscesses, and other solutions of continuity which are not directly repaired by the adhesive process.

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* Lectures on the Principles of Surgery, p. 235.

The mind of Hunter was too comprehensive to allow him to rest contented with views, however luminous, of so important a process as that of inflammation, till they were extended to the modifications which it undergoes, as influenced by the peculiar vital endowments of the different tissues in which it is developed. The study of general anatomy had not yet taken the definite form, or the particular designation, which it was destined shortly after to assume in the hands of Bichat; but the principle prevails throughout the writings of Hunter, and it is impossible to compare these with the writings of the French anatomist, without arriving at the conclusion that Bichat either borrowed very largely from Hunter without acknowledgment, or that he had no acquaintance with his works; the latter would be the more charitable conclusion, though we fear the former is by far the more probable one.

Hunter, however, fully recognising, if he nowhere formally announces, the "doctrine of the tissues," carefully describes the phenomena and effects of inflammation as exhibited in the mucous, serous, and synovial membranes, in the cellular and cutaneous textures, in the osseous and fibrous structures, and, in fact, in every tissue throughout the body.

He recognised, also, every variety of inflammation with which we are now acquainted —acute and chronic, healthy and unhealthy, phlegmonous and erisypelatous, common and specific. He describes it as modified by the gouty and scrofulous diatheses. He was familiar with the fact, that erysipelatous inflammation invades other textures beside the cutaneous, and he well knew the tendency of this kind of inflammation to become indefinitely diffused in the absence of that adhesive disposition by which healthy inflammation sets limits to its own progress. The particular phenomena attending inflammation of the veins were altogether unknown, till Hunter gave a full and accurate account of them; notwithstanding which, the subject seems to have been nearly as much neglected as before, till the publication of Mr. Hodgson's treatise 'On Diseases of the Arteries and Veins,' in 1815, which drew more general attention to it.

Not only did Hunter thus variously illustrate the processes of inflammation in their local effects, and in their modifications in different tissues. His observations on the constitutional disturbances arising from them were of at least equal value. That various febrile and nervous disorders attended local inflammation, wounds, and injuries, must have been a familiar fact from the earliest times; but Hunter was the first to describe these distinctly and faithfully, to associate particular forms of local and constitutional disease, to expound the sympathies of different organs and systems on which they depend, and to bring physiological reasoning to bear directly and profitably on the phenomena of disease. All these topics have now become so much a matter of course, that it is not easy to imagine a period of our art, comparatively recent, in which they were either altogether neglected, or made the subject of crude hypotheses, founded merely on conjecture, or hasty and imperfect observation. Yet such assuredly was the state of things when the genius of Hunter arose, and shed a new and permanent light on the progress of surgery. The late Mr. Abernethy, who lived when these great changes had only just been brought about, and who, having been himself an ingenious and successful labourer in this newly-opened field of inquiry, must be esteemed a highly competent witness, thus expresses himself on the subject:

> "An evil seems to me to have arisen from the artificial division of the healing art into the medical and surgical departments. This division has caused the attention of the physician and surgeon to be too exclusively directed to those diseases which custom has arbitrarily alloted to their care. The effects of local disorders upon the constitution have, in consequence, been too little attended to; and, indeed, I know of no book to which I can refer

> a surgical student for a satisfactory account of those febrile and nervous affections which local disease produces, except that of Mr. Hunter."*

Indeed, the very term "constitutional" appears to have come into everyday use, in consequence of the general recognition of truths which Hunter was the first to explain.

> "In his pathology," says Mr. Green, "Hunter, by contemplating life as an agency working under the control of law, remained true to the principle already secured in his physiology; and it enabled him to regard the living body in disease, no less than in health, as a living whole and an organic unity. Thus we find him not only recognising the living body as a constitution by virtue of which it forms a system of interdependent parts, and of balanced forces mutually reacting and combining to one end; but also raising into notice the fact that these powers may and do exist in various degrees of intensity, and relative subordination, the result being, in each instance, the constitution of the individual, with its marked peculiarities. And if, from this vantage ground, he was led to determine the pathological significance of the terms 'susceptibility,' 'disposition,' 'irritability,' and the like, and to penetrate the nature of 'hereditary tendencies;' it also induced him to devote a large portion of his lectures to a consideration of sympathy, the term being intended to express the community, and as it were consent of feeling and action, which preserve the bond of interdependence in all the parts and actions of the living body in their conspiration to an organic whole. He saw it was from a knowledge of morbid sympathy that we are enabled to anticipate the immediate and remote effects of injury to the living frame; and that it is under the conditions of sympathy that we have to study the nature and end of constitutional irritation in its various forms. And as many of the actions excited by sympathy are for the purpose of effecting processes which tend to the repair of injuries, and to the removal of disease, the principle which he establishes supplies an intelligible meaning to the so called *vis medicatrix naturæ*, as the law of integrity, or the ever-present tendency to integration, which, in all life, having produced a whole, ever tends to preserve and restore that which it has produced."†

We have quoted this passage because, though somewhat peculiar in its phraseology, it appears to us to be very full of thought, and to take in much of the general scope of Hunter's views of the animal economy.

Considering, then, that we owe to Hunter the true knowledge of inflammation throughout the wide range of its phenomena, and the original and masterly illustration of the constitutional effects of local disease and injury, it is evident that he is justly entitled to be regarded as the father of pathological surgery, and as the first who raised surgery from the rank of an art to that of a science. If this had been all he ever did, what name in the annals of medicine could have had a prouder claim to distinction?

The first approach to an accurate knowledge of the anatomy of the lymphatic system, and the establishment of its true functions, are due to the conjoint labours of William and John Hunter. The existence of lymphatic vessels had long been known: and somewhat earlier than the time of the Hunters, conjectures had been thrown out as to their absorbent action; but their relation to the lymphatic glands, and their connexion in one entire system permeating the whole animal frame, and ministering to an important general function, were brought to light for the first time by the researches of these distinguished brothers.‡

John Hunter showed that an injection of mercury, forced into the substance of the lymphatic glands, would fill not only the glands, but all the lymphatic vessels proceeding from them; and it was his intention to have traced the lymphatics in this manner throughout the body, and to have given a complete description and figure of the whole absorbent system; but he was prevented, by a long illness, from prosecuting his design. His brother William bears witness to these facts, and Cruikshank, who perfected the anatomy of the absorbent system, distinctly acknowledges the priority of John Hunter. An immediate application of the physiological views developed by his own and his brother's researches was made by John Hunter in his beautiful theory of the action of the lymphatics in balancing that of the nutritive vessels, and modelling the different organs of the body into their just shape and proportions. Still more important was the use which he made of these discoveries in explaining the process of ulceration, which constitutes, "perhaps one of the most successful efforts that has hitherto been made by any pathologist to apply the knowledge of a living function to the explanation of morbid appearances."*

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* On the Constitutional Origin and Treatment of Local Diseases, p. 1.

† Address delivered before the Royal College of Surgeons, March 29th, 1859, as reported in the Lancet.

‡ It must be confessed, however, that they fell into a great error in denying the absorbent power of the veins, which was then generally admitted; and that they thus made a step backward, as well as one in advance.

Among the general subjects on which Hunter exercised his versatile powers was the venereal disease. In the investigation of this, however, he fell far short of the excellence which he attained on most other subjects involving the illustration of general principles, and the application of vital laws to the elucidation of morbid actions. Still, he was undoubtedly the first who cultivated this subject in a scientific spirit; and there is one point of view in which his treatise may be considered as particularly valuable—namely, as the earliest attempt to illustrate the laws of morbid poisons. Many of Hunter's theoretical views of syphilis have been proved, by more recent observation, to be erroneous, such as that of its invariable tendency to go on from bad to worse, and that of its incurability by any other than a specific remedy. But Hunter did not excel in pure theory; his mind was not of a logical cast; and his hypothetical reasoning was often—nay almost always—vague and inconclusive. His *forte* lay in seizing with amazing sagacity on those points of a subject which afforded scope for anatomical demonstration or physiological experiment—in bringing to bear on such points a most exact and comprehensive knowledge of comparative structure—and in devising, with the most subtle ingenuity, experiments directly adapted to solve the point at issue. The venereal disease was not a subject which afforded much scope for these qualities, and therefore, as truly remarked by Dr. George G. Babington in his preface to the treatise, it was less adapted than many other subjects to the peculiar genius of John Hunter.

It is not to be denied, however, that Hunter brought to the illustration of the venereal disease the same laborious observation of facts which characterizes all his works. His delineation of its local and constitutional effects is given with a masterly hand, and his precepts for its treatment have in the main held their ground amid all subsequent fluctuations of opinion. Before he took the subject in hand it was quite out of the pale of scientific inquiry, and the treatment of the disease was altogether empirical and unsatisfactory. He left it enriched with copious and correct observations, and with sound views of treatment; so that his treatise on this subject, though it will not bear a comparison with his greater works, is still of no ordinary merit, and must be regarded as the production of a very great pathological and practical surgeon.

The observations of Hunter on the nervous system were perhaps less extensive than might have been expected from so ingenious and indefatigable an inquirer. This seems to have been one of the few instances in which speculative views occupied him too much, to the exclusion of that inductive method of research which usually guided him to such sound and beneficial results. On some points, however, following his accustomed modes of inquiry, he arrived at highly important conclusions. He distinctly showed that organs which are endowed by one nerve with a special sense, derive their common sensation from another nerve having a different origin; and he determined this nerve of common sensation, in the case of the eye, the nose, and the ear, to be the nerve of the fifth pair. He extended the same reasoning to the organ of taste, though he did not verify it, as in the preceding instances, by anatomical demonstration. Hunter, therefore, unquestionably originated, and pursued with no trifling success, that method of inquiry into the functions of the nervous system which Sir C. Bell afterwards carried out to so large an extent, and with such brilliant results. Another observation of great interest, both in a physiological and psychological point of view, was first made by Hunter—namely, that nerves adapted to the reception of peculiar impressions convey such peculiar impressions to the brain, though merely a mechanical stimulus be applied to them; and he instances the sensation of light produced by a mechanical impression on the retina, and that of sound by a similar impression on the acoustic nerve. Later experiments have extended this observation, by showing that if the stimulus be of a chemical or electrical, instead of a mechanical nature, the nerve will still convey its appropriate impression.

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* Thomson's Lectures on Inflammation, p. 369.

There is no function which Hunter has more largely illustrated by an appeal to comparative structure, than that of digestion, and the preparations in his museum displaying the anatomy of the organs which minister to this function, from the lowest animals up to man, form a most beautiful and truly instructive series. The power of living matter to resist the action of the gastric fluid had already been incidentally remarked by Grew; but Hunter adduced a most interesting illustration of the fact, in the case of the stomach itself, by showing that in some instances this organ is partially dissolved after death by the very fluid which it secreted while living. This fact has been disputed, and the phenomena referred to morbid actions going on during life by Cruveilhier and others; but the experiments of Dr. Carswell have set the matter at rest, and established the correctness of Hunter's views.

That the act of vomiting is performed by the diaphragm and abdominal muscles, while the stomach itself is passive, was a position maintained by Hunter, though M. Magendie imagined that he was the first to discover the fact.

To John Hunter we unquestionably owe the resuscitation of the study of transcendental or philosophical anatomy. This study did, in truth, originate with Aristotle; but that extraordinary man was, in this respect, not years only but two thousand years in advance of the age in which he lived; and neither Hunter, nor others far more learned than he, had any conception of the physiological treasures sealed up in the writings of the "mighty Stagyrite"—treasures which have fully come to light only within the last thirty years.

In Hunter's descriptions of his drawings illustrative of the development of the chick, is the following very remarkable passage:

> "If we were capable of following the progress of increase of the number of parts of the most perfect animal, as they first formed themselves in succession from the very first, to its state of full perfection, we should probably be able to compare it with some one of the incomplete animals themselves, of every order of animals in the creation, being at no stage different from some of those inferior orders; or, in other words, if we were to take a series of animals from the more imperfect to the perfect, we should probably find an imperfect animal corresponding with some stage of the perfect."

Surely never was an original magnificent conception so poorly clad in language? But here is the unequivocal announcement of a theory which, though too absolutely adopted by some writers, has exercised, and is worthy to exercise, no small influence on the reasonings of philosophical anatomists.

The subject of monstrosities engaged much of Hunter's attention. He framed a classification of them, and produced them artificially by curious and well devised experiments on living animals. He arrived at the conclusion that such deviations from ordinary structure were not, as it was then the fashion to term them, *lusus naturæ;* for he observed that every species had a disposition to deviate from normal development in a manner peculiar to itself—a position virtually the same with that which M. Isidore St.-Hilaire makes the basis of his celebrated treatise on the 'Anomalies of Organization.' Hunter explained congenital defects by a reference to the transitory structures of intra-uterine life. It was thus that he solved the question how the intestine came to be in contact with the testicle in congenital hernia. He observed the position and relations of the gland in the abdomen of the fœtus; traced its descent into the scrotum; found that it carried along with it a peritoneal pouch like a hernial sac; and showed that, in the event of the simultaneous passage of a portion of intestine, this pouch must remain a common receptacle for the intestine and the testis. He then went on to show how the abdominal position of the testis, and the transitory condition of the tunica vaginalis in the human fœtus, are permanent conditions in the lower mammalia.

Of Hunter's labours in the field of zoology it is now impossible to form a complete estimate, because that portion of his manuscripts in which his observations in this department were especially recorded, was destroyed by Sir Everard Home. Sufficient however remains, collected in his museum, and scattered through his published writings, to prove how great must have been the entire amount of his contributions.

He made several attempts at a classification of animals based on their anatomical structure. One of these he derived from the distribution of the nervous system; another from the reproductive organs; and another from the structure of the heart. The first was not carried out to its full extent; the second was relinquished as unsatisfactory; and the third was only an improvement upon that of Linnæus. Hunter, however, made very important advances towards a perfect classification of animals according to the distribution of the nervous system; and there was a stage of his inquiries at which, if he had not been deserted by his usual acuteness, he would assuredly not have left for Cuvier the grand division of animals into *vertebrata* and *invertebrata*. Hunter notes the aggregation of the nervous system into spinal and cerebral masses, as distinguishing fishes from those animals which are now called mollusca and articulata; but he did not perceive that the existence of this cerebro-spinal axis is equally characteristic of the classes above fishes; neither did it occur to him that, wherever there is a cerebro-spinal axis, there is also a bony case for it.

The study of what is now styled *palæontology* was in its infancy in Hunter's days. But there is a paper of his on some fossil bones, presented to the Royal Society by the Margrave of Anspach, which shows that he had much larger and more enlightened views on the subject than any which were then generally entertained. Professor Owen has given an analysis of his paper, which we here transcribe, as being more to the purpose than anything which we could offer.

> "In this paper, we may perceive that Hunter appreciated the value of the study of fossil remains, and their application to the elucidation of many important objects. First, with reference to the extension of our ideas respecting the zoology of this planet, we find him comparing the fossils which are the subject of the text with their recent analogues, and he shows that they differ both from them, and among themselves: his observations and comparisons are, it is true, too general and summary, and it was left to his successors in this field of inquiry to pursue the comparison with the requisite minuteness and precision, and to give names to the distinct but extinct species. Hunter next briefly alludes to the different situations and climates in the globe to which animals are more or less confined; and this subject, or the geographical distribution of animals considered in relation to fossil remains, elucidates, amongst other interesting questions, the changes of temperature to which different parts of the earth have been subject at different epochs. Hunter points out more distinctly, and with more detail, the evidence which extraneous fossils afford respecting the alternations of land and sea of which the earth's surface has been the theatre; and by his frequent allusion to the 'many thousand years' which must have elapsed during these periods, seems to have fully appreciated the necessity of an ample allowance of past time in order to account philosophically for the changes in question. Lastly, he treats of the nature and causes of the different states in which the remains of extinct animals are found: and many of the fossil bones which are the subject of his chemical experiments are still preserved in his museum."*

Having thus taken a cursory view of Hunter's doctrines and opinions, and of the general principles which he sought to establish, we proceed to notice some of the more remarkable of his particular discoveries, and these we shall take in the order in which they suggest themselves.

1. He discovered and described the organ of hearing in the sepia—a discovery which has been attributed by Cuvier to Scarpa.
2. He first described the semicircular canals in the cetacea, the observation of which Cuvier claims for himself.
3. He preceded Camper by a short time in the discovery of the air-cells in the bones of birds, though there is no reason to doubt the originality of Camper in the same observation.
4. He discovered the peritoneal canals, or openings, in the eel, salmon, and cartilaginous fishes, as also in the crocodile.
5. He described the continuation of the peritoneal canals into the corpus cavernosum penis in the chelonia—an observation brought forward as new by M. Isidore St.-Hilaire and Martin St. Ange.
6. He discovered the motion of the blood in insects, describing correctly the action

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* Note to Hunter on the Animal Economy, pp. 479, 490.

of the dorsal vessel, and the relation of the circulatory to the respiratory systems—points on which Cuvier was subsequently in error.

7. He first observed the bi-auricular structure of the heart in the caduci-branchiate batrachia.

8. He discovered that the tubuli uriniferi extend to the surface of the kidney.

9. He first discovered the renal organ in the snail.

10. He discovered the circular arrangement of the nervous ganglia round the oral aperture of the mollusca, as also the double abdominal nervous cord of the articulata.

11. Conjointly with his brother William, he ascertained the true nature of the connexion between the placenta and the uterus. How much of the merit of this was due to each, it is impossible to determine, as each claimed the whole, and the dispute gave rise to a bitter feud between them.

12. He discovered the lymphatics in birds.

13. Amongst the most important improvements in practical surgery of the age in which Hunter lived, and which may be classed among his discoveries, was the operation, first suggested and practised by himself, of tying the femoral artery for the cure of popliteal aneurism. Considered simply in the light of an operation for the cure of aneurism, in which a ligature was thrown round the artery above the aneurismal tumour, leaving the tumour itself unopened, Hunter's operation was certainly not new, because, Anel had performed at Rome, in 1710, precisely such an operation on the brachial artery, for the cure of an aneurism at the flexure of the elbow, and with complete success; but considered in the light of an operation based on the principles that the collateral bloodvessels would enlarge and carry on the circulation when the main trunk was rendered impervious, and that the aneurismal tumour would be gradually removed by the action of the absorbents, Hunter's operation was entirely new. It was new also in respect to its application to popliteal anuerism. The claim of priority which has been set up for Desault is absurd, for in his operation the artery was tied in the ham—not in the thigh; and though he did not open the tumour, it burst of itself, so that the operation affords no parallel with that of Hunter, and as regards results, was altogether inconclusive.

Several other of Hunter's particular discoveries have been already mentioned in connexion with his general doctrines and views, and more might be cited, but the foregoing may answer the purpose of the present brief survey.

We have thus endeavoured to give a sketch of what Hunter did for science, derived chiefly from an examination of his writings. But he has left a record of his thoughts and his discoveries far more interesting and impressive than any writing, in that museum, which is viewed by all capable of appreciating it with daily-increasing wonder. This is in reality by far the most remarkable of Hunter's works, and is, to our apprehension, utterly different in its whole scope and meaning from everything else that it ever entered into the mind of man to conceive or into his plans to execute. Anatomical museums, previously to that of Hunter, had been merely the repositories of certain objects calculated to convey information as to particular facts, or to exhibit points of individual interest or curiosity. Each object spoke for itself, and it spoke of nothing further. A museum which should carry out an abstract physiological principle through an almost endless series of forms, each exhibiting some new adaptation of structure to its manifestation, was a thing which had never been even dreamed of. Yet such was Hunter's museum. In the physiological portion of it at least, there is scarcely a preparation which stands alone. Each stands in relation to that which precedes and that which follows it, and each forms a link in an unbroken chain of investigation into the developments of the vital force. The whole is one continuous train of what may be called visible and tangible reasoning. It addresses us in an extraordinary symbolical language, in which the powerful but peculiarly constituted mind of Hunter delighted to embody its conceptions. As we have already hinted, he had in his composition some of the elements of a poet, and we may now add of an exceedingly great poet. That he was a man of vast imagination cannot be doubted, otherwise he never could have formed so stupendous a design as that of thus turning to shape the universe of life and giving it a local habitation. The German Fichte has maintained that the whole universe is the development of a "divine idea," which it is the province of poets and philosophers to interpret to man; to which view the practical objection is, that none but a mind commensurate with that of the Deity could take in the idea, or be equal to the task of expounding it; but assuredly, as far as life was concerned, Hunter appears to have been possessed, more than any other mortal, with some such divine idea, and being gifted with scarcely an ordinary command of language, he was at once driven by a natural deficiency and guided by the singularity both of his genius and of his subject, to the adoption of a mode of expression of all others the most suitable, and compared with which all verbal description or illustration would have been poor and unemphatic. His tongue was not fluent of speech, and his pen was not that of a ready writer, but his hand, long skilled in dissection, became the readiest and the most faithful interpreter of his thoughts.

PART SECOND.

Bibliographical Record.

Art. I.—*Sur une Fonction peu Connue du Pancréas, la Digestion des Aliments Azotés.* Par Lucien Corvisart.—*Paris*, 1857–58. pp. 123.

On an Almost Unknown Function of the Pancreas, the Digestion of Nitrogenous Food. By Lucien Corvisart.

We may say at the outset that this is a truly workmanlike effort in the cause of science, and that whatever success it may achieve will be well deserved. With this brief preamble let us on.

The author proposes to examine what are the transformations which the principal azotized foods undergo from the gastric and from the intestinal digestion; what are the stages of these transformations; what is the quantity of each food which each of these digestive processes changes; and how much a fixed quantity of gastric or pancreatic juice is capable of digesting. The result of numerous careful experiments is summed up in the following propositions, which we abstract: (1) The pancreas digests azotized food in a precisely similar way to the stomach; it does not act upon matters already digested by the stomach, but upon those which are little or not at all altered. (2) The digestive action of the pancreas may equal that of the stomach, for though the amount of its secretion is ten times less than that of the stomach, it is ten times richer in ferment (pancreatine). (3) The pancreatic fluid acts equally well in digestion, whether it be alkaline, neutral, or acid, and thrice as rapidly as the gastric. (4) The disintegration of the food which takes place in the stomach enables the pancreatic juice to act more rapidly than it would otherwise. (5) If pepsine and pancreatine are mixed together they no longer exert the same action that they would when separate—in fact, all digestive power may be lost from their destroying each other. This mixing is prevented in the body by the pylorus, by the absorption of the pepsine in gastric digestion, and by the bile, which nullifies the activity of the gastric ferment. (6) Bile is precipitated by the acid of the chyme, and does not itself precipitate the gastric pepton. (7)

The kind of food has a material influence on the quantity of pepton which the two digestions, the gastric and pancreatic, can produce. Thus musculine and caseine furnish thirty grammes of pepton, while an equal quantity of gelatigenous tissue, or albumen, furnishes only fifteen grammes. (8) Both digestions destroy the most characteristic properties of the several foods. (9) The generic characters of the peptons are solubility in water, whether acid, neutral, or alkaline; incoagulability by heat; non-precipitation by acet. plumb. for the most of them. The peptons resulting from either digestion, as a rule, do not contain albumen, at least not more than exists in the normal and pure pancreatic fluid. (10) During the first three hours after food there is no material absorption into the radicals of the portal vein, but yet there occurs a considerable increase in the proportion of albumen contained in the blood. This increase the author ascribes to the agency of the pancreatic juice poured into the duodenum at a time when it is destitute of ingesta; he believes that in the vena porta the globules and the fibrine of the blood are transformed by the absorbed pancreatic secretion into caseiform albumen. (11) No really differential character has been made out between the azotized matters called extractives, and the albuminose produced by gastric or pancreatic digestion. Now, the chyliferous vessels, *the vena portæ, and its continuation, the hepatic veins*—i.e., the vessels which receive most directly the products of digestion, are much richer in extractive matters (albuminose) than the rest of the blood; and so it may be remarked they are in glycose.

Certain pathological inductions may be made from the above conclusions. (A) A duodenal dyspepsia may be induced by the absence, vitiation, or insufficiency of the pancreatic juice. The uneasiness attending this is felt from two to three hours after taking food, and is deeper seated than in gastric dyspepsia. (B) Duodenal dyspepsia may also be induced by any condition which permits unabsorbed or unneutralized gastric juice to mingle with the pancreatic. An excessive amount of gastric secretion, or an open state of the pylorus, or an insufficiency in the biliary secretion, may all have this effect. (C) The derangement of the intra-venous (portal hepatic) digestion may also occasion a form of dyspepsia.

We have not herewith presented our readers with even a complete summary of our author's views, but we cordially advise them to study the work for themselves. It has the great merit, so commonly possessed by French writers, of being clearly and precisely written; and it has the further recommendation (which the satiated mental appetite will appreciate in these days of book-surfeit) of being short, containing altogether but one hundred and twenty-three pages. We would it had been possible for us to have obeyed the author's injunction, and to have repeated his experiments. Only so could we have really qualified ourselves to criticise accurately his work. But in default of doing this ourselves, we trust that what we have said will induce some of our readers to undertake the duty, and so "sa conviction . . . . dans tous les cas servira la science, si peu avancée sur ce sujet."

ART. II.—*On Poisons in Relation to Medical Jurisprudence and Medicine.* By ALFRED S. TAYLOR, M.D., F.R.S., Professor of Medical Jurisprudence and Chemistry in Guy's Hospital, &c. Second Edition.—*London*, 1859. pp. 863.

TOXICOLOGY, whether we consider it in relation to the science of life, to the detection of crime, or to the chemical art of discovering deleterious substances in organic mixtures, has made immense progress during the eleven years which have elapsed since the appearance of the last edition of this work. The study of the action of poisons on the living organism which constitutes what may be strictly called the science of toxicology, has been advanced by several extended researches undertaken with a purely scientific object, and without any view to the juridical relations of the questions investigated. In the practical part of the subject a similar progress has been made, and has no doubt been promoted by the occurrence during the last few years of several important criminal trials, which have tended to expose many fallacies, and to establish some valuable principles in medical evidence, and generally to stimulate investigation by drawing public attention to the practical bearing of questions which had before a merely scientific interest.

We are therefore not at all surprised at being told by Dr. Taylor, in his preface, that he has found it impossible within the same limits to treat of all the subjects comprehended in the former edition; and that he has found it necessary to confine himself even more strictly than before to the practical objects of the work. If any one, misled by the title, should take up Dr. Taylor's book, hoping to find in it an exposition of the relations of toxicology to the science of disease, he would be disappointed. In the comparison of the phenomena of disease with those of poisons we have a most valuable means of pathological investigation. On the one hand, we have a poisonous agent capable of being readily isolated, weighed, measured, and experimented on; on the other, the agent is subtle, and for the most part, we know neither whence it comes nor whither it goes; and even when we have some knowledge of its origin, we cannot seize upon it, or define its nature in any other way than by describing its effects. It is the study of the *modus operandi* of poisons, as affording the means of making the comparison we have indicated, which constitutes toxicology considered as a branch of the science of medicine; but in this sense, as we have already hinted, it is not included in Dr. Taylor's plan. His book is to be regarded merely as a repertory of all those facts and principles relating to poisons which bear on the detection and proof of crime, its object being to aid the lawyer in the due appreciation of medical testimony, and to fit the medical practitioner for his duties as witness. As a treatise on poisons in relation to crime we think that the work in its present form is the best and most comprehensive in existence in this or in any other language, and that all the modifications which have been introduced into the present edition will be found to be improvements.

As regards the employment of medical men as scientific witnesses, we adhere to the opinion that we expressed ten years ago in our former notice, that it would conduce to the interests of justice, no less than to the credit of the medical profession, that in every case of supposed criminal poisoning the aid of a professed toxicologist should be obtained, and that he should conduct the analysis and be answerable for the whole chemical evidence. We venture to say that a medical man, even if he has made the best possible use of the opportunities of becoming acquainted with practical chemistry commonly afforded by our medical schools, ought not in any case to take upon himself the responsibility of swearing to the presence or absence of a poison in an organic mixture. We regret that Dr. Taylor has not expressed an opinion on this matter, but we think we may infer that he agrees with us, from the fact that his descriptions of processes of detection are obviously not adapted for the instruction or guidance of the analyst, but rather for the suggestion and solution of all possible objections which might be raised in a court of law to the chemical proof. Dr. Taylor is well aware that no description of the chemical processes required for the detection of a poison, however perspicuous and detailed it may be, will fit a man to undertake an investigation to which he is unaccustomed, and which, even in the hands of those whose life is devoted to similar occupations, is attended with many difficulties.

Among the most important additions are the chapters in which the author has collected the facts relating to the deposition of poisons in the living body, and the period required for their elimination. From these facts it is concluded that the time required for this purpose varies, not only according to the nature of the poison and of the animal observed or experimented on, but that in the same animal or in man the same poison administered under conditions which are apparently similar, may be expelled by the secretions at rates which differ at different times. On this ground Dr. Taylor enforces the necessity of the utmost caution in founding conclusions as to the time which has elapsed since the administration of the poison from the quantity found in the stomach, liver, or other organs. In illustration of the necessity of this caution, Dr. Taylor refers to the well-known trial of Ann Merritt, for the murder of her husband, by the administration of arsenious acid; on this occasion a chemist of note declared upon oath that the poison must, from the quantity found in the stomach of the deceased have been taken a few hours before death. This opinion was purely speculative, for although arsenic is absorbed and eliminated more rapidly than any other metallic poison, excepting antimony, there are no facts to prove that it would disappear from the stomach in so short a time as a few hours, or even a day. At the trial of William Palmer, an opinion was expressed by Dr. Taylor himself, which at first sight appears similar: but here the question was one not of hours, but of weeks. It was then said that the presence of antimony in the contents of the stomach and intestines indicated that it had been administered within three weeks of death. This inference the author shows, we think satisfactorily, not only to be in accordance with all that was then known on the subject, but to be further supported by the experiments of Dr. Nevin, of Liverpool, undertaken since the trial, with a special view to the elucidation of the question.

We regret that our space does not allow us to pass on to the other chapters which distinguish this edition from the last. We invite the attention of the reader especially to those which treat of poisoning by opium and nux vomica and their respective alkaloids. With regard to these, the opinion is maintained that no single chemical reaction should be admitted as proof of their presence; and that the value of any process used for detecting them is to be estimated, not so much by its delicacy, as by the absolute certainty of the results which it is capable of yielding.

Art. III.—*Sulle Virtù Igieniche e Medicinali della Coca, e sugli Alimenti Nervosi in Generale.* Del Dottor Mantegazza.—*Milano*, 1859. pp. 76.

On the Hygienic and Medicinal Virtues of Coca, and Tonic Articles of Diet generally. By Dr. Mantegazza.

The author accepts the distinctions which Liebig has drawn between the plastic or nitrogenous, and the respiratory or carbonaceous articles of food. To these two he proposes to add a third class, the "*alimenti nervosi*," which have a stimulating effect upon the nervous system. He gives a classification of the various articles belonging to this class, of which he makes three subdivisions—the alcoholic, the alkaloid, and the aromatic. The substances comprised in the second of these subdivisions owe their properties to a peculiar alkaloid, and are farther subdivided into the *coffee-like* substances, such as tea, coffee, chocolate, and Paraguay tea; and the *narcotic*, such as opium, Indian hemp, and coca.

Coca is the dried leaves of a shrub which grows extensively in Bolivia, and on the Andes of Peru. It is the *Erythroxylon coca* of Lamarck, and belongs to the natural order Erythroxylaceæ. Some idea may be formed of the immense extent to which the substance is employed, from the fact that the value of the amount annually produced in the republic of Bolivia is estimated at twelve millions of francs. Its use appears to be limited to Bolivia, Peru, and certain provinces of the Argentine Confederation, and consists in slow mastication along with certain other substances.

Its physiological actions are said to be the following:

1. It stimulates the stomach and promotes digestion.
2. In large doses it augments animal heat, and accelerates the pulse and respiration.
3. It induces slight constipation.
4. In moderate doses (1–4 drachms) it stimulates the nervous system, so as to render it more tolerant of muscular fatigue.
5. In larger doses it gives rise to hallucinations and true delirium.
6. Its most precious property is that of inducing the most pleasant visions ("fantasmagoria") without any subsequent depression of the nervous energies.
7. Probably it diminishes some of the secretions.

The author observes that the antipathy against coca, which the vicious practice of chewing it, prevalent among the inhabitants of Bolivia and Peru, has gendered among Europeans, deserves to be overcome, and endeavours to show that the substance has medicinal properties of a high order. Besides being an excellent dentifrice, it stimulates powerfully the digestive functions, while at the same time it exercises a calmative influence over the mucous membrane of the stomach and intestines. In this double action upon the stomach—stimulant and calmative—it resembles Calumba. It is also recommended as an antispasmodic, and as of great service in many nervous disorders, and particularly in spermatorrhœa and debility of the generative functions.

The preparations recommended are the leaves themselves, an infusion, and a tincture. The memoir concludes with the histories of nineteen cases, in which the remedy was of great benefit.

ART. IV.—*A Treatise on Fracture.* By J. F. MALGAIGNE, Chirurgien de l'Hôpital St. Louis. With One Hundred Illustrations. Translated from the French by J. H. PACKARD, M.D.—*Philadelphia*, 1859. pp. 675.

MALGAIGNE'S treatise on Fractures and Dislocations has enjoyed so wide a circulation, and such well-deserved renown, that we must own to a feeling of surprise at learning that before the appearance of the present work no attempt has been made to present so popular an author in an English dress. The present book is a contribution to our literature from America, and is the work of a gentleman whose name is not otherwise known to us. It is one which we can conscientiously pronounce very valuable, and which will, we hope, soon be followed by a similar translation of the other part of Malgaigne's great work, that on Dislocations.

As to the original work we need not add anything here to the high eulogium which was pronounced upon it in a former number,* except to say that time and experience have now given even a higher estimate of it. It was therefore with great pleasure that we saw it placed within reach of that numerous class of the profession in England who read foreign books either with difficulty or not at all. Dr. Packard's, however, claims to be something beyond a mere translation. He says in his preface:

> "I have made it my great aim to render the text of the author as faithfully as possible, endeavouring at the same time to avoid offending the taste of the reader by the use of Gallic idioms. The notes which I have taken the liberty to insert are intended to set forth peculiarities in American views and practice, or accounts of cases in point; in one or two instances I have been able to look up quotations which were beyond my author's reach. An index has also been added, and a list, as full as circumstances would allow, of works hitherto published upon the same subject."

The former part of his task—that which regards the translation merely—has been executed by Dr. Packard in an extremely satisfactory manner. Seldom indeed has it been our lot to read a translation of any book, still less of a scientific treatise, in which the language is more clear, elegant, and natural—in which, as the translator says, "the taste of the reader is less offended by Gallic idioms." Nor is it, apparently, because Dr. Packard is pre-eminently familiar with the language that this satisfactory result has been obtained. In fact, to judge from some accidental slips which we have noted here and there, and which we should think could hardly have occurred to a practised French scholar, we should suppose that our translator does not enjoy a more intimate acquaintance with French than is very commonly possessed by gentlemen of good education.

It would perhaps be tedious to enumerate such slight errors as we had marked in the translation. They do not in general obscure the sense materially, and are in general easily recognisable by readers accustomed to the style of French medical authors. One blunder which runs through the whole book, and is frequently destructive of the sense of the author, is to render the French word "observation" by its English synonyme—the former being, as every French scholar is aware, their term for a written case. There are a few other such slips in the work, but there are few translations without them; and they need not prevent our recommending this book to English readers as a very faithful and readable translation of Malgaigne's classical work. Dr. Packard's style, we should say, is deformed by a few queer Americanisms, such as the use of "quite" for "very," which runs through the whole book, and appears sometimes several times in a page;

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* Vol. iii.

"concluded" for "determined," &c. These are to be regretted, as they grate upon the ears of a reader in England, and might have been easily avoided by a man of education.

We cannot help regretting also that Dr. Packard should have confined himself so strictly to the servile labour of translating, especially as the passage we have quoted from the preface seemed to promise something of a re-edition. The additions, however, which Dr. Packard has furnished are merely short accounts of detached cases, or descriptions of apparatus for the treatment of particular fractures. The more important questions which have been imported into the general subject since Malgaigne wrote—by the introduction of chloroform, and by the great extension of the practice of excision—have been quite passed over by Dr. Packard. Others which Malgaigne has mentioned cursorily, but which deserve longer treatment on account of the greater experience of them which we now possess, such as the use of tenotomy in oblique fractures of the leg, the treatment of fractures of the lower extremity without confinement to bed, &c., are dismissed with the few contemptuous lines which Malgaigne assigns to them. Where Malgaigne has fallen into errors, or where modern inquiry has rectified or enlarged the views which he propounded, Dr. Packard seldom does more than indicate the authors who may be consulted, and sometimes leaves the question altogether. Hence this book, which we opened with great interest, hoping to find a complete exposition of modern theory on the subject of fractures, together with the numerous expedients which American ingenuity might have invented in its treatment, turns out to be nothing more than our old friend Malgaigne with a new English face—a very useful work, indeed, and one which we hope will have an extensive circulation, but which cannot demand a more detailed notice at our hands.

We should perhaps have stated that the illustrations to Malgaigne are reproduced in this book, very small and not very well executed, but still sufficiently clear in general to render the author's text intelligible.

Art. V.—*Pathology and Social Science. The Irritable Bladder: its Causes and Curative Treatment.* By Frederick James Gant, M.R.C.S. Eng.; Surgeon and Pathological Anatomist to the Royal Free Hospital, and Conservator of the Museum; late Surgeon to Her Majesty's Military Hospitals, Crimea and Scutari.—*London*, 1859. pp. 136.

This is an ill advised work; it looks as if it were written for the public rather than the profession. The "pathology" is in a great measure borrowed from Sir B. Brodie, and is most of it quite familiar by this time even to tyros in medicine; "social science," as the author confesses, is very dangerous ground, nor can we say much for his dexterity in avoiding its dangers. It is perhaps inevitable that a work written for the perusal of lay readers should have a few ornaments from lighter literature, but really Mr. Gant makes a most liberal use of this licence. Shakespeare is of course to be quoted, for Shakespeare seems to come in as infallibly into the writings of our minor medical authors as King Charles I. did into Mr. Dick's memorial in 'David Copperfield.' The apostle Paul is also laid under contribution, and then we have references to a host of other worthies who would stare no little at finding themselves dragged in to assist at a consultation on "Irritable Bladder," were they made aware of the honour done them. Thus we meet with Moses, Soyer, Lord Clive, Byron, Voltaire, and even an unnamed poet, who probably stands to Mr. Gant in the same relation as the author of the 'Old Play' did to Walter Scott, and who sings as follows the horrors of nervousness:—

> "In every age and country there lives a man of pain,
> Whose nerves like chords of lightning shoot fire into his brain;
> To him a word's a sting—a look or sneer a blow,
> And more in one short hour he feels than some in ages know." (p. 68.)

Lastly, a digression of nine pages is introduced for the benefit of sea-bathers, which Mr. Gant confesses to be not very relevant to his subject, ending with the following paragraph, which we quote as a sufficient justification for saying that the book is one intended for the public, and not for professional readers:

> "On re-entering the machine the head should be washed with fresh water, and thus the hair may be preserved smooth and soft, and will not become crisp and coarse, as every one must have seen and experienced. If after this precaution the daughters of Neptune should choose to unloosen their tresses to the breeze, they may do so without the slightest risk of hair-splitting. I remember—but no matter; 'tis more the fashion now to roll the wet hair up in a ball, and thus ensure many a cold and headache. Such, then, are a few suggestions that I would offer to healthy sea-bathers, and I trust that they may prove useful and acceptable." (pp. 55, 56.)

Surely we could not have a plainer confession than this last sentence contains, that this treatise is intended for general perusal; we need not therefore be surprised that in a book which treats of a symptom often obscure, and referred by the author to the most various causes, no attempt should be made at diagnosis, that not a single case should be quoted, that irrelevant topics should be imported so freely into the discussion, and that matters which are as familiar as possible to surgeons should be described with all the minuteness proper to new discoveries. It would perhaps not have occurred to many surgeons, that it was necessary to treat the subject of *fistula in ano* in a work on the present topic; yet Mr. Gant gives us a full description of the operation with a view of recommending a new form of director for it. All that we need say as to this is, that Mr. Gant appears to operate for fistula precisely as everybody else does, and that no instruments beyond those in use seem to us necessary for so very simple a proceeding. To conclude, had Mr. Gant's researches into this subject furnished him with any materials for its diagnosis and treatment beyond those which are before the profession, we would have welcomed a treatise from him which would then have been a valuable addition to our practical literature; as it is, we must protest against attempts to popularize pathology, which we regard as having no tendency to elevate science, while they are only too likely to degrade its professors.

Art. VI.—*A Treatise on Medical Electricity, Theoretical and Practical; and its Use in the Treatment of Paralysis, Neuralgia, and other Diseases.* By J. Althaus, M.D. —*London*, 1859. pp. 352.

We so recently[*] and amply discussed the applications of electricity in medicine that we cannot avail ourselves as we would otherwise gladly do, of the opportunity afforded us by the publication of Dr. Althaus' work, to enter again upon the details of this interesting question. But our readers may have noticed that no English work of any magnitude headed the article alluded to. There existed a hiatus in British medical literature on the subject, which the present work most worthily fills up. Dr. Althaus' treatise is complete, accurate, and scientific. It is free from the assumption and pretension that unfortunately characterize so many electro-galvanic lucubrations which have issued from the press, and the whole stamps the author as a man of sound judgment and as a well-trained physician.

The book contains five chapters and an appendix. The first chapter is devoted to the consideration of the various forms of electricity, including static electricity, dynamic electricity, electro-magnetism, and animal electricity. In the second chapter, the physiological effects produced by the electric current upon the brain, spinal cord, the organs of sense, and other organs of the body, are examined; in the third chapter, we find an account of the various forms and methods suitable for the therapeutical employment of electricity, while the diagnostic uses of the agent are investigated and expounded in the fourth chapter. These four chapters lead very appropriately to an inquiry into the therapeutic use of electricity in medicine, to which the fifth chapter is devoted. The subjects to which the author here first directs special attention, are the treatment of the various forms of paralysis of the cerebro-spinal and sympathetic systems, and of the arrest of the lacteal secretion; the treatment of spasmodic diseases, of anæsthesia, of neuralgia, and of "rheumatic callosities"; the introduction of medicinal agents into the body by electricity, the extraction of metallic substances from the body, the therapeutical uses of electricity in surgery and midwifery, successively find their appropriate place in the fifth chapter. In the appendix, Dr. Althaus discusses very briefly the relations of atmospheric electricity.

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[*] British and Foreign Medico-Chirurgical Review, Jan. 1859.

With the exception of a few idioms which prove that the author does not discourse in his native language, the book is written in a clear and even elegant style. Dr. Althaus does not address us with the enthusiasm of a specialist, but with the honest conviction of a man of science; hence we feel sure that his book will prove acceptable to the medical profession, and will serve to diminish the ignorance which prevails upon the employment of electricity in medicine, while it will materially assist in establishing the claims of this agent upon our serious attention.

Art. VII.—1. *A Manual of Medical Diagnosis; being an Analysis of the Signs and Symptoms of Disease.* By A. W. Barclay, M.D. Cantab. and Edin., Fellow of the Royal College of Physicians, Assistant-Physician to St. George's Hospital, &c. Second Edition.—*London*, 1859. pp. 616.

2. *Clinical Lectures on the Principles and Practice of Medicine.* By John Hughes Bennett, M.D., F.R.S.E., Professor of the Institutes of Medicine and Senior Professor of Clinical Medicine in the University of Edinburgh, formerly Lecturer on the Practice of Physic, Physician to the Fever Hospital, &c. Third Edition, with 500 Illustrations on Wood.—*Edinburgh*, 1859. pp. 1005.

It is very evident that the authors of the above works have met a want of the profession, for although it is not a year since we introduced the former editions to our readers, and spoke of them in terms of high commendation, we have to record the unusual fact of both already appearing in new editions. Both authors may justly congratulate themselves upon such manifest success. Dr. Barclay's volume is re-issued without any material alterations, but in the Clinical Lectures of Dr. Bennett we find additions extending to above fifty pages, with numerous new woodcuts. Dr. Barclay has been particularly fortunate in meeting the requirements of the student, and we regard it as an evidence of the higher position the student of the present day occupies, that he should be ready to appreciate a work so entirely removed from the class of books that have ordinarily found most favour in the student's eyes. The mine of wealth offered in the Clinical Lectures imparts to Dr. Bennett's work a value that will insure it a high place after the present generation has passed away, not only because it marks the times in which it has appeared, but also because its faithful delineations of nature in her morbid manifestations will render it permanently useful to the earnest student.

Art. VIII.—*On Dislocations and Fractures.* By Joseph Maclise, Fellow of the Royal College of Surgeons.—*London*, 1859. Fasciculi v. to ix.

A year ago, we brought under the notice of our readers the first four numbers of the valuable illustrated work 'On Dislocations and Fractures,' by Mr. Maclise. The publication of the successive numbers has progressed steadily, and having appeared at the rate of one every three months, we are now able to record the issue of the ninth and concluding fasciculus. The following are the subjects treated of in the numbers before us: dislocations and fractures of the radius and ulna, dislocations and fractures of the hand, of the femur, of the tibia, the fibula and the patella, and dislocations and fractures of the foot. The concluding chapter or commentary discusses the law of articular development, with the causes, effects, and mechanical treatment of false joint and anchylosis.

We shall probably in our next analyse more fully the labours of Mr. Maclise, and must now content ourselves with the announcement of their termination.

Art. IX.—1. *Tracts of the Ladies' National Association for the Diffusion of Sanitary Knowledge.—London.*

2. *On the Hygienic Management of Infants and Children.* By T. Herbert Braker, M.D. Lond., Fellow of the Royal College of Surgeons of England, Fellow and Fothergillian Gold Medallist of the Medical Society of London, &c.—*London*, 1859.

We cannot but again express our satisfaction at seeing the laws of physiology, so far as they apply to the preservation of health and the prolongation of life, popularized by such works as those at the head of this notice. The rational physician finds no better assistance in the treatment of disease than the rational patient, and a suitable knowledge of the laws that regulate the economy must necessarily lead the laity to understand and appreciate more fully the scope and value of scientific medicine. But a knowledge of physiology will confer yet greater benefits upon all members of the community, by enabling them to avoid and prevent the debilitating influences which lay open the system to the incursions of disease. We hail with much satisfaction the formation of a Ladies' Association for the Diffusion of Sanitary Knowledge, for it rests more with the women of England that our race shall increase in vigour of body and mind, than with trainers, schoolmasters, or doctors; to them is entrusted the care and nurture of the infant and the child, and what is neglected in early life in the mother's or nurse's training can never be perfectly redeemed by after management. The society have begun their work well by issuing a series of penny tracts under such titles as the following: 'The Health of Mothers,' 'Why do not Women Swim?'* 'How to Feed a Baby with the Bottle,' 'The cheap Doctor: a word about fresh air,' 'How to Manage a Baby,' 'The Evils of Perambulators,' and the like. With the exception of one or two exaggerations, and the unnecessary introduction of the movement-cure, as an illustration, in the pamphlet on swimming, we cordially approve of the manner, matter, and method of these tracts.

Dr. Barker's treatise, equally with the tracts just spoken of, is addressed to the general reader, and contains profitable and available information on the subject of the physical and moral treatment of children, which it would be well for all young mothers and ladies otherwise interested in education to study. Nor do we think it inappropriate to advise the junior practitioner who is just emancipated from the discipline of the schools, to read such books, for they present him with physiological aspects which are not commonly offered to him in the systematic lectures and works to which his attention has been called. Dr. Barker treats successively of the causes of the frightful mortality still prevailing in early life, of the diet and regimen of the child, including such topics as clothing, temperature, air, sleep, bathing, light, exercise, and amusements. He then dwells upon the importance of vaccination, and after adverting to the physiology of dentition, concludes his little volume with some remarks on education in its more limited sense, with which we are disposed to concur as much as with the general tenor of the work.

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* In reference to this important matter—important in reference to the hygienic aspect as well as to the question of the preservation of life from drowning—we would venture to suggest the propriety of making the numerous swimming baths now existing throughout the metropolis available for the purposes advocated in the pamphlet. If certain hours in the day were set apart for female bathers, and a swimming-mistress attached to each bath, opportunities would be afforded which now do not exist in or near the metropolis, and the female part of the community might readily learn an art which would assist in prolonging and saving many a life that now languishes or is lost for the want of this acquirement.

Art. X.—*A Guide to the Practical Study of Diseases of the Eye. With an Outline of their Medical and Operative Treatment.* By James Dixon, F.R.C.S., Surgeon to the Royal London Ophthalmic Hospital, Moorfields, formerly Assistant-Surgeon to St. Thomas's Hospital.—*London*, 1859. pp. 435.

Four years ago, we had the pleasure of presenting to our readers a very ample account of Mr. Dixon's valuable work on the Diseases of the Eye. In the second edition, which is now before us, the author has made numerous additions, while the whole has been carefully revised, in parts re-arranged, in others re-written. Thus the seventh chapter, which treats of the choroid and retina, is in reality a new production, both in substance and in form. The author's larger experience in the use of the ophthalmoscope has enabled him to go much more into detail in regard to the appearances presented in morbid conditions of these parts when subjected to the illumining and magnifying power of that instrument. Numerous cases are also given in the present edition which were not included in the former one. We can therefore have no hesitation in reiterating the strong expressions of commendation which accompanied our former analysis of Mr. Dixon's work, and urging its study upon those of our readers who are desirous of obtaining sound information in the important branch of medical science to which it relates.

Art. XI.—*Engravings of the Ganglia and Nerves of the Uterus and Heart.* For the use of Students in Anatomy and Physiology. By Robert Lee, M.D., F.R.S.—*London*, 1858.

Dr. Lee presents us with a republication of the plates which have accompanied his several memoirs on the nerves of the uterus and the heart, "in the hope that a department of anatomy and physiology of such importance in medical practice may no longer remain in that obscurity and error in which it has been left involved in the most recent works on anatomy published in Great Britain and on the Continent of Europe." The plates are unaccompanied by any letterpress beyond what is necessary to a bare explanation of the parts delineated. The plates themselves are thirteen in number, and have the merit of being clear and intelligible. Nine are devoted to the uterus and four to the heart. We cannot but thank Dr. Lee for having made the results of his researches so readily accessible to a larger class of inquirers, and trust that they may prove conducive to extending the knowledge of the intricacies of the nervous distribution in the heart and uterus.

Art. XII.—1. *Illustrations of Typhus Fever in Great Britain, the result of Personal Observations made in the Summer of* 1853; *with some Remarks as to its Origin, Habits, Symptoms, and Pathology; to which is appended a Brief Account of the Reappearance of Typhus in Boston in the Winter of* 1857–58. By J. Upham, M.D., &c.—*Boston*, 1858. 8vo. pp. 46.

2. *An Essay upon the Relation of Bilious and Yellow Fever, prepared at the request of, and read before, the Medical Society of the State of Georgia, at its Session held at Macon, April* 9th, 1856. By Richard D. Arnold, M.D., Professor of the Theory and Practice of Medicine in the Savannah Medical College.—*Augusta, Ga.*, 1856. 8vo. pp. 30.

In the first of these pamphlets, which is a republication of a paper in the 'Boston Medical and Surgical Journal,' the writer gives an account of his observations on typhus fever in this country. Some ten years before, he had published, in the same journal, a description of the maculated or ship fever, which prevailed at the South Boston and Deer Island Hospitals.

Dr. Upham had been convinced of the distinct nature of *typhus* and *typhoid* fever by his own observations in 1847–48, and he seems inclined to subscribe entirely to the views of Dr. W. Jenner, as to the existence of four distinct diseases, long confounded under the head of "continued fever." Dr. Upham arrives at the conclusion, that the typhus of Great Britain and Ireland is the same with that which raged on the shores of America in 1847–48; the disease in both instances presenting a like adynamic character, and requiring a similar plan of treatment. His observations will, of course, be read with greater interest in America than here, on account of the comparative novelty of the disease in that country.

Dr. Arnold's essay on the relation of bilious and yellow fever is a practical contribution to a subject on which, notwithstanding the great accumulation of facts, the state of opinion is still unsettled. The highest authorities, however, and those whose opportunities of personal observation have been greatest, seem to be fast ranging themselves on that side of the question which regards yellow fever as a peculiar pestilential disease, by no means to be confounded with the common bilious remittent of warm climates. Dr. Arnold is a supporter of this opinion, and his remarks are valuable, as being those of a practical man who has had extensive opportunities of comparing the two diseases, and who has been led, by the observation of facts, to a view of the subject different from that which he originally entertained. The observations of Dr. Arnold are directed to two principal points of inquiry:—First, is yellow fever a distinct disease, or only a more malignant form of bilious remittent? Secondly, Is yellow fever contagious or not? On the first of these questions, he holds that yellow fever is, without doubt, a disease *sui generis*; and he founds this belief as well on the symptoms during life as on the appearances after death. We regret that we have not room to particularize points of contrast between the symptoms of yellow fever and of bilious remittent. Among the necroscopic appearances, a pale anæmic condition of the liver is considered by Dr. Arnold as the most peculiar and invariable. The second question—as to whether yellow fever be contagious—is answered by Dr. Arnold decidedly in the negative.

In an "addendum," Dr. Arnold gives the details of three cases, which prove "that sporadic cases of yellow fever do occur, having all the symptoms of those during an epidemic, and the same pathological appearances after death."

We have read Dr. Arnold's pamphlet with pleasure; it is the production of a man who takes a sincere interest in his subject, and who records, without fear or prejudice, the results of personal observation.

ART. XIII.—*Summary of New Publications.*

AMONG the numerous works which the past quarter has brought, there is one which in regard to intrinsic value and physical size claims the first position in this summary. It is the 'Cyclopædia of Anatomy and Physiology,' edited by Dr. Todd, which was commenced in 1835, and has now reached its termination. There are few of our readers who have not profited by the instruction which this great work conveys, and who will not be ready with us to congratulate the editor upon the successful conclusion of his labours. We hope in our next to devote a full consideration to the aspects of physiology as presented by the Cyclopædia. The subject that has specially engaged medical attention during the past months, Diphtheria, forms the subject of numerous contributions, which we also intend to analyse in our October issue; one of the volumes of the new Sydenham Society consists of the Memoirs of Bretonneau and other French authors on this subject, edited by Dr. Semple; Drs. Copeman and Ranking, and Mr. Ernest Hart also present us with papers on Diphtheria as observed in different parts of England. The first part of a work by Dr. Hirsch on the Geographical Distribution of Disease, promises to become an important contribution to medical literature. 'The influence of the Variation of Electric Tension on Epidemic and other Disease,' is considered by Mr. Craig, and will receive further notice. Dr. Headland's valuable essay on the Action of

Medicines on the System has already reached its third edition. A work entitled 'Art *versus* Nature in Disease,' by Mr. Henriques, is devoted to an elaborate attack upon the work of Sir John Forbes—'Nature and Art in Disease'—upon the ground of the latter being, "however cunningly devised and carefully concealed, an attack upon the assumed delusion of the homœopathic system of medication." In a pamphlet of fifty-six pages Dr. Roods discusses Sciatica and Spinal Irritation; from Dr. Handfield Jones we receive a further *exposé* of his views regarding the influence of the malarious poison in producing many prevalent disorders that are commonly classed among neuroses, under the title of 'A Tract on Neurolytic and Aguish Disorders,' which we recommend to the careful attention of our readers. From Vienna the Report of the great Hospital of that town for 1858, drawn up by Professor Haller; and from Philadelphia the essays of Dr. John Kearsley Mitchell on various medical subjects, have reached us.

Among the surgical works before us we would first mention Mr. Tomes's 'System of Dental Surgery,' a work that will doubtless create an era in that department, and which we hope to analyse in our next number; the subject of Hæmorrhoids and Prolapsus of the Rectum finds an exponent in Mr. Henry Smith; Mr. Butcher favours us with a third series of his 'Reports on Operative Surgery;' the January number of the 'Ophthalmic Hospital Reports,' with articles by Dr. Taylor, Mr. Hulke, Mr. Dixon, is before us. Dr. Fraser presents us with Crimean reminiscences in the form of 'A Treatise upon Penetrating Wounds of the Chest;' a third edition of Mr. Chapman's work 'On the Treatment of Ulcers of the Leg,' a reprint of Dr. Coghill's 'Observations on Strabismus;' and a German work 'On Stricture of the Urethra,' also deserve mention.

Obstetrical science brings us a republication, under the auspices of the New Sydenham Society, of Dr. Gooch's work 'On some of the most Important Diseases peculiar to Women,' with other papers. It appeared thirty years ago, when British medicine was peculiarly barren of all sound information on the points to which it is devoted; the value of its republication is enhanced by an analytical and argumentative memoir by Dr. Ferguson, which is prefixed to the volume. With this volume we would also mention another publication of the same Society, which nothing but want of space has prevented our already alluding to more fully, the translation by Dr. Whitley of Diday's 'Treatise on Syphilis in New-born Children and Infants at the Breast.' 'The Use of Chloroform and other Anæsthetics, their History and Use during Childbirth,' by Dr. Chapman; and a reprint of Dr. Duncan's papers 'On the Cervix Uteri in Pregnancy,' conclude our list in this department of medical science.

In psychology and mental pathology we have first to introduce Mr. Bain's new work entitled 'The Emotions and the Will.' Those who are familiar with the 'Asylum Journal' will be glad to hear that the interesting papers of the learned editor on the Psychology of Shakespeare have been republished in a separate form, with additions. The Scotch Commissioners in Lunacy have issued their first Report; from the English Commissioners in Lunacy we have received a Supplement to their Twelfth Report; we have before us a German work on General Mental Pathology, by Dr. Wachsmuth, with sundry Reports on Lunatic Asylums and continuations of the periodical literature devoted to Insanity.

Under the head of 'State Medicine and Sanitary Science,' Dr. Milroy's paper on Quarantine claims to be mentioned; an article by Mr. Sidney Herbert, reprinted from the 'Westminster Review,' 'On the Sanitary Condition of the Army;' the second edition of Mr. Erasmus Wilson's translation of Hufeland's 'Art of Prolonging Life,' and the sixth edition of Mr. Wilson's 'Healthy Skin; a Popular Treatise on the Skin and Hair,' also come under the same category. We cannot conclude this summary without adverting to a charming little book by Mr. Grindon, entitled 'Manchester Walks and Wild Flowers,' in which the young Manchester naturalist receives copious information available in his promenades in the vicinity of the El Dorado of cotton-spinners. We must also mention an inquiry by Dr. Struthers into the mode of Improving the Teaching in the Scottish Universities, in which the author advocates the licensing of extra academical teachers, so as to maintain a spirit of emulation in the professors; nor may we omit a passing allusion to Mr. de Morgan's paper 'On the Structure and Functions of the Hairs of the Crustacea.'

PART THIRD.

Original Communications.

ART. I.

Entoptics. By JAMES JAGO, A.B. Cantab., M.D. Oxon, Physician to the Royal Cornwall Infirmary.

(*Concluded from our last.*)

§ V. *The Retina.*

27. We do not find any spectres from the homogeneous hyaloid membrane (except *possibly* in a *moving* pencil minute dark dots from nuclei in it), but the rays of light in traversing the transparent retinal substance, encounter a set of bloodvessels, which, like much of the web in the vitreous, is visible with the naked eye against the sky, as dark shadows with lucid borders caused by light reflected from them, and therefore much brighter in the case of the whiter pulsating arteries than in that of the veins.

Nevertheless, though up to this time we have advantageously, as one means, practised our observations upon the shadows whilst they remain ocularly *steady*, the results to be earned by such method in the example before us are so obscure, that with that alone our investigations would be abortive; for it seems fruitless to search for a pair of shadows of any vessel in a couple of divergent pencils.

We are in condition to proceed, however, for by 10 we have an artifice which elicits these shadows with singular precision. When divergent rays impinge upon the retina from some point in advance of it, and we impress this point with a lateral motion with respect to the retinal surface illuminated by it, the shadows of all the vessels, which are at right angles to the direction traversed by the point, become conspicuous; and may, by great excursions of the point, be plainly detected making small excursions of their own *in the direction in which the point moves*, similarly to the conduct of the objects next them in the vitreous humour, but to a less extent; intimating by the deviation that the vessels figure themselves by true shadows, which are received upon a screen at a certain distance from their own site. Indeed, whilst the said movement is inoperative for bringing such portions of the vessels as are parallel to it into view, the slightest degree of it suffices for those at right angles. But no sooner is the movement stopped than the apparitions summoned forth become latent again. Wherefore, if the point be made to describe a circle in a plane perpendicular to the optic axis, so much of the *vasa centralia* as occupy the illuminated portion of the retina will be completely disclosed, as in their turn all will be *crossed* as required.

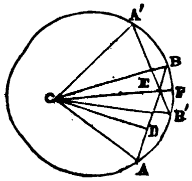
The very existence of the limited reflective margin proclaims that the vessels lie near the sentient points. From the same proximity there is no brightness from inflection within the bounds of the shadow, and no inflective fringes at the sides,—at least, none clearly pronounced, for I fancy that in using fine pencils in these movements I really discern a single subtile dark ring of this kind, round certain capillary dots.

Now, as a parallax attends the apparition in its maintenance by perpetual movement, the essential condition of the phenomenon must lie in the nature of the *sense* of sight, and depend upon the fact of the shadow being ever thrown upon *fresh* sentient points, as if when we would fain keep the shadow upon the same sentients, it oscillates somewhat over them so as to slur it and its bright borders into one another, or rather, as if the sentients refuse to reply nicely for more than an instant to a given stimulus. Thus, if we suddenly open an eye, that has been awhile shut, against the sky, or walk along by a hedge-row through which the sun shines, we get glimpses of the vessels.

28. Purkinje, to whom we owe these expedients for conjuring into view the vascular phantom, has bequeathed to us another striking one, the same in principle, though demanding a special explanation. This is that of waving a lighted candle before the face.*

Let us suppose (Fig. 5) ABA' to be a (circular) section of the sentient surface of the retina by the plane of the paper, in which the optic axis falls, and C to be the lenticular centre of the eye. Let AB and $A'B'$ be equal chords of the circle cutting each other in E. Draw CD perpendicular to AB, and join by straight lines C with A, A′, B, B′, and E, producing CE to meet the circumference $A'BA$ in F.

FIG. 5.



Then $EF = CF - CE$. But $CE = \dfrac{CD}{\cos ECD}$. And $CD = CB \cos BCD$, whilst $BCD = \frac{1}{2} ACB = \frac{1}{2}(ACF + BCF) = \frac{1}{4}(ACA' + BCB')$. Again, $ECD = BCD - BCF = \frac{1}{4}(ACA' - BCB')$. Moreover CB, very nearly $= CF$. Hence, if $ACA' = \alpha$, $BCB' = \beta$, $EF = d$, and the optical radius $= r$, the equation first given becomes

$$d = r \left\{ 1 - \frac{\cos \frac{1}{4}(\alpha+\beta)}{\cos \frac{1}{4}(\alpha+\beta)} \right\} \quad \ldots\ldots (3)$$

Acquiescing implicitly for the present in the hypothesis that the vessels are in front of the sentient surface, if A be the retinal place at a given instant of the image of a candle-flame, which is being waved laterally before the eye to keep the vessels in sight, and E the place of some vessel; then B, found by joining AE by a straight line, and producing it to meet the surface, marks the point by which E will be seen. Then, should we carry the candle round to the opposite side of the eye, so that the shadow of E, whose position is supposed unknown, deviates equally, twice, in one plane, that in which the optical centre of the eye and the two resting points of the flame lie, then B′ will mark the sentient place of the second shadow. Then, if we observe the whole angle α between the two resting points of the flame, and the angle, β, between the pair of shadows, we can determine the distance, D, of the vessel from the sentient surface. For example, let $\alpha = 96°$, $\beta = 4°$, $r = \frac{3}{5}$ of an inch (8), then $d = \frac{1}{108}$ of an inch, as is easily found by aid of a table of natural cosines.

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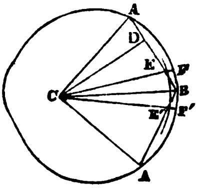
* Beiträge zur Kenntniss des Sehens, 1819, s. 89. Neue Beiträge, 1825, s. 115, 117. But it was Gudden (J. Müller's Archiv fur Anat. und Physiol., 1849, s. 522) who first noticed the parallax. However, he left for H. Müller (Verhandl. der Med.-physik. Ges zu Würzburg, 1855, ss. 411–47) to propound the theory of the relation of the vessels to the "perceiving membrane" adopted in this paper. He arrives at conclusions precisely similar to the above by measurements and calculations from the properties of the chords of circles; yet as the relative determination of the primary sentient seat is of such a high physiological importance, I have ventured to deviate from his plan, of using the chords that cross each other at the vessel in two observations, as we draw them in the course of the rays that project the shadows from the image of the flame in the back of the eye to that of the vessel. Not only because by substituting equation (8) I obtain a simple way of calculating the direct removal of a vessel from the sentients, but because I thus get a formula convenient for instituting certain comparisons with other *prima facie* possibilities of structural arrangement, which I am induced to think deserve a closer consideration than H. Müller has bestowed upon such contingencies. It is not enough to show, in order to refute this notion, that with shadows reflected from behind upon an anterior sentient surface, there must be two images. There actually are supplementary shadows of the vessels; one which seems to have escaped him altogether, which I shall describe in 81, and another which I shall speak of in 82, as an undoubted shadow, which he regards as arising from pressure of the blood current in the central vessels upon the retinal sentients, a sort of picture I believe never to happen (86); or, at all events, regards as complicated with such a picture.

Thus in ordinary language, when we hold the flame below the eye, the capillary patch at the punctum aureum will appear above any objective point upon which we gaze directly, revolving round it with a notable parallax as the flame encompasses the optic axis, receding from the point should the flame approach the axis, and *vice versâ*; the shadows of all the other vessels appearing in accordance with the plan of finding their places, as above expounded. So that it is indisputable that the shadowy figures of the vessels are projected by rays which diverge by reflection at the site of the image of the flame in the back of the eye.

With other conditions the same, the parallax is greater for a vessel more removed from the sentients. The principles of 4 and 10 apply here generally, the divergent pencil falling upon the vessels from very obliquely situated points, yielding much deviation of the shadow from its perpendicular retinal projection; the screen thus, too, becoming further separated from the object as the radiating image of the flame approaches it. So, as the candle travels about, do we see any two vessels which decussate one over the other, glide across each other from a consequent difference in parallax. Also the relations between the positions of the radiant points, the body and the screen (with an allowance for its obliquity), and the size of the shadow (3) hold good here also, whence the broad shadows afforded by a vessel when in the vicinity of the image of the flame. Were we, in figure 5, to join A and A′ by a straight line, and draw a tangent at F, and produce A, B and A′, B′ to meet it, we should obtain a figure similar to so much of figure 1 as has reference to two divergent pencils (4), and we might use the equation given in connexion with it for finding the distance of E from the tangent, and thus at no great labour, as we know the size of the eye, to get d.

29. Yet, again, let A B (Fig. 6) and A′ B be two equal chords of the outer of two concentric circles, in the plane of the paper, in which the geometrical and optic centres

Fig. 6.



of the eye are assumed to lie, and cutting the inner in E′ and E respectively; C the eye's optical centre; and C D perpendicular O A′ B. Draw the straight lines C A, C A′, C F, C F′ and C B. Then, as for fig. 5—

$$\text{E F} = \text{C F}\left\{1 - \frac{\cos\frac{1}{2}\,\text{A C B}}{\cos\left(\frac{1}{2}\,\text{A C B} - \text{B C F}\right.}\right\}.$$

Or, using the same notation for the angles, and for E F, as for fig. 5, but calling C E, r, instead of C F, we have

$$d = (r + d)\left\{1 - \frac{\cos\frac{1}{4}\alpha}{\cos\left(\frac{1}{4}\alpha - \frac{\beta}{2}\right)}\right\};\ \text{whence } d = r\left\{\frac{\cos\left(\frac{1}{4}\alpha - \frac{\beta}{2}\right)}{\cos\left(\frac{1}{4}\alpha\right.} - 1\right\}(4).$$

If E E′ indicate a section of the sentient surface, and F B F′ a section of some tunic without it, and light radiating from the image of the flame A, were to cause the point B in the tunic to be seen by being there reflected, E is the point by which we should behold it, and B C F the parallax. If the image of the flame rest at A′, similarly, B C F′ will be the parallax.

Glancing from equation (4) to its fellow (3), and remembering that β is comparatively small, it is plain that the fraction involving the cosines in the former instance must be a very little less than unity, whilst in the latter it must be a very little greater, so that for the same observed angles α and β, the two equations must give values of d not differing appreciably from each other.* So that, altogether, the conception of an exterior point, B, being seen by a second reflection of the rays from the flame, demands for it a parallax difficult, if not impossible, to discriminate from that of a vessel placed just as far interior (speaking in terms having sole reference to the eyeball) to the sentient surface.

There is a phenomenon, which if not an example of the kind imagined, closely simulates it. For as the flame nears the optic axis from a lateral position, so that objects lying by the latter may reach their greatest parallactic deviation, the middle of the vascular effigy acquires an umbrageous complexion, and if, during a few seconds, we whirl the flame with somewhat of briskness round the eye, an abruptly defined, dark, quite circular area, whose diameter subtends with me just 4°, as if from a sentient circle of about $\frac{1}{25}$th of an inch in diameter, comes forth; and as the flame travels round the optic axis, doing the same on the distal side of it in such a way as to show that the axis passes through its cause's own centre. In a word, it is made evident that the phenomenon is co-extensive with the foramen centrale, and is begotten by it; and recollecting the circumstance that the fovea is *par excellence* the retinal spot that suffers the pigment of the choroid to be visible from within, and that it has been demonstrated (28) that it is by the rays of light reflected from the internal periphery of the eyeball that the vessels are revealed, we instinctively ask ourselves whether the pigment could be seen as conjectured in fig. 6. And observing that the mean capillary patch, sweeping over the pigmentary circle, enjoys about half as much again of the scope of angular freedom that the said shady area does, whether we should assign from equations (3) and (4) the sentient surface an intermediate position to them and it.

30. However, if we adopt the opinion that the sentient surface is exposed to be excited by lucid images conveyed to it from rays traversing it centripetally, we allow that there is, *primâ facie*, no improbability in the hypothesis that the very shadows of the vessels inspected by us may be the reflection of shadows previously cast outwardly upon a surface enclosing the sentient one; and so, whether the last be located without, at, or within the vessels.

If the reader, in imitation of the style of figs. 5 and 6, will draw four concentric circles near together, and consider the outmost one an axiform section of a reflecting mirror A B A′, and the other three of sentient surfaces, passing severally without, through, and within the vessel E, whose shadow is cast by the ray A B upon the mirror at B, and reflected in B A′, which cuts the three sentient circles in e′, e″, e‴, indicating the parallaxes F C F′, F C F″, F C F‴, then it will be obvious that these parallaxes fall all in the direction of those which we actually witness in the vessels, and that these angles are greater, first as E is further from the mirror, and secondly, as the sentient circle is further from these. In the case in which E and e″ are in one circle—the surface in which the vessel is imbedded receiving its reflected shadow posteriorly at another place—we may simply by writing $\frac{\beta}{2}$ for β in equation (3) find d, the distance of either sentient or vessel from the mirror, or

$$d = r\left\{1 - \frac{\cos\frac{1}{4}\left(\alpha + \frac{\beta}{2}\right)}{\cos\frac{1}{4}\left(\alpha - \frac{\beta}{2}\right)}\right\} \quad .\ .\ .\ .\ (5).$$

And we have only to imagine the radii of the sentient surfaces supposed to lie within or without the vessel to vary, in order to diminish or increase this value so as to approach the value of d as estimated from equation (3).

If $\alpha = 40°$, and $\beta = 4°$, equation (3) gives d = 0·0087 of an inch, equation (4) gives d = 0·0088 of an inch.

Though it is true, then, that the trials of equation (3) upon variously disposed vessels in our own eyes will be found to accord very well with the hypothesis upon which we started, that the sentient points are a little external to the vascular plexus, within the limits of their known distance from the choroid, we can scarcely feel so sure that our mode of estimation is so conformable to the conditions of the standard—much less of an individual eye—or that the angles to be observed can be so reliably taken, as to entitle us to neglect the possibilities of other textural super-impositions, as hinted at by equations (4) and (5), backed by the spectral intrusion of the *foramen centrale.* We should not, without some hesitation, decide between the following arrangements:

a. The sentient surface without, the *causes* of the dark figures, of the foramen and the vessels, both casting direct shadows.

b. The sentient surface between, receiving the direct shadows of the vessels, and the reflected rays of the foramen.

c. The sentient surface within, at, or without the vessels, and receiving their images and that of the foramen, by reflection.

d. The sentient surface and the vascular plexus intersecting each other; either partly within, at, and without the other; the sentients receiving the images of the vessels and that of the foramen by reflection.

31. In considering if there be any circumstances which tend to eliminate any of these rival claims, it occurs that if the shadows of the vessels be disclosed after their reflection from any surface, the rays from the given pencil must not only pass the vessels, but must pass them, *again*, and, consequently, whichever of the three positions the sentient surface hold, there must be, from the same pencil, a pair of shadows for each vessel—if the sentient surface be the inmost, a direct one, and one with a parallax happening originally in the *re*-passing (reflected) rays. If the vessel and sentient points lie in the same surface, a direct one reflected with a decided parallax, and one happening originally in the re-passing rays, with no, or scarcely any parallax. If the sentient points lie on the outside of the vessels, a direct one, and with a greater parallax, the same direct one reflected.

In experimenting with the candle, I find that we may actually behold a notable supplementary version of the vascular figure. As we bear the candle round the eye, all over the more sensible parts of the retina, though gradually becoming more indistinguishable laterally, we may see, in a delicate guise, shadows of vessels as fragmentary black lines, of varying breadth and length, separated by lustrous interstices, conforming to the type of the vascular phantom when begotten by a bright line, as the back of a knife, moved across its length perpendicular to the optic axis, which only shows vessels and portions of vessels that happen to have a course parallel to itself. I observe, further, that the parallelism of the lines in the example before us, for ever indicates the meridional (that through the optic axis) plane, which the flame occupies *for that instant;* in short, a changing picture, rotating about the optic axis, as the flame revolves round it; successively ushering in such vessels as lie over the regions of the retina that sees them, or such as happen to be parallel to the said meridional plane, without developing more than a very slight parallax, *and that in the direction taken by the meridional plane.* Whence the intrusive phenomenon cannot emanate at all from the pencil that occasions the dominant figure, but must be attributed to aberrant rays of light from the flame itself that permeate to the back of the eye, without touching at the tunics: as some may well do by undergoing irregular reflections and refractions in the ocular media. And there are other facts to intimate that many rays really do so.

32. But besides this, we have yet another additional manifestation of the vessels. All the time that the flame is being whisked about the eye, each vessel in a flickering, though in a forcible, mien keeps its own image, as it were, stamped upon the sentients nearest it—just where it falls retinally when we look against the sky with the naked eye. If the flame has waved about before one eye for a little while, and we close this organ also, a brilliant, glancing, exquisitely complete copy of the vessels will vibrate before us for a few moments; even if the protected eye be suffered to view a surface too faintly illuminated to impart strong images of the vessels to that eye, or to extinguish acute impressions upon the nervous substance of the other, the said vessels will actively

disport on that surface. In fine, this phenomenon, I infer, is due to the circumstance that those sentients which lie directly under the vessels are usually less exposed to lucid stimulation than others, and that therefore when light is made to flow over the retina in a uniformly diffused fashion, they are in a state to become *comparatively* much excited. Thus whilst the whole retina, by the process we subject it to, is affected by luminous impression, the sentients underneath the vessels are pre-eminently so.

Save these two, not a glimpse can I catch of extra vascular spectres. The former has no existence with the divergent pencils we began with (27), or rather, when we use the candle, standing in lieu of those produced by divergent pencils, and may be seen in the very face of the flame if we look right into it whilst it oscillates near the eye. The latter accompanies experiments by all the pencils, and might be regarded as an example of the sort of supernumerary phantom we are in quest of, if there be such a one, when the sentient surface was spread immediately upon a whole plexus of vessels; so that it might be difficult to say, *hence only*, whether that expansion were the inmost or outmost of the spherical stratification. Yet when we balance the intrinsic consistency of the explanation proffered by H. Müller, with retinal anatomy, and the fact that this is borne out by entoptical phenomena in placing the vessels at various retinal depths, whereas no membrane, or any continuous surface contiguous to them *all*, overlays them, there seems to be little or no reason for attaching weight to this phenomenon, if found standing alone in ambiguous significance, in a sense contrary to the concurrent import of those remaining, if they shall be discovered to have such concord. Lastly, the other case imagined, of two shadows from the same radiant, would give birth to two pictures of the vascular plexus, so competing in size that they would breach one another, which does not appear to happen.

Being led, then, *per viam exclusionis*, to regard the vascular phantom as an immediate projection upon the sentient points, the question remains whether, somehow, the image of the foramen may not be simply a shadow similarly projected. H. Müller's suggestion seems worthy of acceptance, and is to the following effect:

33. We do not see in waving the candle round the eye a uniformly dark circular area, nor a complete circle at all, at any one instant, though we may elicit the whole circle in the course of one revolution of the flame round the optic axis. At any one moment we see a crescent, whose convexity looks to be towards the flame itself, and is really towards the flame's retinal image, this crescent approaching half-moon shape as the image advances towards the foramen centrale, and having a parallax like the vessels have, though of less amount. It is, therefore, the image of the crest of the wall of the central pit of the retina. On the outer side of the crescent there may be remarked a bright beam, which Helmholtz supposes to be reflected from the upper surface of the fovea, but which I rather regard as reflected, as in the case of the vessels, from the brim of the pit. I find that with $\alpha = 40^\circ$, $\beta = 4^\circ$ for the crescent, when for the same value of α, the vessel crossing the foramen, which presents the most parallax, gives $\beta = 8^\circ$. From which it results that the brim of the pit is 0·0036, or $\frac{1}{277}$ of an inch, and the vessel 0·0738, or $\frac{1}{136}$ of an inch, from the sentient surface.

From many entoptical trials, H. Müller estimates the interval between the "percipient layer" of the retina and the vessels at 0·17, 0·19 to 0·21, 0·22, 0·25 to 0·29, 0·29 to 0·32 millimetres. In the case of three other observers, 0·19, 0·26, 0·36 millimetres; and these numbers harmonize very well with those I have obtained. Then, from anatomical measurements, he determines that the bacillar layer of the retina in the region of the yellow spot is from 0·2 to 0·3 millimetres behind the vessels, and from the elements of this layer pointing as normals to the retina and collateral reasons, he concludes that they are the percipients.

Chary as we may be in trusting to absolute calculations from such observations as we can make, for the resolution of so principal a question as the primary seat of vision, yet it can hardly be gainsayed that a comparative scrutiny of all the phenomena fairly entitles us to decide that there is a certain anterior placing of the vessels and margin of the fovea with respect to the sentient surface, which appears to point with a high probability to the site assigned.

APPENDIX.

Visual Sentients.

34. Having expended our means of analysis by aid of entoptical, parallax, and allied helps, when we arrive at the seat of the sentients themselves, we must now resort to other expedients in order to get some acquaintance with their differential structure, as affecting the use of sight, and with any troubles of vision that may arise from the working of the machinery of the eyeball. Such knowledge is requisite to protect us against any error that might else creep into entoptical research proper, from a posterior source, as also may lead us directly to a more fundamental idea of the mode in which light stimulates the sentients. I shall proceed as systematically as I can under the altered circumstances.

If the extremity of the finger or nail be laid upon the sclerotic immediately, or through the lid, *even never so lightly*, in the nearest attainable regions to the most sensible parts of the retina, we shall elicit a quasi-luminous areola of the contour of the applied surface, though the surface itself seems lit up but in a very meagre degree, or not at all—that is, unless the applied surface be very small, when the brightness of the areola will so encroach upon the included area as to make it difficult to say whether it pales away entirely at the surface itself or not. Thus the resulting image of the surface stimulates the shadows of opaque bodies embedded in the retina, with their reflective borders. This marginal effect must, as Young remarks,* be due to the flexure undergone by the retina along that line. He says *greater* flexure along the contour than upon the applied surface, but I would wish to make the distinction that, though it is obvious that such a flexure as crowds together the internal elements of the retina produces a sensation of light, yet the experiment seems to yield no evidence that the bare pressure upon its outer surface, or the gathering of its elements closer to one another in the act of depression, has any consequence of a similar kind. Hence, we might infer that it is only the inner surface of the sentient layer which is sensible, or if the cones rather than the rods are sentients, that the rays of light affect them by impinging upon *their sides*. And if this be the arrangement, that each little sentient surface should be veiled from pressure in a little pit, we might perceive why the most employed portion of the sentient expansion should be walled in at the bottom of an abrupt fovea.

However, the characteristic colour of this and other phenomena from pressure is white, with a tendency towards the less refrangible colours. As if all sorts of lucid vibrations were engendered by mechanical stimulus, with some tendency towards those of slowest waves. The nervous impression in the instance before us is but transitory, but severe compression of the globe produces *spectra* of diversified orbital structures; and may be made to endure, seemingly along the lines chiefly where flexure such as above has been suffered, quite as long as the *spectral* image of the noonday sun has ever been known to last.

Not only is the retinal response to objective light injured or annihilated, according to the intensity of these *spectra*, but we find our vision impaired, or, except for strongly illuminated objects, obliterated by the sole existence of such retinal flexure, as occasions a marginal quasi-light, even though this does not suffice for leaving a discernible *spectrum* behind it.

35. When we turn in the dark the eyeballs sharply, or even mildly, a couple of white circular rings, brighter at one margin than the other, each enclosing a paler area with a central dark spot, flash forth, the diameter subtending an angle of several degrees. They are angularly apart from each other, and from the spectrum of a bright disc planted at the foramen centrale, and enjoy lateral angular play in strict conformity with the received opinion that they originate at the base of the optic nerve. The phenomenon is plainly the result of flexure of the retina where the nerve runs into it, as the eye is

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* Philosophical Transactions. 1793.

pulled round in its socket until it drags upon the nerve. And it is to be noted that it is *again* where the inner retinal elements are squeezed laterally that the phenomenon is disclosed.

36. In connexion with these facts, it may be convenient to notice an opinion universally adopted by physiologists, from Purkinje to the latest student of the accidents of vision, that the vascular figure, at least for the chief vessels, is rendered visible to us by the motion of the blood in the vessels, and may be thus made more so by pressing upon the contents of the eye through the anterior surface. I have finally convinced myself that all this is but an illusion, though one so deceptive that very much caution is required in trying to unmask it. It is astonishing how little light is necessary to display to us the vascular figure by the rays reflected from the coats of the vessels, especially of the white arteries, as if when the eye receives a very small quantity of light the increase effected where the reflected beams fall is of great moment in producing sensation. And if we carefully darken the eyes the *spectral* condition of the retina, which is impressed with the images of the vessels (see 32), just like the spectrum of a candle-flame, oscillates a considerable interval before it utterly departs; but if we shut ourselves in a room from which *all* light is excluded for a half-hour or more the last remnant of the spectrum vanishes, and then it is in vain to attempt to trace the course of a central retinal vessel. Nor can we do so by pressure upon the ball of the eye. The path of the vessels observed on opening an eye, after being compressed, against the sky, is nothing more than a momentary vision of their shadows by a retina impressed with mechanical spectra. This negative fact again seems to intimate that the retina only shows us quasi-lights when it is *creased* on its inner side, or at any rate only when squeezed upon hard enough in a radial direction, to yield a similar mechanical effect, in forcing together laterally, by flattening, the internal elements.

37. Yet, even in the darkest place, after any time we discover that false sensations of light forbid us to realize the fact of the total absence of light, because lights reach us in transient whitish or reddish spots, as if from some percussing body, seizing upon any region from the horizon to the zenith; one or more lucid clouds, if I may so speak, more or less apart, arising in any instant and dissolving in the next. Should we yawn we aggravate the appearances very singularly—that is, we squeeze the retina by forcibly compressing the eye behind the orbital muscle.

Having watched these lucid phantoms against a feebly illuminated ceiling of a room, we soon learn to distinguish them against a cloudless noonday sky, and to ascertain that they are always present.

Now, to the duplicate sclerotic coat of the conjoint globes twelve muscles are attached, eight before and four behind the equator, their inserent spaces so distributed as, when projected into our visual vault, to leave but little of it unrepresented by them. Thus it becomes a puzzling task to extricate from one another quasi-luminous phenomena brought to the general stock by individual orbital muscles. But, after the experience we have had of the instantaneous respondence of the sentients to the slightest touch of the sclerotic, and to a gentle pull of the optic nerve, we should anticipate that the direct strain of these potent instruments upon the tunics would be attended with vivid phosphorescence, as tending to draw them or the retina conically outwards, and thus to bend the inner ends of the sentients together; and a vigilant survey satisfies me that muscular action imparts the greater quantity, if not the whole, of the involuntary phantoms we are contemplating. We see frequent false lucidities after sleep, when the recreated sentients are aroused by the most trifling shock, and these indefatigable muscles are apt to jerk at the ball discordantly; and habitually the balls roll in their sockets to give room to the blood impelled at each pulsation to equally traverse their substance, or for the disposal of the fluids of the conjunctiva. Still, whilst the *will* does not interfere with these movements, the eyeballs are nearly kept *suspended* by the muscles, so as to be enabled to preserve their globular form, and are wheeled round smoothly, with so slight resistance to the traction of any muscles, that but exceedingly weak impressions are made upon the sentients. Nevertheless, if the will overrule this mutual consent of the muscles, the result is far otherwise; and if they be made to contract in vehement antagonism to one another, striking events accrue. Then dragging spasmodically at the globe, both before and behind the equator, in spite of the quantity of soft fat which they embrace, these muscles squeeze it against their knotted bellies, and other orbital protuberances, loosening their strain and clonically enforcing it again; not only striving to drag the coats outward at their insertions, but variously flexing the retina, and squeezing the vitreous fluid against it in depriving the eye of its sphericity. The phenomenon in its simpler aspect seems particularly noticeable to me over the region of the broad insertion of the superior oblique muscle, emerging in the visual field within and below the middle point of the field.

As a large artery is included in the trunk of the optic nerve, and two or three others lie near to its outside, some of the quasi-lucidity might proceed from them; but we have no like experience in the other special nerves, and though fibres of the optic nerve in traversing the retina are flexed when the latter is, yet we see no effect anywhere else than at the exact seat of flexure—that is, none where the flexed fibres terminate; nor does stretching the optic nerve, by turning the eyes, produce lucidities through it. It it possible that some of the minuter spectres might be occasioned by flexure of the retina by the very small choroid vessels. Even the nerves of common sensation are more easily excited at the points of their final distribution than along their course; so that, upon the whole, we seem to have no palpable ground for believing that any of the quasi-lights that assail vision originate posteriorly to the proper sentients of the retina.

Nevertheless, to return to the muscular action, a long suite of illusions originate therefrom. For every surface before the face whose image encounters a quasi-lucid cloud is sullied or expunged. If the eye rests gently upon some point of the wall of the room, patches of the pattern of the paper, the strings by which the pictures are suspended, or whole pictures, are buried in a mist, or entirely extinguished, singly or in groups, from moment to moment. And even if the other eye be brought to the rescue, but partial relief is obtained, for in both retinæ the sentients are very commonly compressed at correspondent regions. In combining two pictures into a solid form by the stereoscope much of this obliteration obtrudes itself.

If we lay a task upon the orbital muscles, as by regarding intently some object placed so near between the two eyes, as to demand an unusual contraction of the internal straight and superior oblique muscles; or if with absorbing effort we gaze towards the hands of another person so waved about before our face that the distressed eyes rather fixedly stare forwards than succeed in following their motions, the above events crowd upon one another, or even absolutely overwhelm the visual faculty. If a landscape is before us, when we strain our eye, and even, with some qualification, both our eyes, as the muscles are thrown into paroxysms of spasm, trees, houses, and fields take their departure here and there, through considerable angles; yet the degree of brightness of the objects tells upon the result—a moon certainly may be quite put out of sight, a sheet of water illuminated by the sun possibly, but the sun itself, in my experience, can never be thus removed. In a room the same phenomena prevail—pens, paper, &c., strewed over the table, flit away as if by enchantment; and even the flame of a candle may be thus extinguished. If we resolutely gaze into the powerful *equally diffused* light poured into the eye by a half-inch focus lens held very near both to the eye and flame, the rapidly diversifying efficiency of the different sentient patches, under the convulsive warping of the tissues, presents us with a furious whirl and seething of brilliant and grey patches, which is truly astonishing to behold. In this example the lucid cloud is indicated by the darker parts, as less acted upon by objective light. All these visual effects may be obtained just as certainly by squeezing the ball of the eye between the fingers whilst we gaze forwards, as by the strain of the orbital muscles.

38. Such unwonted efforts of the orbital muscles entail collateral effects. As we strain the eye with a divergent or convergent pencil in use, we see that the iris at the moment that the orbital muscles most violently pull upon the ball, contracts greatly. It may be because the retina is practically, as to objective light, darkened, but more probably from consentaneous muscular action. I have nobody at hand conversant with the laws of optics to watch the conduct of the images of a flame from reflections at the cornea, and two surfaces of the crystalline lens for me when I strain my eye. But I have no doubt that one or more of them would exhibit some instructive variations in

aspect. An intelligent observer, whom I had not previously told what sort of changes to look for, was struck with the dilatation of the pupil, and so particularly with the behaviour of the posterior inverted image, that he always forgot to notice the others. At the instant of spasmodic strain, as the pupil enlarged, the inverted image, as he expressed it, "dilated and retreated from his view, even quite from his view;" as if the lens became flattened behind.

39. Under the torture the ocular tunics become painful; the orbicular and levator palpebræ muscles, as well as those attached to the ball within and without it, become excessively fatigued and sore; so that the eyes seem to weigh down, and any use of them oppressive. This soreness may burthen us for hours and even *days*. Since the time of Mesmer the train of phenomena here detailed has been widely experienced. And the flitting away of objects from the field of view has been described by physiologists as "irradiation," or "extension of sensation," as if the lucid impression made upon a large area of sentients might also affect a limited number included by them, not touched by the light. This would be a visual defect, of which it is now plain we have no reason to accuse the retina.[*]

40. There is a certain period as the evening is closing in when, as we wink the eye, every separation of the lids is attended with a phantom of the central hole of the retina, that looks like a shadow of it. Now when the light has waned a little fainter still, we fail to see white objects not extending more than 4° at the eye (33) with direct vision, when we see them plainly with oblique, intimating that the region of the hole is less sensible to weak lucid impressions than other parts. From which it may be deduced that the foregoing shadowy appearance arises from the circumstance that the retina at the hole, in light of just force enough to enable it to see as well as at other parts, requires a moment longer to be equally affected by it. This is a long-known phenomenon, and I believe has generally been properly understood.[†] It is not a shadow of the brim of the hole exclusively (33), for it comes forth just when light is too scanty to enable us to catch the shadow of the vessels by winking; yet it looks sometimes annular, as if the shadow were associated with it.

41. I have now discussed, as far as I am aware of, all the essential mechanical disturbances of retinal action, and optical indices of a differential constitution in the bed of sentients. With a few words on the probable properties and seat of the sentients I shall bring this paper to an end.

Our visual experience teaches us that the region of the foramen centrale possesses usually the highest distinguishing power, and that this power gradually diminishes from it through the punctum aureum to the ora serrata. This ratio of diminution is perhaps proportional to that of the number of cones (not rods) that may be counted in equal areas from centre to circumference. Which suggests, that with an allowance for the peculiar central spot, the cones may be the true sentients, whilst the rods may assist or not. They are also normals to the retina, as seems necessary in the ultimate sentients. The law of isolation of sensation is absolutely stringent all over the retina, which seems to require the nerve to end in a mosaic bed of nervous bodies. In the anatomy of the retina, the radial fibres appear to be of non-nervous substance, and they issue from the internal centres of the rods and cones to connect them with a limitary membrane. If one or both of these latter, therefore, is the sentient, it is not unlikely there is an obstacle to its action at the tip. We might conjecture from this, that the elements of *conical* form are the sentients, and that they act *at their sides*—an idea confirmed by the most sensible part being lodged in a pit, and presenting a direct unveiled surface to the light. The sentients are behind the vasa centralia, at a calculated distance that agrees with the actual interval found between them and the vessels. Light falling posteriorly to a certain point would not affect the sentients, for pressure through the tunics evinces that it is only anteriorly that they respond to stimulus. This pressure also (34) showing that quasi-luminous appearances are produced by creasing the inner part of the retina, accords very nicely with the hypothesis that the sentients are cones pointing inwards, and that generally the sentient surfaces are in little cavities walled round, after the type of the foramen centrale. Such are the arguments that occur to me for assigning the office of receiving sensations from light to the cones.

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[*] Ocular Spectres, &c., p. 56. I cannot find the slightest trace that the true nature of this series of phenomena had ever been suspected by other observers; unless a suggestion of Donders, that pressure upon the eye lessens the sensibility of the retina. by checking its supply of blood, be a correct indication.

[†] Brewster's Optics, p. 418.

ART. II.

On the Simultaneous Existence in the Human System of Two or more Diseases, which are supposed to originate from Specific Morbid Poisons. By CHARLES MURCHISON, M.D., L.R.C.P., Assistant-Physician to King's College Hospital and the London Fever Hospital.

PERHAPS in no science more than in that of medicine, do preconceived opinions lead to an erroneous interpretation of facts, and in none is it more necessary that doctrines which are generally accepted should have a firm and sure foundation. Take an illustration from the subject before us. There is a prevalent belief in the profession that no two of those febrile diseases, which are thought to depend upon the introduction of a morbid poison into the blood, can exist in the system at one and the same time; and this doctrine has been turned to account as an argument against the non-identity of typhus and pythogenic[*] fever. Cases have been described in which the eruptions of these two fevers have co-existed, and have been adduced as positive proofs that the poisons of the two fevers must be identical. But if the doctrine upon which the argumeut is founded be erroneous, the force of the latter immediately disappears.

The doctrine itself originated from high authority, being first propounded by John Hunter. Before him, Adams tells us "it was overlooked." Hunter adduced several instances, in which he thought the introduction of a second poison arrested the progress of a previous one, and he drew the conclusion that "no two of them can exist in the same part of the body at the same time;" and that in the case of the eruptive diseases which "are necessarily the consequence of fever, it is impossible at the same time for the two to have their respective eruptions, even in different parts, because it is impossible that the two preceding fevers should be co-existent."[†] The law laid down by Hunter was subsequently confirmed by Joseph Adams, who in his admirable 'Observations on Morbid Poisons,' although he alludes to certain instances in which two such poisons did co-exist, yet considered these as only exceptions which proved the law. So early as 1801, however, the opinion of Hunter was strenuously opposed by John Ring, who collected, in his work on Vaccination,[‡] numerous cases to show "that two morbid actions and two eruptive diseases can co-exist." A similar opinion has in more recent times been held by Dr. Robert Williams,[§] who has brought forward many examples of the co-existence in the same individual of two of the eruptive fevers. Many isolated instances also, illustrative of the same point, have been recorded in the medical journals and periodicals, both English and foreign. Still the prevalent opinion in the profession is, as above stated, opposed to the doctrine of the possibility of co-existence. It is purposed, therefore, here to collect a portion of the evidence bearing upon the question, this evidence being derived partly from the experience of others, and partly from my own observation.

A. *Variola and Scarlatina.*—The co-existence of these two diseases has been alluded to by several systematic writers. The author of the article 'Variole,' in the 'Dictionnaire de Médecine,'[‖] observes:—"La variole peut être compliquée accidentellemente avec la rougeole, la scarlatine, et plus souvent avec le purpura hæmorrhagica." Dr. Gregory observed at the Small-Pox Hospital "several unequivocal cases of the simultaneous existence of small-pox and scarlatina anginosa."[*] Williams maintained that the variolous poison was capable of co-existing with many other poisons, and among others, with scarlatina;[†] and Erasmus Wilson remarks: "Variola is occasionally complicated with rubeola and scarlatina."[‡] Lastly, Dr. Copland quotes several authorities to prove that "scarlet-fever has been seen co-existing with variola, both distempers pursuing their regular courses."[§]

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[*] The term *pythogenic* is synonymous with *typhoid*, and has been proposed as a substitute for the latter, for reasons which will be found in a paper On the Classification of Fevers, in the Edinburgh Medical Journal, Oct. 1858.

[†] Hunter's Works, edited by Palmer, vol. i. p. 818; and vol. iii. p. 4.

[‡] Treatise on the Cow-pox, 1801–8, p. 1029.

[§] On Morbid Poisons. 1836.

[‖] Deuxième édition, 1846, tome xxx. p. 578.

Examples of the co-existence of these two diseases may be divided into—1. Those in which the scarlet-fever has first manifested itself. 2. Those in which the manifestation of the scarlet-fever and variola has been simultaneous. And 3. Those in which the indications of the variolous poison have preceded those of the scarlet-fever. The following are examples of the first class:

Illustration I.—An infant under the care of M. Revolat of Bourdeaux. In the course of an attack of scarlatina, and while the scarlet rash was well out, variolous pustules appeared, first, in the lower extremities, and on the following day on the face, tongue, and fauces, so as to impede deglutition.[‖]

Illustration II.—In the 'Gazette des Hôpitaux' for 1842,[¶] a case is mentioned of an infant aged three years, who had been under the care of M. Baudelocque for scarlet-fever, and in whom a varioloid eruption appeared on the twenty-fifth day from the first appearance of the scarlet-rash. The child had been vaccinated.

Illustration III.—For the particulars of the following case I am indebted to Dr. Walshe. Most complete details of it are recorded in the case-books of University College Hospital.[**] A female, aged twenty, was admitted June 23rd, 1847. She had been vaccinated satisfactorily, and had had both measles and scarlet-fever, but never small-pox. She had not been exposed to any traceable contagion.

June 19th.—Went to bed perfectly well.

June 20th.—Vomiting, violent headache, coryza, and suffusion of eyes.

June 21st.—Pains in back and loins, and in afternoon a well-marked scarlet-rash, appearing first on the face and then on the arms and shoulders.

June 22nd.—Headache gone; still very sick; rash more distinct.

June 23rd (fourth day).—Pulse 104; respiration 38; much lumbar pain; conjunctivæ injected; much coryza; tongue with a thick white fur; voice hoarse; throat sore; a sensation of choking; right tonsil red and enlarged; face bright red; and a purplish efflorescence, slightly raised, and on some places crescentic, on shoulders, chest, and abdomen.

June 24th (fifth day).—Pulse 120; respiration 34; tongue with yellowish-brown fur; throat much sorer, and both tonsils ulcerated; has passed two and a half "chamber-pots" full of blood *per vaginam;* skin very hot; face swollen; neck, chest, and fore-arms of a general uniform red tint; on left arm a number of papules, size of a split-pea, and in front of either wrist from ten to fifteen vesicles containing opalescent fluid.

June 25th (6th day).—Pulse 120; respiration 32; intellect clear; tongue almost clean, and voice improved; face less swollen, and has lost its bright red hue, but on forehead are innumerable purple petechiæ, varying in size from a pin's head to a mere point; efflorescence on arms less bright; vesicles much increased in number, and at some places passing into pustules, and distinctly umbilicated.

June 26th (seventh day).—Pulse 120; restless and delirious in night; much dysphagia and blood in stools; purple spots on face increased, and some ecchymotic spots on legs.

June 27th (eighth day).—Pulse 152; face very swollen and livid, with numerous petechiæ; an abrasion of left cheek, discharging bloody serosity; petechiæ and ecchymoses general over body; lips dry and brown; still discharge of blood *per rectum et vaginam;* sensible until a quarter of an hour of death, at one P.M.

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[*] Cyclopædia of Practical Medicine, vol. iii. p. 744.

[†] Op. Cit., vol. i. p. 211.

[‡] Diseases of the Skin, p. 84.

[§] Dictionary of Practical Medicine, vol. iii. p. 819.

[‖] Copied from Journ. de Méd. de Bourdeaux, in Gaz. Méd. de Paris, 1832, vol. v. p. 411.

[¶] Deuxième série, tome iv. p. 74.

[**] Female Case Book, vol. ii. p. 217. 1847.

On post-mortem examination, the tonsils, pharynx, and posterior nares were found large, prominent, and disorganized into a putrid greenish detritus; the epiglottis, larynx, and trachea were covered with punctiform ecchymoses, and there were numerous blood-coloured spots at base of right lung; there were also deep ecchymoses of the mucous membrane of the stomach, of the lowest thirty inches of the ileum, and of the large intestine, as also in the pelvis of the right kidney, and beneath the lining membrane of the portal vein. The blood was fluid, and the coats of the vessels generally stained.

Illustration IV.—A female servant, aged twenty-two, was admitted into the London Fever Hospital, February 7th, 1855; had had measles and scarlet fever—the latter ten years before. No record taken as to vaccination.

Feb. 4th.—Rigors, vertigo, and headache.

Feb. 7th.—On admission, pulse 112; respiration 28; tongue red at tip, with thick white fur; a characteristic scarlet eruption on face, arms, trunk, and about knees; face swollen.

Feb. 8th.—Pulse 96; a number of hard papules on face and a few on trunk; scarlet rash disappeared from breasts, but copious on arms and legs; much headache; no sore throat or dysphagia; tongue with thick white fur and prominent red papillæ.

Feb. 10th.—Still some scarlet rash on neck; some of papules contain lymph, others pus.

Feb. 11th.—Some sore throat, and a trace of albumen in urine.

Feb. 12th.—Pulse 80; sore throat and albuminuria gone; pustules on face and arms continue; desquamation of cuticle on fore-arms.

Feb. 14th.—Desquamation on arms continues; pustules disappearing.

Feb. 23rd.—Discharged well.

Illustration V.—This case in its general history closely resembles No. III. A boy, aged seventeen, was admitted into the London Fever Hospital, August 21st, 1858. He had been vaccinated at the age of three months, and had a satisfactory mark. When one year old he had measles, and at the age of six he had small-pox, the marks of which remained. No history of exposure to any contagious disease could be made out, and no one else of the family was ill.

Aug. 19th.—Rigors, followed by hot skin, severe pain in back and limbs, great thirst, loss of appetite, and sore throat.

Aug. 20th.—Body covered with a scarlet rash.

Aug. 22d.—Pulse 108; slept a little; sore throat; tongue moist and furred; scarlet rash well out.

Aug. 23rd.—Pulse 120; little sleep; much restlessness, but no delirium; throat sore; skin very hot, and covered with scarlet rash.

Aug. 24th.—Nine A.M.—Rash of a mulberry tint—at some places crescentic, and very like that of measles; coryza.

Eight P.M.—Pulse 112; no delirium, and is quite conscious, but has passed much blood per rectum; arms, hands, and face covered with a papular rash, many of the papules containing lymph. Several large petechiæ.

Aug. 25th.—Pulse very rapid and small; much weaker, and quite unconscious; face livid; many of papules vesicular, a few pustular and umbilicated; died at five P.M.

Illustration VI.—A man, aged twenty, a soldier in the Somerset Militia, was seized with all the symptoms of well-marked scarlatina, including the eruption, the characteristic tongue, sore throat, and delirium. Between the third and fourth day after the eruption had declared itself, and while this was still perceptible, but much faded, a number of vesicles, depressed in the centre, appeared; and in the evening of the same day the face was covered with well marked small-pox pustules. On the following day this eruption was in its most confluent form; involuntary stools, profound coma, gradual sinking, and death.[*]

In the following cases the eruptions of the two affections appeared almost simultaneously:

Illustration VII.—A boy, aged thirteen, was admitted into the Hôpital des Enfans Malades, under M. Baudelocque, Feb. 10th, 1834. He was a music printer, was in good condition, and had always enjoyed good health. About the middle of January his body became covered with purpura spots; and on Feb. 8th he had severe rigors, followed by febrile symptoms and great lumbar pain. On admission, face and arms covered with a scarlet rash, and a papular eruption, which appeared in the evening of Feb. 10th on face, trunk, and limbs; also numerous petechiæ and ecchymoses. Tongue thickly furred. Voice nasal. Tonsils could not be examined.

Feb. 12th.—Pulse 120; great prostration; intellect intact; several of the papules on arms have become vesicular, and are distinctly umbilicated; scarlatina eruption more violet in tint.

Feb. 13th.—Pulse 184; respiration 60; much restlessness and delirium at night, and now in profound coma; impossible any longer to distinguish the three eruptions; died at three P.M.

After death there were found variolous pustules upon the tongue and the surface of the tonsils: the pharynx and the larynx were covered with gangrenous sloughs.

In the remarks upon the case it is stated, that in the same hospital where cases of small-pox and scarlet-fever were constantly being brought together, a combination of the two was a matter of not unfrequent observation.[†]

Illustration VIII.—A young Russian naval officer, while scarlatina and variola were both very prevalent, was seized, Nov. 25th, 1834, with vomiting. On the 26th he was better, but on the 27th he had alternate rigors and flushes, headache, vertigo, &c. On the 29th a scarlatina eruption appeared on the face, neck, breast, and upper extremities; as also several solitary papules on the face; much dysphagia and redness of fauces.

Nov. 30th.—Scarlet eruption paler, but the papules distinctly variolous, and extending over the body. The case ran its course with great mildness, and the patient recovered. In the account of the case it is stated, that a similar one had been observed by Hufeland, at Weimar, in 1798.[‡]

Illustration IX.—A girl, aged nine, residing in London, was seized on Dec. 17th, 1844, with sickness, anorexia, and dysphagia. Had been vaccinated.

Dec. 18th.—Pulse 100; tongue furred, its tip red; fauces injected; chest and abdomen covered with a diffused scarlet rash, interspersed with thickly studded elevated red points.

Dec. 20th.—Pulse 90; the red points have increased into well-marked papules, which are general over the body, and on lining membrane of mouth; scarlet rash gone.

Dec. 21st.—Papules enlarged, centres depressed, and at many places nearly confluent.

Jan. 4th.—The variolous eruption has run its usual course, and the scabs have partly fallen off.

Jan. 7th.—Anasarca and œdema of face; urine not examined.

Within twelve days after the accession of the attack, her three sisters, who had all likewise been vaccinated, and were living in the same room, became affected with modified variola; while another girl residing in the same house was seized on the twelfth day with scarlet fever.[§]

Lastly, we have cases in which the rash of scarlet-fever has supervened upon that of variola.

Illustration X.—Vieusseux, in 1799, recorded an instance of three children in one family who were seized with small-pox. This affection ran its ordinary course until the period of desiccation of the pustules, when all three became affected with well-marked symptoms of scarlet fever, including the eruption. Two died, not of small-pox, but of the scarlet-fever; the third recovered.*

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[*] Mr. Gallwey: Lancet, vol. ii. 1858, p. 229.

[†] Gaz. Méd. de Paris, deuxième série, tome ii. p. 312. 1834.

[‡] Dr. Glehn: British and Foreign Medical Review, 1837, vol. iv. p. 219.

[§] Dr. Barnes; Lancet, vol. i. 1845, p. 640.

Illustration XI.—This case was recorded by M. Spadafora, an Italian physician, as an example of several similar instances which had occurred in his practice in 1830. A man, aged thirty-four, became covered with numerous purpura spots; on the fifth day a variolous eruption appeared on the face, chest, and hands; on the sixth day, the variolous pustules were well marked, and a second eruption had appeared, presenting all the characters of scarlet-fever: on the eighth day the purpura spots, the scarlet and variolous eruptions, were all well marked; on the tenth day the patient died comatose.†

Illustration XII.—For this and for references to several of the other illustrations I am indebted to an excellent paper by Mr. Marson, Surgeon to the Small-Pox and Vaccination Hospital, which was published in the thirtieth volume of the 'Medico-Chirurgical Transactions.' I cannot do better than quote Mr. Marson's own words:—

> "In the course of the last eleven years I have seen at the Small-Pox Hospital seven persons who had variola and scarlatina simultaneously. No two of these patients were received from the same place. The only discoverable disease under which they were labouring on their arrival was small-pox; but in the progress of this disease, scarlatina also became evident between the fourth and fifth days of the variolous eruption. Judging by the length of time that each disease is known usually to remain latent in the system after its reception, before constitutional symptoms are manifested, we may conclude that the germs of scarlatina were received towards the end of the incubative stage of variola
>
> "It is desirable that it should be clearly understood that these patients had all of them the leading symptoms of scarlatina well marked, and that the eruption was different from the roseola which frequently precedes the eruption of small-pox, and also different from the erythema arising from the miasm of hospitals; in fact, it was the florid red eruption peculiar to scarlet-fever, which can hardly be mistaken for anything else by a medical man whose eye has once been rendered familiar with it. Three of the patients had small-pox in the unprotected state, and four after vaccination. Six of them were adults; the seventh, a child four years of age. Three were males, and four females. In one the scarlet-fever was followed by anasarca; and in two by swelling of the parotid and submaxillary glands; and all had desquamation of the cuticle—a sequence which, it will be remembered, is not usual in small-pox, but, on the contrary, almost constant in scarlatina. All the patients recovered, but one."

Illustration XIII.—Mr. Marson, in the paper just referred to, also states, on the authority of Dr. Goodfellow, then Resident Physician in the London Fever Hospital, that three cases had been admitted into that institution with slight variola, and had there contracted scarlet-fever, the eruptions of the two diseases being concurrent.

I am not prepared to assert that all of the above cases were examples of the co-existence of small-pox and scarlet-fever. Some of the first class may have been instances of what has been described as roseola variolosa, which, according to Rayer, precedes the eruption of inoculated small-pox, about once in fifteen cases.

It seems difficult, however, on any other supposition than that here advocated, to account for those instances in which the scarlet eruption supervened upon the variolous, and was followed by desquamation, anasarca, and enlargement of the parotid and submaxillary glands. Illustration IX. also, in which the patient affected with the double disease would appear to have communicated scarlet-fever to one and variola to others residing in the same house, is of peculiar interest.

B. Variola and Rubeola.—The co-existence of these two exanthemata would appear, from the records of Medicine, to be by no means uncommon. Many such instances have been recorded by the older authors, such as Diemerbroeck, De Haen, and others; and Dr. Macbride, of Dublin, described a disease, under the designation *Morbilli variolosi*, or "measles resembling small-pox," in which, combined with a well-marked eruption of measles, there were many pustules which suppurated like those in the small-pox; and he tells us that in 1769, a number of the children in the Foundling Hospital at Dublin having been inoculated for small-pox, were in the meantime seized with measles, both species of eruption being perfectly distinct.*

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* Recueil Périodique de la Soc. de Méd. de Paris, tome vi. p. 417.

† Gaz. Méd. de Paris, 1837, deuxième série, tome v. p. 234.

Pinel † and others, it is true, have recorded instances in which the supervention of measles after inoculation with small-pox would seem to have delayed until the termination of the measles the supervention of the variola with its concomitant fever; but there can be no doubt that, in very many instances, the eruptions of the two diseases have existed simultaneously. In a few instances the appearance of eruption of measles has somewhat preceded that of small-pox, but in the great majority the order has been inverted. The following cases may be cited by way of illustration:

Illustration XIV.—Dr. Copland quotes, on the authority of Ettmuller, an instance in which the eruption of small-pox broke out on one side of the body, and that of measles on the other.‡

Illustration XV.—In 1765, small-pox and measles were both epidemic at Aleppo. Dr. Russell tells us that in many cases the one disease succeeded the other within two or three weeks, and that several were met with in which the pustules of the small-pox appeared on the face before the total disappearance of the measles on the limbs. Two cases are also recorded in which both eruptions were contemporaneous. In one, a female, aged two, there were the usual premonitory symptoms of measles, and on the fourth day the eruptions of both measles and small-pox appeared. On the eighth day the measles were fading, but the pustules continued very distinct, and ripened perfectly. The second case was a boy aged three. On the third day of febrile symptoms a measly rash and variolous pustules appeared. The pustules came to perfect maturity.§

Illustration XVI.—Two cases are recorded by Dr. Tracy, as having occurred to him in the spring of 1797. One of them, a young man, who a day or two previously had been exposed to the contagion of measles, which had excited some anxiety in his mind, was inoculated with small-pox. A mild attack of variola ensued; but on the tenth day the patient was seized with severe rigors, followed by febrile symptoms, and the day after by coryza and an eruption of measles, some of the variolous pustules not only remaining visible, but progressing towards maturation. The patient recovered. The other patient contracted small-pox after exposure to contagion; and on the third day after variolous eruption, a universal measly eruption, with the usual concomitants, supervened. Many new variolous pustules appeared after this, and the two diseases pursued their ordinary course "like two friendly sojourners in separate apartments of one tenement, without officious interference or molestation to each other." This patient also recovered.‖

Illustration XVII.—An infant, five months old, was inoculated in London with small-pox on April 19th, 1800. The inoculation was followed by a mild attack of variola. On the twelfth day, while the pustules were advancing to maturation, there was an increase of fever with stuffing at the nose. On the nineteenth day there was much coryza, redness and lachrymation of eyes, sneezing, and an eruption precisely resembling that of measles. On the fourteenth day the variolous pustules were still increasing, and the "morbillous eruption general and very distinctly marked." On the fifteenth day, "measles disappearing, small-pox advancing." On the seventeenth day, measles quite gone and the variolous pustules dying away also. Convalescence.¶

Illustration XVIII.—Ring records numerous cases in which the eruptions of variola and rubeola co-existed, without at all interfering with each other.**

Illustration XIX.—Dr. Winterbottom tells us that, in the winter of 1804–5, small-pox and measles were both epidemic in South Shields, and frequently co-existed in the same patient.[*]

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* Practice of Physic, 1772, pp. 876–9.

† Nosographie Philosophique, tom. ii. p. 51.

‡ Dict. of Pract. Med., vol. iii. p. 819.

§ Trans. of Soc. for Med. and Chir. Knowledge, vol. ii. p. 90. 1800.

‖ Medical and Physical Journal, vol. iii. p. 572.

¶ Ibid., vol. iv. p. 29.

** Treatise on Cow-pox, pp. 262, 267, 1029.

Illustration XX.—In the 'Quarterly Report of the Edinburgh New Town Dispensary,' for April, 1819, two cases of small-pox and measles co-existing are described. In the one the eruption of measles came out on the fifth day of the eruption of the small-pox, and both ran their usual course; in the other the eruption of small-pox came out on the third day of the eruption of measles, which last continued visible for two days more. Both children recovered.[†]

Illustration XXI.—A shoemaker, residing in Exeter, had three children, whose respective ages were six years, four years, and seven months, of whom the eldest alone had been vaccinated. The eldest child was attacked with measles, which, on March 29th, 1824, disappeared. On the same day the second child began to sicken with febrile and severe catarrhal symptoms. On April 1st, a measly eruption appeared, interspersed on the right cheek with a few minute pimples. On the 2d the pimples were more numerous, and some of them vesicular. On April 4th there were distinct umbilicated pustules, and on the breast and arms were "the brown marbled spots frequent after measles." On April 8th the pustules began to dry up; and on the 11th the crusts were separating. A child was inoculated with some of the matter from one of the pustules, and small-pox ensued. On April 8th the infant of the first family became covered with the rubeolar eruption. By the 15th it had completely recovered; but on this day she became feverish, and between this day and the 20th had several convulsions, and on the latter day small-pox appeared. Measles at the time was very prevalent in Exeter; and it was discovered that the two eldest children had been attending the same school as a child who had recently died of what had been described as combined small-pox and measles.[‡]

Illustration XXII.—Two cases in which the eruptions of measles and small-pox co-existed occurred under M. Guersent, in the Children's Hospital at Paris. The eruption of measles appeared in one case on the fourth, and in the other on the third day of the variolous eruption. One case recovered; the other proved fatal from double pneumonia.[§]

Illustration XXIII.—Two cases of small-pox concurring with measles and purpura have been described by Dr. F. J. Brown as having been observed at Rochester in 1845. One man had the concurring affections mildly, and recovered; the other, aged twenty-six, a sailor, died on the eighth day. The rash of measles came out on the third day of the variolous pocks, and increased for four days, during which the pocks remained comparatively stationary; after this the pocks progressed rapidly, and on the sixth day purpura showed itself, and there was hæmorrhage from the lungs and bladder.[‖]

Illustration XXIV.—This was the case of a young soldier in the Military Hospital at Davenport in 1854, which has been recorded by Mr. Broke Gallwey, Surgeon, Royal Artillery. The patient was declining rapidly of phthisis, when he became the subject of measles, so well developed that it might have been selected as a model case from which to study that exanthem. The eruption had arrived at its climax, and gained its turning point, when the patient sustained an attack of rigors, and on the following day the entire face presented, as it were, a substratum of shot beneath the skin. "Twenty-four hours later," writes Mr. Gallwey, "and I found I had a case of confluent small-pox, engrafted on a ground of rubeola, to deal with; and I may truly say that never had I met with more finished representations of either disease than were now delineated together in the same individual." The patient died about the period of maturation of the pustules. Mr. Gallwey considers this case as "unique in the chapter of medical curiosities;" but a reference to the literature of medicine shows that such cases are far from uncommon.[*]

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[*] Medical and Physical Journal, vol. xiv. p. 25.

[†] Edinburgh Medical and Surgical Journal, vol. xv., p. 314.

[‡] Mr. Delagarde: Medico-Chirurgical Transactions, vol. xiii. p. 163.

[§] Gaz. des Hôpitaux, 1834, vol. viii. p. 34.

[‖] Lancet, vol. ii. 1858, p. 290.

C. *Variola and Roseola, or Erysipelas.*—Allusion has already been made to a roseolous eruption which sometimes ushers in that of variola, and which has been designated *roseola variolosa;* but this is rather to be considered as a part of the disease than as a distinct affection.

Erysipelas, again, is a not unfrequent and a very fatal complication of small-pox, as well as of the other exanthemata, and owes its origin apparently, in some cases, to vitiated hospital air, and in other cases to contagion.

D. *Variola and Pertussis.*—Although no cutaneous eruption accompanies hooping-cough, it resembles the exanthemata in attacking an individual but once in the course of life, in being infectious, and consequently in depending upon a specific poison. According to the observation of Dessessarz, quoted by Dr. Copland,[†] the presence of hooping-cough delays the eruption of variola; while, on the other hand, it has been thought that an eruption of variola will arrest, or, in some cases, effect a cure of pre-existing hooping-cough. Dr. Willan mentions cases of hooping-cough, in which "this disorder was instantly superseded by the appearance of small-pox, after the decline of which the cough returned with the same violence as at first."[‡] Mr. Okes relates the case of a child, whom he inoculated with small-pox while labouring under severe hooping-cough. As soon as the eruption appeared, the cough ceased and never returned.[§]

But we have the most abundant and conclusive evidence that the two diseases may co-exist in the system, without at all interfering with each other. Take, for instance, the following account, given by Dr. Willan, of the prevalence of small-pox and hooping-cough in London in 1796:

> "These two complaints have been intimately connected for several months past. In many instances the paroxysms of the cough continued without abatement through the whole course of the supervening small-pox. The hooping-cough, in other cases, first commenced during the eruption of the small-pox, and remained a long time after it without any material alteration. These observations tend to set aside an established opinion among physiologists, that two specific diseases, at least two specific contagions, cannot actuate the human constitution at the same time. That the operation of one contagion is occasionally suspended while the body is under the influence of another, may be granted; but I am convinced, from a variety of cases, that this is not a universal law."[‖]

E. *Variola and Varicella.*—An example of this combination is recorded by Ring:

Illustration XXV.—two children in one family were inoculated with small-pox matter. One had small-pox, followed by chicken-pox. The other child, who had previously been vaccinated, had modified small-pox contemporaneous with chicken-pox. A school-fellow had chicken-pox at the same time.[¶]

F. *Variola and Vaccinia.*—These differ in their mutual relations from the diseases whose combinations we have been already considering, inasmuch as they are both probably only modifications of one and the same poison; yet they are modifications presenting a very striking contrast. As a general rule, after vaccination has run its usual course, the constitution may be regarded as protected against the action of the variolous poison; and, in like manner, after an attack of small-pox, the constitution is proof against the vaccine virus. But, as was shown by Willan, when a person is inoculated with vaccine and variolous matter at the same time, or within a week of each other, both inoculations take effect, and each pursues its course as in two different subjects.[**] Willan, indeed, figures a variolous pustule, which rose and maturated within the margin of the vaccine vesicle; and there are instances on record of both diseases resulting in individuals who have been exposed to the infection of variola at the time of vaccination, or a few days previously.*

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[*] Lancet, vol. ii., 1858. p. 229.

[†] Dictionary of Practical Medicine, vol. iii. p. 819.

[‡] Reports on the Diseases of London, p. 8.

[§] Medical and Physical Journal, 1802, vol. viii. p 426.

[‖] Op. cit. p. 88.

[¶] Treatise on the Cow-pox, p. 944.

[**] On Vaccine Inoculation, 1806, 4to. p. 8.

Illustration XXVI.—Numerous cases of the co-existence of variola and vaccinia are recorded by Ring. One of the most remarkable is the following. A variolous eruption appeared in a girl five days after vaccination. "One of the variolous pustules appeared distinctly within the margin of the vaccine tumour." A child was inoculated with matter from the vaccine vesicle; cow-pox resulted. Three children were inoculated with matter from the variolous pustule which encroached upon the vaccine vesicle, and all took the small-pox.†

Illustration XXVII.—A girl, aged fourteen, was vaccinated, and eight days after inoculated with small-pox matter; having in the meantime been also exposed to the contagion of variola. She contracted the two diseases, and "both went on together without interfering with each other."‡

Illustration XXVII. (*bis.*)—A child was seized with confluent small-pox on the fourth day after vaccination. The vaccine pock ran its usual course, and on the 9th day matter was taken from it, with which three other children were inoculated. All three had the cow-pox only.§

Illustration XXVIII.—Mr. Marson, of the Small-Pox Hospital, writes: "I have several times seen small-pox and the vaccine disease advancing *pari passu*, without the usual progress of each disease respectively having been interrupted."‖

In most cases, such as those just described, the variola is modified, the pustules being hard, and not advancing to maturation; but there are exceptions: Bousquet has given no fewer than sixteen cases in which vaccinia and variola co-existed, and yet all the patients perished.¶

Some curious experiments have been made by inoculating a mixture of the vaccine and variolous secretion. Adams maintained that under such circumstances only one of them will produce its effect;** but of three children inoculated in this manner by Bousquet, two had the cow-pox only; while in the third, the cow-pox proceeded as usual till about the eighth day of that disease, when a slight variolous eruption appeared.††

G. *Vaccinia and Scarlet Fever.*—Illustration XXIX.—Jenner has recorded the case of a child in whom scarlatina with sore throat appeared on the ninth day after vaccination. During the persistence of the scarlatina there was a total suppression of the areola around the vesicle. A sister of this patient, in whom the areola had already formed, was also seized. The scarlet rash was slight, and suddenly disappeared; but four days later, on the decline of the cow-pox, the usual symptoms of scarlatina anginosa appeared, and ran their course.‡‡

In these cases the two poisons would appear, to a certain extent, to have mutually neutralized each other.

Illustration XXX.—Three children were vaccinated by Ring. Before the cow-pox had attained its height they were attacked with scarlet-fever, the violence of which appeared to be neither increased nor diminished by the cow-pox. All recovered.§§

H. *Vaccinia and Rubeola.*—Illustration XXXI.—Ring has recorded five cases in which measles supervened at various periods from the second to the eighth day after vaccination. The measles ran its usual course, and the vaccine pustule was not at all interfered with, the areola remaining perfect.‖‖

Illustration XXXII.—In the winter of 1804–5 many children, who had been vaccinated at South Shields took measles. In all "the cow-pox vesicle proceeded regularly, except in appearing to be larger than usual, and having no areola." Three children were vaccinated with matter taken from a child on the eighth day after vaccination, and on the fourth of the eruption of measles. All three took the cow-pox, but none of them measles.*

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* Williams on Morbid Poisons, 1841, vol. ii. p. 40.

† Treatise on Cow-pox, p. 481, &c.

‡ Mr. Pole: Medical and Physical Journal, 1801, vol. v. p. 340.

§ Medical and Physical Journal, vol. xiv. p. 25.

‖ Medico Chirurgical Transactions, vol. xxx. p. 125.

¶ Traité de la Vaccine, p. 117. 1833.

** On Morbid Poisons, p. 16. 1807.

†† Op. cit., p. 323.

‡‡ Further Observations on the Cow-pox, pp. 133, 170.

§§ Treatise on the Cow-pox, p. 656.

‖‖ Ibid., pp. 108, 249, 744. 1801–3.

Illustration XXXIII.—On December 27th, 1821, an infant was vaccinated in each arm. On the following day its brother had a distinct eruption of measles. On Dec. 30th, the vaccinated infant was also seized with measles, the eruption of which, two days later, was very characteristic. A third and a fourth child in the same family became successively affected with measles. In the vaccinated child, the measles and cow-pox both ran their usual course together. With matter from one of the vesicles another healthy child was vaccinated with perfect success.†

I. *Vaccinia and Pertussis.*—Adams, in his work on 'Morbid Poisons,' expressed a belief that a permanent cure of hooping-cough was often effected by vaccination; and he adds, that in his day, this was so generally understood, that mothers were in the habit of bringing their children to the hospital for vaccination, under the expectation of curing them of hooping-cough, and that he had seldom known them disappointed. On the other hand, Mr. Marson, Surgeon to the Small-Pox Hospital, writes:—"Hooping-cough and the vaccine disease may often be seen co-existing, each pursuing its course without interfering, or being interfered with, by the other."‡ There is no reason why vaccination should not be practised in the course of hooping-cough. The matter takes as well as under ordinary circumstances, and is equally effective; while in occasional instances the hooping-cough may be suspended by the supervention of vaccinia; but still the two diseases not unfrequently co-exist.

K. *Vaccinia and Varicella.*—In some instances the action of the cow-pox virus would appear to be suspended by the supervention of chicken-pox; but, at the same time, there are many cases on record which show that the two diseases are capable of co-existing. The following are illustrations of both these phenomena:—

Illustration XXXIV.—An infant, aged thirteen months, was inoculated with vaccine matter, which did not take; but on the eleventh day an eruption of chicken-pox appeared. The latter disease was thought to be prevalent in the neighbourhood. The infant was afterwards vaccinated with effect.§

Illustration XXXV.—Two children in one family were vaccinated on the same day; in one the cow-pox went through its usual course; the other had, on the eighth day, an attack of chicken-pox, of which there were several other cases in the same house; and in the meantime there was a complete suspension of the cow-pox until the decline of the chicken-pox, about the thirteenth day, when the vaccine pustule began to be developed.‖

In the following the two diseases co-existed:

Illustration XXXVI.—Ring has recorded several instances of chicken-pox appearing a few days after vaccination, and not at all interfering with the progress of the cow-pox. Thus of one case he remarks:—"Fresh pustules appeared in succession during the formation of the cow-pock pustule; totally refuting the fallacious theory that two morbid actions at the same time are incompatible."¶

Illustration XXXVII.—In the 'Medical Gazette' (vol. ii. p. 633), a case is recorded of chicken-pox contemporaneous with cow-pox. The vesicles of the chicken-pox were very numerous and characteristic, and appeared on the eighth day of the cow-pox; the former disease having been communicated by the infant's brother, who had caught it at school.

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* Winterbottom: Medical and Physical Journal, vol. xiv. p 25.
† Mr. Gilder: Medico-Chirurgical Transactions, vol. xii. p. 186. 1823.
‡ Medico-Chir. Trans., vol. xxx. p. 126. See also Williams on Morbid Poisons, vol. i. p. 171.
§ Dr. Patterson: Medical and Physical Journal, vol. vi. p. 43. 1801.
‖ Mr. Washbourne: Medical and Physical Journal, vol. ix. p. 870. 1803.
¶ Treatise on the Cow-pox, pp. 109, 287, 524, 875, 942, 944. 1820.

L. *Rubeola and Pertussis.*—Illustration XXXVIII.—Dr. Bateman mentions a case in which the hooping-cough, of six weeks' standing, was suspended by the occurrence of measles, but returned on the decline of the latter.[*]

Illustration XXXIX.—In a Report of the Dispensary for Children in London in 1819, several cases of children are mentioned, in whom well-marked measles appeared while they were under the complete influence of hooping-cough of three weeks' standing. In three or four days the measles disappeared, leaving the patients in the same state as regards the hooping-cough.[†]

Illustration XL.—Dr. Williams saw three cases in which hooping-cough and measles co-existed, all of which proved fatal.[‡]

M. *Variola, Rubeola and Pertussis.*—Illustration XLI.—Ring mentions upwards of a dozen cases in which the small-pox, the measles, and the hooping-cough, all three co-existed, and all three ran their course together.[§]

N. *Scarlatina and Rubeola.*—The co-existence of these two diseases is probably not an uncommon occurrence.

Illustration XLII.—In an account of the diseases prevalent at Paris in 1800, we are told that scarlatina was frequently observed complicated with measles, and that such cases were always of extreme severity.[‖]

Illustration XLIII.—During the year 1833, in which many cases of both measles and scarlet fever were admitted into the Hôpital des Enfans Malades, at Paris, numerous instances were observed by M. Guersent, in which the two diseases co-existed.[¶]

It is not so generally known as perhaps it ought to be, that an affection partaking of the characters of both measles and scarlet fever is of very frequent occurrence upon the Continent, and is not uncommon in our own country. It is characterized by an eruption appearing about the end of the third day, at first resembling that of measles, and subsequently that of scarlet fever, by a combination of the morbillous coryza with scarlatinous angina, and by a subsequent desquamation of the cuticle, in the form of fine branny scales. This is the disease spoken of by German writers under the terms *Rötheln* and *Rubeola.* Under the latter name it was accurately described by Hildenbrand.[**] The rubeola, then, of German writers is not the same disease as we in England call rubeola. Before the time of Sauvages and Cullen, the term *morbilli* was always applied to the measles even in this country, and the substitution of *rubeola* by those nosologists has created no small confusion. Other designations have been applied to this German rubeola or *rötheln*, such as *morbilli scarlatinozi*, *scarlatina morbillosa*, *scarlatina hybrida*, *bastard measles*, *bastard scarlatina*, &c., all of which plainly indicate that the disease is very different from English measles. Much difference of opinion exists as to the precise nature of this affection, and observations are still wanting in order to form an accurate decision. Partaking of the characters of both measles and scarlet fever, and occurring, as it generally does, during contemporaneous epidemics of these two affections—and more especially during the transition period, when an epidemic of the one is immediately followed by an epidemic of the other—it has naturally been considered by many as a hybrid disease;[††] and seeing that we can now no longer deny the possibility of something approaching to a hybrid between any two of the exanthemata, this view is perhaps the more probable. On the other hand, it must be allowed that

[*] Diseases of London, p. 91. [†] London Medical Repository, vol. xiii. p. 90. 1820.

[‡] On Morbid Poisons, vol. i. p 808. [§] Treatise on the Cow-pox, pp, 107, 267, 1029.

[‖] Recueil Périodique de la Soc. de Méd., tome viii. p. 859. 1800.

[¶] Gazette des Hôpitaux, vol. viii. p. 84. 1834.

[**] Institutiones Practico-medicæ, vol. iv. p. 414. 1833.

[††] Hildenbrand, op. cit.; and Copland's Dic. of Pract. Med., vol. iii. p. 655.

arguments of no small weight have been adduced to prove that *rötheln* is a specific disease distinct from both measles and scarlet fever. The principal of these are the following: 1. It is said that we may have epidemic of *rötheln* alone, without either measles or scarlet fever: and 2. It is alleged that previous attacks of either true measles or scarlet fever confer no immunity from *rötheln*, while in one case I find that an attack of *rötheln* was followed in four months by simple scarlet fever. For further information upon this interesting subject I must refer to the work of Hildenbrand already quoted, to the article *Rubeola* in Dr. Copland's 'Dictionary of Practical Medicine,' and to two papers giving an account of epidemics of the disease in this country—one by Dr. Robert Paterson, of Leith,* and the other by Dr. George Balfour, of Cramond.†

O. *Typhus and Scarlet Fever.*—I am not aware of any instance in which the eruptions of the true typhus and of scarlet fever have co-existed in the same individual. It is, however, by no means unusual for patients admitted into the London Fever Hospital with either typhus or scarlet fever, to contract the other affection before their discharge. I have notes of four cases in which patients admitted with typhus took scarlet fever subsequently—one on the fourteenth day of the primary convalescence, a second on the tenth, a third about the seventeenth, and a fourth about the seventh. Again, I have notes of five cases, in which patients admitted with scarlet fever contracted typhus. In one case there appeared to be an interval of five weeks between the two diseases; in two other cases, a fortnight intervened: in a fourth, ten days; and in a fifth, only three or four days. This last case deserves more especial mention.

Illustration XLIV.—A boy, aged four years and a half, was admitted into the London Fever Hospital with all the usual symptoms of scarlet fever, including well-marked eruption and sore throat. On the fourth day, the rash was well out, but from this date it continued to fade, and on the eighth day the boy was convalescent. Three or four days later, the febrile symptoms returned, and on the fourteenth day from the first commencement of the scarlet fever, a typhus rash made its appearance. Ten days later, he had anasarca, lumbar pain, and scanty urine, containing albumen. The anasarca and albuminous urine continued for about a week, after which the patient recovered.

This patient, then, would appear to have gone through an attack of typhus, before the system had thrown off the scarlet fever poison.

P. *Scarlatina and Typhoid or Pythogenic Fever.*—No cases have hitherto been published showing the possibility of these two diseases co-existing. In the London Fever Hospital it is far from rare for a patient labouring under the one disease to be exposed to the contagion of the other, and many instances have occurred in which a patient admitted with the one has taken the other while in the hospital. This remark applies more especially to scarlet fever supervening upon pythogenic. This, indeed, is not to be wondered at when we remember that although pythogenic fever may, under certain circumstances, be communicated from the sick to the healthy, it is very rarely so, and is probably never *contagious* in the ordinary acceptation of that term. During a period of ten years I have been able to find the notes of only one case, in which a patient admitted with scarlet fever contracted pythogenic fever while in hospital. In this case the pythogenic fever supervened during convalescence, on the 26th day from the date of commencement of the scarlet fever, and on the thirteenth day after admission into hospital. Moreover, it is to be remembered in reference to such cases, that a delicate scarlet tint of the skin sometimes precedes for two or three days the eruption of the rose spots of pythogenic fever, and may cause the disease at first to simulate scarlet fever. Dr. W. Jenner has recorded an instance, in which such a case was sent into the Fever Hospital with a certificate from a physician of eminence, that the disease was scarlet fever. After a few days, the scarlet rash disappeared, and was followed by rose spots.‡ The etiology of such cases deserves closer investigation than it has yet received. It is not impossible that some of them at all events may be examples of the co-existence of the two exanthemata, and in Dr. Jenner's own case it is to be noted that sore throat was a prominent symptom, while diarrhœa and epistaxis (both symptoms of pythogenic fever) existed during the presence of the scarlet rash.

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* Edinburgh Medical and Surgical Journal, vol. lii. p. 381. 1840.

† Edinburgh Medical Journal, vol. ii. p. 717. 1856-7.

‡ Medical Times, vol. xxii. p. 277. 1850.

On the other hand, instances of scarlet fever supervening upon pythogenic have been by no means rare, and are not open to the same fallacy as I have pointed out may be applicable when the sequence of events is reversed. I have notes of nine such cases, and in four at least of these the eruptions of the two diseases were present at one and the same time. With regard to one of the cases, my notes are very imperfect, and I am unable to state at what period of the pythogenic fever the scarlet fever supervened. In the second and third cases the scarlet fever appeared in the third week of convalescence, and five weeks after admission, and in one of these cases the scarlet fever was followed by enlargement of the submaxillary glands, general dropsy, and albuminuria, and during the persistence of these symptoms, about the thirtieth day from the supervention of the scarlet fever, well-marked variola showed itself. In a fourth case, the scarlet fever supervened nine days after admission, and on the twenty-first day of the primary fever. It was followed by glandular swellings and discharge from the ears, and proved fatal. No mention is made of rose spots after the appearance of the scarlet rash, but diarrhœa, which had been a prominent symptom before, still continued. In a fifth case, scarlet fever appeared six days after admission, and on the sixteenth day of the primary fever. Rose spots were noted three days before the appearance of the scarlet rash, and it is not impossible but that they existed afterwards. In all these cases, as well as in those about to be mentioned, the usual symptoms of scarlatina, in addition to the rash, were present. In the four following cases, the eruptions of pythogenic and scarlet fever existed simultaneously.

Illustration XLV.—A girl, aged fourteen and a half, was admitted, Oct. 20th, 1856, with all the usual symptoms of pythogenic fever, including headache, sickness, diarrhœa, &c., having been ill five days before admission. On the ninth day eight or nine rose spots made their appearance, and for six days similar spots continued to come out in successive crops. On the thirteenth day six fresh ones were noted. On the fourteenth day, eight days after admission, a scarlet rash appeared all over the body, which on the subsequent day had assumed all the characters of perfect scarlatina. No fresh spots were noted after the fourteenth day, but on the sixteenth one or two of the old ones still remained, surrounded by the scarlet rash. Simultaneous with the appearance of the rash there were increased rapidity of pulse, sore throat, and the characteristic tongue of scarlet fever. In the same ward there were many cases of scarlet fever. The patient recovered.

Illustration XLVI.—A policeman, aged twenty-three, was admitted from a house from which several other cases of pythogenic fever had come, on July 26th, 1857, having been ill four days. His symptoms were vertigo, quick pulse, tympanitic abdomen, gurgling in the iliac fossa, and watery diarrhœa. On the fifth day rose spots began to appear, and on the eighth upwards of four hundred were counted on the anterior aspect of the body. On the seventeenth day the spots still continued numerous. On the twentieth there was great aggravation of the febrile symptoms; the rose spots had nearly all disappeared, but a few still remained, and there was in addition a scarlet rash, having all the characters of that of scarlatina. The throat was sore and the fauces very red. The diarrhœa still persisted. This rash disappeared after a few days, and the patient recovered.

Illustration XLVII.—A policeman, aged twenty-three, was admitted November 9th, 1857, having been ill for three weeks with well-marked symptoms of pythogenic fever, including red, glazed, and fissured tongue, tympanitic abdomen, urgent watery diarrhœa, and rose spots. These rose spots on admission were so numerous as in some places to run into one another. Eight days after admission the rose spots, which for some days before had been increasing in number, still continued abundant, and the diahrrœa persisted. There was, in addition, a general scarlet rash, identical with that of scarlet fever, a strawberry-red tongue, with large papillæ, sore throat, and injected fauces. Two days later the rose spots continued in great numbers, and the scarlet rash persisted. Two days after this the scarlet rash was fading, but the rose spots continued out for a few days longer. A week after the disappearance of the scarlet rash there was copious desquamation. The patient made a good recovery.

Illustration XLVIII.—A boy, aged fourteen, was admitted, August 25th, 1858, from a house in which there had been other cases of pythogenic fever. He had the symptoms of pythogenic fever in a mild form, and with no very well-marked abdominal symptoms. Rose spots made their appearance on the thirteenth day, in sparing numbers, and continued coming out in successive crops. On the twenty-second day there were still some rose spots, and also a general scarlet rash having all the general characters of that of scarlet fever. The tongue was moist, with thick white fur and large red papillæ; the throat sore; the tonsils enlarged and red, and coated with a white membraniform deposit. On the same day the pulse had risen from 72 to 132, and the temperature under the tongue from 99° to 104° Fahr. After three or four days both the eruptions disappeared. On the twenty-fifth day the tonsils were so large as almost to meet, and the tongue was clean, red, and of a strawberry aspect. On the twenty-seventh day desquamation commenced. Convalescence was delayed by tedious swellings in the neck, one of which terminated in abscess. When this boy was admitted, a scarlet fever patient lay in the adjoining bed, and there were many other cases in the same ward.

Q. *Typhus and Pythogenic Fever.*—The doctrine of the compatibility of two of the exanthemata has an important bearing upon that of the non-identity of typhus and pythogenic fever. They who maintain that the poisons of the two fevers are identical, have appealed triumphantly to certain cases in which they have observed the eruptions of the two fevers to co-exist. Now, allowing for a moment that the facts in all these cases have been correct, the conclusions which have been drawn are based upon a doctrine which is utterly fallacious. The co-existence of two eruptions no more implies an identity of the two diseases, than it does in the case of variola and scarlet fever, or of scarlet and pythogenic fever. But there can be little doubt that, in the majority of cases, the facts themselves, from the manner in which they have been described, must be viewed with no small distrust. On few subjects does so much confusion prevail in the profession as with regard to the eruptions of continued fevers. A very common mistake is to imagine that petechiæ constitute the characteristic eruption of typhus, a mistake which has been strengthened by "petechial fever" being one of the appellations applied to the disease; and it has been argued, from a patient presenting both "rose spots" and "petechiæ," that the eruptions of pythogenic fever and typhus have co-existed. But petechiæ do not constitute the characteristic eruption of typhus; and they are met with in the course of pythogenic fever, in the same way as they show themselves in the course of variola, scarlatina, and many other affections. All they who have had much practical experience in studying both typhus and pythogenic fever, will admit that it is excessively rare to find the *measly eruption* characteristic of the one, co-existing with the *rose spots* characteristic of the other. In my essay upon the Etiology of Continued Fevers, published in the 'Medico-Chirurgical Transactions' (vol. xli. p. 275), I expressed an opinion that such a co-existence was possible; but I maintained then, as I do now, that no argument could be based upon such a co-existence as to the identity of the typhus and pythogenic poison, any more than we should employ a similar argument to show that variola and scarlet fever, or scarlet fever and pythogenic fever, were one and the same. I shall now proceed to detail the facts which testify to the possibility of typhus and pythogenic fever co-existing.

Such facts might be naturally looked for under circumstances in which a patient labouring under the one disease has been exposed to the contagion of the other, as for example in the London Fever Hospital. When the doubtfully contagious character of pythogenic fever (already alluded to) is remembered, it will not be wondered at that patients admitted with typhus have seldom contracted the former disease. During a period of ten years I have only been able to find the notes of two such cases. One was that of a female, aged twenty-one, who, with seven others of the same family, was admitted with well-marked typhus, and who, in the third week of convalescence, had an attack of pythogenic fever, the symptoms of which, however, were mild and not very characteristic. The second case will be shortly alluded to. On the other hand, it has been by no means rare for patients admitted with pythogenic fever to contract typhus during their stay in hospital. But, in most cases, this has been in the fifth to eighth week of convalescence from the first fever, and two or three weeks after the patients have been removed to the convalescent ward, a circumstance which is explained by the patients in the convalescent ward being thrown into more intimate relations with one another, and by typhus being avowedly more contagious during convalescence than during the height of the disease. In the following instances, however, the eruptions and other symptoms of the two diseases were almost contemporaneous.

Illustration XLIX.—A female, aged twenty-two, had an attack of pythogenic fever, which was attributed to the putrid emanations from a bad drain. She was admitted into the London Fever Hospital. The primary attack lasted three weeks. After a fortnight she had a relapse, with a return of the "rose spots," and the day after this there was a subcutaneous "typhus mottling," along with drowsiness, heaviness, and other symptoms of typhus.

Illustration L.—A male, aged twenty-five, was admitted with well-marked pythogenic fever. On the 27th day there was a great aggravation of the symptoms, with much headache and stupor, and in addition to several rose-coloured spots there was a distinct subcutaneous mottling. The diarrhœa still persisted. Four days later the subcutaneous mottling had become developed into a well-marked typhus rash. The patient recovered.

Illustration LI.—A female, aged twenty-seven, was admitted on the third day of an attack of typhus. The rash began to fade about the fifteenth day, but there was no abatement of the general febrile symptoms. On the eighteenth day there was watery diarrhœa, tympanitic abdomen, and several rose spots. The latter symptoms continued for about a fortnight, after which the patient gradually recovered.

It not unfrequently happens that patients are exposed to the poisons of both typhus and pythogenic fever before their admission into a hospital. In my researches, elsewhere published, I have endeavoured to show that the poison of pythogenic fever is generated in the emanations from decaying animal matter, and that of typhus by the respiration of an atmosphere charged with the exhalations of living bodies, although in the majority of cases the latter disease is propagated by contagion. Now, if a certain poison can generate one group of symptoms, and another poison generate another, surely it is but reasonable to expect that a combination of the two poisons may give rise to a morbid condition of an intermediate character, without its being necessary to conclude from the existence of such a hybrid affection that the first two morbid conditions have been merely different manifestations of the same poison.

The three following instances were made the subject of repeated and most careful observations by myself:

Illustration LII.—In December, 1857, a girl, aged sixteen, was admitted into the Fever Hospital from 17, Windmill-row, Lambeth; ill a week. Her body was covered with an unmistakeable mulberry (typhus) rash, and she presented all the usual symptoms of typhus—dry, brown tongue; confined bowels; heavy confused expression; small pupils; and low, wandering delirium. The case attracted particular notice, as typhus was at that time very uncommon. Two days after the symptoms underwent a complete change. The mulberry rash (which was certainly not the scarlet rash which occasionally precedes the eruption of pythogenic fever) faded, and was succeeded by rose spots, which came out in successive crops for more than a week, and were accompanied by diarrhœa and abdominal tenderness. The tongue became moist and red; the pupils, dilated; and the drowsiness and wandering vanished. This girl was a hawker; for some weeks had been very destitute, and a fortnight before she had slept for two or three nights at another house, in the same bed with a girl who had "fever." This second girl, with her mother and sisters, was admitted into the Lambeth Workhouse; but the father and brother were admitted into the Fever Hospital with well-marked typhus. On the other hand, Dr. Odling, the officer of health for Lambeth, informed me that the courtway in front of No. 17, Windmill-row, was badly paved and badly drained; and that, although the cesspools in the house had been done away with, the habits of the inmates had rendered the privy arrangements as insalubrious as before. This girl was therefore exposed both to the contagion of typhus, and to the causes which there is reason to believe generate pythogenic fever.

Illustration LIII.—A man, aged twenty, a street hawker, was admitted into the Fever Hospital, October 15th, 1858, from 7, Feathers' court, Drury-lane. This man had diarrhœa, and the characteristic tongue of pythogenic fever; but on the other hand (what is more peculiar to typhus), he had delirium coming on so early as the third day. From the eighth up to the twenty-second day he had distinct "rose spots" coming out in successive crops; and, in addition, there was from the seventh to the eleventh day a faint mottling on the arms and trunk, precisely similar to the eruption of typhus. This man's brother and sister were both laid up at home with fever. Both had diarrhœa, and in both the body was covered with an eruption compared by the mother to that of measles. Now, pythogenic fever was at the time very prevalent in London, so that it was not to be wondered at if the patients should be exposed to its exciting causes; but typhus was scarcely to be met with, and it became interesting to determine whether these patients had been exposed to the causes which are known to generate it. In the first place it was ascertained that they were very destitute, and destitution is known to be the great predisposing cause of typhus; and secondly, the room in which they lived was over-crowded—five adults sleeping in a room seventeen feet square and eight feet high, with one door and one window, so that, making no allowance for furniture, each individual had only about three hundred cubic feet of space. There were no means of ventilation. The single window was seldom or never opened.

Illustration LIV.—A girl, aged six, was admitted into the Fever Hospital, September 13th, 1858, from 3, Horse-shoe-court, Cow-cross, Holborn. On the eighth day of her illness rose spots were observed, perfectly characteristic, lasting for a few days and then succeeded by others up to the eighteenth day. On the ninth day, and lasting for four days, in addition to these rose spots, there was a distinct subcutaneous mottling, not disappearing on pressure, and precisely resembling the eruption of typhus. The general symptoms more resembled those of typhus than of pythogenic fever. There was no diarrhœa, and throughout there was much tendency to stupor and slight delirium. Three other children from the same family were admitted about the same time into St. Bartholomew's. All of them, according to the mother, had a rash resembling that of measles; but all had also diarrhœa, and in one boy rose spots were noted during his stay in St. Bartholomew's. Pythogenic fever at the time was very prevalent in London. Typhus was almost unknown; yet in the house from which these patients came the causes known to generate typhus existed in a marked degree. The house itself was situated at the top of a closed court, and the room in which the father, mother, and five children lived and slept was at the top of a narrow stair in this house, and measured fourteen feet in length, thirteen and a half in breadth, and seven and a half in height, a space which, even making no allowance for furniture, allowed only two hundred cubic feet of air to each individual.

Lastly, it seems not improbable that a co-existence of two different diseases may have accounted for the anomalous symptoms observed in the two following instances:

Illustration LV.—An outbreak of fever occurred in autumn, 1857, in Dudley-street, Paddington, in which I am assured by Dr. Sanderson* that there were some cases which presented the characters of both typhus and pythogenic fever, including the presence of the two eruptions. I have elsewhere shown that in Paddington there are various causes to account for the generation of pythogenic fever, and that typhus is there extremely rare. It becomes interesting, then, to ascertain under what circumstances typhus, or something resembling it, may there originate. Now, in the houses in which these "mixed cases" occurred, the two causes which I have supposed to generate typhus and pythogenic fever were present in a marked degree. First, the residents were principally dustwomen, and the houses were daily stocked with selections from the street-sweepings of the metropolis, such as old grease-pots, &c., materials sufficiently prone to decomposition in hot weather. But, secondly, these two houses were overcrowded to such a degree that compulsory measures had to be adopted to diminish the number of inmates. Cases of fever occurred at the same time in other houses of the same street, which were not overcrowded; but these were pure examples of pythogenic fever.

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* Dr. Sanderson has had ample opportunities of studying the two fevers at the London Fever Hospital.

Secondly, Illustration LVI.—M. Landouzy[*] has given an account of a remarkable fever which prevailed at the gaol at Rheims, in the autumn of 1840. Many of the symptoms during life, including the eruption, were those of typhus; but the intestines after death presented the lesions characteristic of pythogenic fever. Now, from the locality and the season of the year, one would have expected the latter; and in addition to these causes, we are informed that there was a most disagreeable odour in the gaol (proceeding from the grease of the woollen fabrics manufactured by the prisoners ?). That the fever was really pythogenic fever is proved by the lesions found after death. On the other hand, a cause was not wanting to account for the symptoms of typhus during life; and, it must be remembered, that a copious mulberry rash would entirely mask a few rose-coloured spots, even if these were present. The circumstance to which the fever was mainly attributed, was the over-crowding of the prisoners. The number which the gaol was calculated to hold was from one hundred and thirty to one hundred and fifty; but a month or two previous to the outbreak of the fever, this had been raised to one hundred and ninety.

The illustrations cited in this essay might have been greatly multiplied; and it might have been shown that the paludal poison, and the poisons of syphilis, the plague, cholera, and influenza, are compatible not only with one another, but also with the poisons of the diseases already enumerated. Enough, however, I trust, has been done to prove that the doctrine of the incompatibility of two or more contagious diseases is erroneous; and consequently that, upon the mere occasional co-existence of the peculiar eruptions of typhus and pythogenic fever, no argument can be based as to the identity of the poisons of these two diseases.

Art. III.

On some Points in the Clinical History of Asthma. By Hyde Salter, M.D., F.R.S., Assistant Physician to Charing Cross Hospital.

PHENOMENA OF A PAROXYSM.

Premonitory and initiatory symptoms—Drowsiness, dyspeptic symptoms, headache, excitability, profuse diuresis, neuralgic pains—Time of attack, the early morning; why ?—Description of access of paroxysm—Appearance of the asthmatic in the height of the paroxysm—Pulse—Itching under the chin—Muscular phenomena—Enlargement of capacity of chest—Auscultatory signs—Conclusions.

In considering the phenomena of asthma, I shall take first the phenomena of a paroxysm, and then the phenomena of the disease generally; and I shall adopt this order because the phenomena of the paroxysm are so much more pronounced and marked, and constitute so much the body of the malady—while those of the disease generally, in opposition to those of the paroxysm, are rather the phenomena of the intervals, and consist of certain permanent conditions influencing the paroxysms, or produced by them.

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[*] Archives Gén. de Méd., troisième série, vol. xiii. p. 7.

As in epilepsy we have premonitory symptoms, in the form of the *aura epileptica*, spectra, and other subjective phenomena; then the establishment of the paroxysm; then those conditions of the nervous and muscular systems which constitute its climax; and then its abatement and the post-epileptic sleep: so in asthma we have certain precursory symptoms, and then the attack in its accession, perfect establishment, and departure.

The precursory symptoms of a fit of asthma are liable to great variety in different individuals; some persons never experience any, but having been guilty of some imprudence, or the regular period of an attack having recurred, the seizure of the dyspnœa upon them is the first indication of its approach. But I think that the majority of asthmatics *do* know that an attack is coming on them by certain feelings in themselves, or certain conditions of which they are aware. These symptoms generally show themselves on the night previous to the attack; but in some cases for a longer time. The patient will feel himself very drowsy and sleepy, will be unable to hold his head up or keep his eyes open, and that without having undergone any particular fatigue or done anything that could account for it.* I remember one case in which this was very strikingly marked, the asthmatic always knew when he was going to be ill the next day by the extreme drowsiness that overpowered him at night; he would go sound asleep over his reading or writing, or whatever he might be engaged in, and that at an early hour of the evening. It was in vain for him to rouse himself, in spite of all his efforts, and in spite of the prophecies of those about him that he was going to be ill, and his own convictions of what awaited him, to bed he must go. And probably any resistance of these feelings would have been of no avail, and would neither have postponed nor modified the attack; the asthma was not the *result* of the heaviness, but the heaviness merely indicated the approach of the asthma; it was the commencement of that particular nervous condition of which the succeeding respiratory phenomena were but the more complete development; in fact, it must be looked upon as an integral part of the paroxysm. I find this precursory drowsiness to be the commonest of all the premonitory symptoms of asthma.

Others, again, know by extreme wakefulness and unusual mental activity and buoyancy of spirits, that an attack awaits them; and I knew one case in which an attack of ophthalmia always ushered in the asthma: the man was liable to inflammation of his conjunctiva; it was always worse before his attacks than at any other time, and he invariably knew by the state of his eyes when he was going to suffer a paroxysm. It might be thought that this was a case of mere catarrh, that the asthma was caused by the inflammation of the eyes creeping down through the nasal mucous membrane into the air-passages; but this was clearly not the case—there was no coryza, no bronchitis—the ophthalmia was strumous, and I believe that an exacerbation of the strumous cachexia, a more debilitated, and therefore a more irritable condition of system, was the cause alike of the inflammation of the conjunctiva and the spasm of the air tubes. At other times the precursory symptoms are connected with the stomach, and consist of loss of appetite, flatulence, costiveness, and certain peculiar uneasy sensations in the epigastrium; but here I think we have something more than mere premonitory signs; I think the relation of these symptoms to the spasm which follows is often that of cause and effect.

Of all the circumstances attending the commencement of an asthmatic paroxysm, none is more constant than the time at which it occurs. This is almost invariably in the early morning, from three to six o'clock. There are some cases in which the usual time is the evening—some just after getting into bed, before going to sleep, and some in which there is no particular time, but the attack may come on at any hour of the day or night, on the occurrence of some exciting cause, such as a fit of laughter, a full stomach, change of wind, &c. In nineteen cases out of twenty, however, the dyspœna first declares itself on the patient's waking in the morning—or what is much more common, wakes him from his sleep when he has had but half a night's rest.

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* Floyer was perfectly aware of this premonitory sign, having noticed it in his own person. "There appears," he says, "a great dulness and fullness of the head, with a slight headache, and great sleepiness on the evening before the fit."

Now I think there are two reasons for the attack coming on at this time; one is the horizontal position of the body—the other the greater facility with which sources of irritation, and, indeed, any causes of reflex action, operate during sleep than during the hours of wakefulness. The first cause acts thus: whèn a person lies down and goes to sleep, the recumbent position favours the afflux of blood to the right side of the heart, and therefore to his lungs; in addition to this, the position of the body places the muscles of respiration at a disadvantage; add to this, the diminished rate at which the vital changes go on during sleep; lastly, add to this the lowered sensibility of sleep which prevents the arrears into which the respiration may be getting from being at once appreciated; and I think we have a sufficient explanation both of the time at which the attack generally comes on, and of the amount of dyspnœa that may accumulate before the asthmatic is roused from his slumbers. He goes to bed quite well, perhaps; the position of his body, and the torpor of sleep soon throw his lungs into arrears, and they become congested; this goes on for some time, gradually increasing, without producing any particular effect: by and by, however, this pulmonary congestion reaches such a pitch that it becomes itself a source of great local irritation, and gives rise to asthmatic spasm; this, in its turn, cuts off the supply of air and increases the congestion, and thus the congestion and the asthma—the cause and the effect—mutually augment one another, till they produce such an amount of dyspnœa as is incompatible with sleep, and the patient suddenly wakes with all the distress of an asthmatic paroxysm full upon him. Now in this case all the causes I have mentioned act together, but we know that each individually has its separate agency in producing the effect, because by removing any one of the causes, you may prevent the result; we know that the position of the body has to do with it, because an extra pillow may prevent the attack; we know that the disadvantage at which the muscles of respiration are placed during sleep has to do with it, because the attack may in some cases be prevented by laying the head on the arm, so as to make the shoulder a fixed point from which the accessory muscles of respiration can act.[*] Lastly, we know that the greater proneness to excitomotory action during sleep has to do with it, because some asthmatics do not dare to go to sleep after the commission of any imprudence, whereas they may be guilty of any irregularity with impunity if they only keep awake for some time afterwards. I know one asthmatic who often sits up half the night after taking a supper (breathing perfectly freely), because he knows that if he goes to sleep his asthma will come on him immediately; but by thus sitting up till his supper is fairly digested, his stomach empty, and the source of irritation thus removed, he may go to sleep fearlessly and have a good night's rest.

One cannot help seeing the striking resemblance that exists between this and the orthopnœa of cardiac disease; only in the one case the extreme dyspnœa is brought about by the obstructed circulation through the lungs; in the other, by the sparing amount of air admitted through the obstructed bronchi; in both, the congestion of the lungs is first induced by the position of the body, and the sense of arrears—the *besoin de respirer*—blunted, and the respiratory efforts postponed, by the insensibility of sleep. But in the orthopnœa the violent and extraordinary respiration that succeeds the starting from sleep soon re-establishes the balance; whereas in the asthma the constriction of the bronchi which persists after waking, precludes the admission of the necessary amount of air, and the dyspnœa remains.

One curious circumstance with regard to time is that it may be varied according to the intensity of the cause—the more intense the source of irritation the shorter will the sleep be before the asthma puts a stop to it. I once knew an asthmatic who was always awaked by his disease with an earliness proportionate to the size of the supper he had taken; certain airs disagreed with him as well as food before sleeping, and if the two causes acted conjointly he would wake with asthma much earlier than if they acted singly: thus if he went to a place that did not agree with him, he might wake about five o'clock with his asthma; the same if he ate a supper in a place that *did* agree with him; but if he ate a supper when staying at a place that did *not* agree with him, he would get no sleep after two or three o'clock; this may seem singular and an over refinement, but it is strictly true; I have watched it over and over again.

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[*] An asthmatic friend, with whose case, I am familiar, tells me that he always sleeps much better on a sofa than a bed; no amount of bolstering can impart to a bed the comfort and ease of a sofa. This he attributes to the fixed support that the side of the sofa affords on which to rest his arm, and the leverage thereby furnished for the accessory muscles of respiration.

How essentially characteristic of the disease this occurrence of the attack in the early morning is—how inherently a part of it—is shown by the fact that, in the great majority of cases, at this time and at this time alone will the attack come on, at whatever time in the twenty-four hours the exciting cause may be applied. For instance, in some cases over-exercise will bring on an attack, in many cases that have come under my care this has been so; but although the asthma was in these pretty sure to follow such over-exertion, it never came on immediately, never till the next morning; the exertion might be followed at the time by a little shortness of breath not much exceeding that of a healthy person, which would speedily and entirely disappear, and the patient would pass the rest of the day, and go to bed, in perfect health; but as surely as possible he would be awoke the next morning at the usual time with his asthma. And it would make no difference at what time of the day the over-exertion had been taken, morning or evening; at the stated time and at that only, neither earlier nor later, would its results declare themselves. Now here we have an exciting cause actually and inevitably bringing on an attack, but powerless to do so, its effect suspended, as it were, and laid dormant, until the characteristic time had come round. Nothing could show, as I think, more clearly than this both the tenacity with which the disease sticks to its favourite time of occurrence, and its essentially nervous nature. For through what but through the nervous system could such exciting causes maintain their influence suspended, and, finally, produce their effects after so long an interval, during which the respiratory and circulatory systems had been in a normal and tranquil condition?

I have always believed that this morning occurrence of asthma is the result of the causes I have mentioned, the horizontal position and sleep, and the conditions of circulation and respiration that they induce, and I cannot but believe that this is its true explanation. But about six months ago a case came under my observation which seemed to imply that this feature of asthma was an essential part of its natural history, and not dependent on external circumstances. The case was that of a night porter, whose duties compelled him to turn day into night and night into day. He went to bed at seven o'clock in the morning, and slept through the early part of the day. But though the ordinary times of sleeping and waking were thus transposed, the asthma came on at the usual time, from five to six in the morning, towards the end of his vigil, when he was up and awake, and when none of the determining causes that I have mentioned could have been in operation. If the asthma had come on in this case at a time having the same relation to sleep and recumbency as in ordinary cases, it would have made its appearance about eleven or twelve o'clock in the day. This case certainly looks as if the particular period that the paroxysm affects depended on some inherent and inveterate habit of the disease. But the teaching of a single case like this is not to be taken in contravention of reason, or unsupported by further evidence. It is, however, I think, worth putting on record, and worth bearing in mind.

One of the symptoms frequently attendant on the first stage of an attack of asthma is *profuse diuresis;* the patient will half fill a chamber-pot with pale, limpid water, exactly like the urine of hysteria. This abundant secretion generally comes on soon after the asthma commences, but I have known it come on so early that the patient was awakened from his sleep by the distension of his bladder, when the difficulty of his breathing was only just commencing. It generally lasts for the first three or four hours, and then ceases altogether. I believe the secretion of this abundant white urine to be of the same nature as the hysterical urine that it resembles—that it is nervous; and I regard it, as I have shown elsewhere,* as one of the many evidences of the nervous nature of asthma.

Another early symptom which I have often observed is *neuralgic pains*—a deep-seated aching in the limbs and joints; the testicles, too, are very apt to be affected with it; and I knew one case in which the testicle and the tibia, from the knee to the ankle, were always affected on the same side, sometimes the right testicle and tibia only, sometimes the left, sometimes both; but always the tibia and testicle on the same side. The pain is constant, deep-seated, and wearying.

Let us now consider the phenomena by which an attack of asthma is generally ushered in. The patient goes to bed in his usual health, with or without premonitory symptoms; he goes to sleep and sleeps for two or three hours; he then becomes distressed in his breathing, and dreams, perhaps, that he is under some circumstances that make his respiration difficult; while yet asleep the characteristic wheezing commences, sometimes, without disturbing the patient himself, to such a degree as to wake those in the same or an adjoining room, as if a whole orchestra of fiddles were tuning in his chest; perhaps he half wakes up and changes his position, by which he gets a little ease, and then falls asleep again, but only to have his distress and dreams renewed, and again partially to wake and turn. Shortly the increasing difficulty quite wakes him, but only perhaps for a minute or two; he sits up in bed in a miserable half-consciousness of his condition, gets a temporary abatement, sleep overpowers him, and he falls back, to be again awoke and again sit up; and so this miserable fight between asthma and sleep may go on for an hour or more, the dyspnœa arousing the sufferer as soon as sleep is fairly established, and sleep again overpowering him as soon as the wakefulness and change of position have a little abated the extremity of his sufferings. By and by the struggle ceases, sleep is no longer possible, the increasing dyspnœa does not allow the patient to forget himself for a moment, he becomes wide awake, sits up in bed to lie back no more, throws himself forward, plants his elbows on his knees, and with fixed head and elevated shoulders labours for his breath like a dying man.

When once the paroxysm is established, the asthmatic offers a very striking and very distressing spectacle. If he moves at all it is with great difficulty, creeping by stages from one piece of furniture to another. But most commonly he sits fixed in a chair, immovable, unable to speak, or even, perhaps, to move his head in answer to questions that may be put to him. His back is rounded and his gait stooping; indeed, his whole figure is deformed. His chest, back, shoulders and head are fixed; he cannot even turn his head from side to side, but when he looks from object to object merely turns his eyes, like a person with a stiff neck; his shoulders are raised to his ears, and his head thrown back and buried between them.

In order the better to raise his shoulders, and at the same time spare muscular effort in doing so, his elbows are fixed on the arms of his chair, or his hands planted on his knees, or he leans forward on a table, or sits across a chair and leans over the back of it, or he stands grasping the back of a chair and throwing his weight upon it,† or leaning against a chest of drawers or some piece of furniture sufficiently high to rest his elbows on in a standing position. At every breath his head is thrown back, his shoulders still more raised, and his mouth a little opened, with a gasping movement; his expression is anxious and distressed; the eyes are wide open, sometimes strained, turgid, and suffused; his face is pallid, and, if the dyspnœa is extreme and long, slightly cyanotic; the labour of breathing is such that beads of perspiration stand on his forehead, or even run in drops down his face, which his attendant has constantly to wipe. He is so engrossed with his sufferings and the labour of breathing, that he seems unconscious of what is going on around him; or else he is impatient, and intolerant of the assiduities of those who are in vain trying to give him some relief.

If the bronchial spasm is protracted and intense the temperature falls; the oxygenation of the blood is so imperfectly performed, from the sparing supply of air, that it is inadequate to the maintenance of the normal temperature; the extremities especially get cold, and blue, and shrunk; I have known the whole body deathly cold, and resist all efforts to warm it, for four hours. But while the temperature is thus depressed, the perspiration produced by the violent respiratory efforts may be profuse, so that the sufferer is at the same time cold and sweating. It is this union of coldness and sweat, combined with the duskiness and pallor of the skin, that gives to the asthmatic so much the appearance of a dying man, and that even sometimes makes the initiated fear that death is impending.

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* Med.-Chir. Review, July, 1858.

† I have known a patient stand in this position for two days and nights, unable to move.

The pulse during severe asthma is always small, and small in proportion to the intensity of the dyspnœa; it is so feeble sometimes that it can hardly be felt. The explanation of this is very simple. The imperfect supply of air produces capillary arrest—partial stasis—of the pulmonary circulation; but a small quantity of blood is therefore allowed to pass on to the left side of the heart, so that the volume on which the left ventricle contracts, and which it impels into the arterial system at each pulsation, is extremely small, and barely sufficient to register itself at the wrist. That the small pulse is due to pulmonary capillary arrest, itself due to the shutting off of air from the lungs, is proved by the fact that immediately the paroxysm yields, the pulse resumes its normal volume. I have never known the small pulse absent in severe asthma; its very explanation proves that it could not be.

One curious symptom of asthma, which I have found present in a large number of cases (I am not sure it is not universally present), but which I have never seen noticed in any treatise on the subject, is *itching under the chin.* I have often known that the breathing of asthmatics was tight, and told them so, from seeing them scratching and rubbing their chins. The itching is incessant, and of an indefinite, creeping character, but although it is impossible to help scratching it, the scratching does not relieve it. It is often accompanied with the same itching sensation over the sternum and between the shoulders, especially between the shoulders. It appears the moment the first tightness of breathing is felt, and goes off when the paroxysm has become confirmed—indeed, I think it is more pronounced in those slight and transitory tightenings of the breathing to which asthmatics are so liable (as, for example, after laughing), than in regular attacks. But I think it is the most strongly marked of all in the asthma that accompanies hay-fever. The sternal and interscapular portion of this itching is, I think, of easy explanation, its distribution to the chin is less easy to understand. According to the law that the pain arising from the irritation of a viscus shall be referred to the superficies, front and back, in the middle line and at a level with the viscus (a law illustrated by the seat of the pain in stomach, bowel, and uterine disease), the seat to which the sensation from bronchial irritation is referred is the sternum and between the blade bones. Thus, in bronchitis, the raw, scraping feeling that accompanies cough is sternal and inter-scapular; so that in relation to this asthmatic itching, the fact would appear to be simply this—that while the impression on the bronchial nervous system produced by inflammation of its mucous membrane gives rise to sternal and interscapular *pain*, that produced by spasm of these tubes gives rise to sternal and interscapular *itching.* The itching of the chin must, I think, be of the same reflex character, and admit of the same explanation, but the reason of its locality is less apparent.

On stripping an asthmatic in the height of a paroxysm, an admirable example is seen of the immense array of muscles that become, on an emergency, accessory to respiration, and some idea is formed of the toil of the asthmatic, and the extremity of those sufferings that necessitate for their relief such intense labour. All the muscles passing from the head to the shoulders, clavicles, and ribs are rigid, and the head is rendered a fixed point from which they can act on their respiratory attachments. Ordinarily these muscles, such as the splenii and scaleni, have their inferior attachments fixed, and move the head and neck, but now their upper attachment is fixed, and from it they act as mediate or immediate elevators of the ribs and distenders of the thoracic cavity; and this is how it is that the asthmatic is incapable of moving his head. By the contraction of the trapezius and levator anguli scapulæ, the shoulders are raised to the ears, in order that the muscles proceeding from the shoulders to the ribs may act at an advantage as elevators of these latter. The muscles of the back are so engaged in respiration, that they cease to support the trunk, and the gait becomes stooping. At every inspiration the sterno-mastoids start out like cords, and produce by their sudden prominence a deep pit between their sternal attachments. I have already referred to the gaping descent of the lower jaw at each inspiration.

Now, what is the explanation of this? What is its mechanism? I think the *rationale* of it is this: by its endeavours to raise the scapula, the homo-hyoid muscle is strongly contracted at each inspiration, but its hyoid attachment being by far its most moveable extremity, the contraction of the muscle tends rather to draw the hyoid bone down than to elevate the shoulder; and as the elevators of the hyoid bone—the mylo-hyoid, genio-hyoid, and digastric—are firmly contracted with the view of fixing it, the drawing down of the hyoid bone also draws down the jaw, and thus is produced the descent of the jaw at each inspiration; so that this gasping movement really depends on one of the depressors of the hyoid bone being, by virtue of its scapular attachment, also an accessory muscle of respiration, and being at the same time, from the loose and floating character of its superior attachment, unable to effect that interchange of its fixed and moving points that takes place with regard to the other extraordinary muscles of respiration. In the case of other accessory muscles of respiration, either extremity can be made the fixed one, and thus render the action of the muscle respiratory or non-respiratory, according to circumstances; if the lower extremity is fixed, as is ordinarily the case, the head or neck is moved, and the muscle is non-respiratory; if the upper extremity is fixed, the shoulders or ribs are raised, and the muscle is respiratory. But the upper attachment of the homo-hyoid not being firmly fixable, the muscle cannot transfer its contractions to its respiratory extremity, and thus, though theoretically, it is not actually a respiratory muscle. This explanation, if correct, is not uninteresting, as it offers an example of the maintenance of a type of action in spite of disturbing circumstances that necessarily make the action inoperative; it is an instance, if I may so express it, of morphological physiology, bearing the same relation to function as the retention, in obedience to type, of superfluous or modified appendages does to structure. I am not sure that the other depressors of the hyoid bone do not share in the action.

Meantime all the muscles that increase the capacity of the chest are straining their utmost and starting into prominence at each inspiration; as each breath is drawn, every muscle is thrown out into bold relief, and since there are hardly any muscles of the trunk that are not mediately or immediately respiratory, the whole muscular system of the trunk may be mapped out in every part of its detail. The straining muscles are rendered all the more conspicuous from asthmatics being generally so thin.

But violent and laborious as are these respiratory efforts, they are abortive; although the muscles that should move the parietes of the chest are contracting to their utmost, no corresponding movements take place—the chest is almost motionless, its walls are fixed as in a vice, as if they could not follow the traction of their muscles; and this is really the case. This immobility, in spite of the violent action of the moving agent, is one of the most singular and striking appearances of asthmatic breathing. How different from the wide range of movement that follows even less considerable respiratory effort in one to and from whose lungs the ingress and egress of air is free!

One result of these straining efforts to fill the chest is a permanent distension of it—its walls are kept fixed in a condition of extreme inspiration. So great is this enlargement of the chest during the paroxysm, that a waistcoat that would ordinarily fit cannot be brought together by two inches. But the chest is enlarged in every way, the diaphragm therefore descends, the abdomen therefore seems fuller, and its girth is increased. This, I believe, is the principal cause of that abdominal distension of which asthmatics complain, and which is generally assigned to flatulence. As soon as the paroxysm goes off, the chest and abdomen resume their original size. I do not see that anything is gained by this distension of the chest; the only difference is that the volume of air locked up in the chest is rather larger, but no more is changed at each respiration, and it is the amount so changed, and not the quantity contained in the lungs, that relieves the demand of respiration. Air is the thing that is wanted, and inspiration is the act that ordinarily relieves that want; this keeping the chest, therefore, at a condition of extreme inspiration must be looked upon as an instinctive, but blind and abortive effort to remedy that which is irremediable.

Such being the external phenomena of the breathing of asthmatics, what are the auscultatory sounds that accompany it? They are exactly such as we should expect—exactly such as are consistent with these external phenomena, and such as imply, if the spasm is severe, an almost impassable bar to the ingress and egress of air. On applying the ear to the chest we hear—respiratory murmur *none*; and this is not because it is drowned by other sounds; if no other sounds are present it is equally inaudible; it is because the conditions of its production do not exist, because sufficient air is not admitted to generate it; just as there is no respiratory murmur in the long-drawn inspiration of hooping-cough, or beneath thoracic parietes fixed by pleurisy or intercostal rheumatism. And this suppression of the ordinary breathing sound is a proof of the depressed standard at which respiration is being carried on, and of the completeness with which air is locked out of and into the chest. The sounds that *are* heard are dry tube-sounds, large and small—rhonchus and sibilus of every variety, of every note and pitch, and in all parts of the chest, converting it into a very orchestra; but the sounds are mostly sibilant, high and shrill, resembling the chirping of a bird, the squeaking of a mouse, or the mewing of a kitten. And this smallness of sound makes me think that it is almost exclusively the smaller tubes that are the seat of the constriction, whilst the diffusion of the sounds all over the chest shows that constricted tubes exist everywhere.

There is one other fact, in relation to the sounds of asthma, that I think is instructive, and that seems to me to imply that the points of stricture are constantly changing their place, that spasm is constantly disappearing in one part and making its appearance in another, and that fact is, that the sounds are continually changing their character and site. On listening over a part of the chest where a few minutes before you heard a loud shrill sibilus you find it gone, while a part that just before was silent is the seat of a chorus of piping. Now, if the sounds were of a moist character, if they were caused by mucus, I grant that such an inference could not be drawn, for sounds so caused may be, and constantly are, suddenly removed by cough, or other dislodgment of the accumulated secretion; but in the early part of an attack of uncomplicated spasmodic asthma there is no accumulation of mucus in the air-tubes—they are dry; the narrowing of the tube, therefore, that gives rise to the musical sound, being solely dependent on bronchial spasm, solely admits of removal by the relaxation of that spasm; and the frequent cessation and change of place of the pipings shows that the spasm that causes them is transient and wandering.

The auscultation, then, of the asthmatic shows us these things:

α. The almost perfect stagnation of air in the chest, in spite of the violent respiratory efforts.
β. That the tubes are generally very small.
γ. That tubes in all parts of the chest are simultaneously affected.
δ. That the points of constriction are constantly changing place.

Art. IV.

On the Effects of Rupture of the Internal and Middle Coats of Arteries. By George Scott, M.D., Southampton, formerly one of the Physicians to the British Hospital at Renkioi, Dardanelles.

The following causes of arterial obstruction are usually enumerated:—viz., 1. Acute inflammation of the artery or arteritis. 2. Rupture of the internal and middle coats. 3. Degenerations of the arterial coats, such as ossification, calcification, &c., of such a degree as to cause great rigidity of the vascular walls, and great diminution in the calibre of the artery. 4. Coagula or pieces of valves, &c., becoming loosened from the central organ of the circulation—the heart, or from the internal surface of some of the larger arteries, and conveyed to, and impacted in, a more distant arterial canal. 5. Coagula which have formed in the veins, have become loosened thence, been carried into the right cavities of the heart, and ultimately propelled into some of the branches of the pulmonary artery.

As regards the *first* cause usually assigned for arterial obstruction, it seems to me that the experiments made by Professor Virchow in 1847, and detailed in his 'Collected Memoirs,' at p. 395,* satisfactorily prove that simple acute inflammation of the arterial walls is not alone sufficient to cause coagulation of the blood in the vessel at the inflamed part; and the experiments described below also show that inflammation of the arterial walls of such a degree as to cause great thickening of the latter, is unattended with coagulation of blood in, and consequent obstruction of, the vessel.

The proper subject of this paper, however, is with the *second* of the causes above mentioned—viz., with the effects of the rupture of the internal and middle coats of an artery.

As is well known, when a ligature is tied tightly round an artery, the external coat or adventitia, from its superior elasticity, resists the pressure of the ligature, while the middle and internal coats are divided. On this occurring, it has been stated by many writers that the divided coats coil or curl up on themselves to such a degree as partially to occlude the vessel. That, however, this coiling or curling up of the ruptured internal coats does not often occur, any one may be convinced of who chooses to make the following experiment. Tie an artery of a dead human body, or of a dead animal, with a twine ligature sufficiently tightly to rupture the internal and middle coats of the vessel, then open the latter. The rupture of the inner coats will be indicated by a clean line or furrow, but no appearance of coiling or curling up of the divided coats will be seen. On the contrary, the whole extent of the intima and media remains quite adherent to the adventitia, except at the ruptured part.

If, again, the internal and middle coats be ruptured, as they well may be, by pulling strongly at both ends of an artery removed from the body; on cutting open the vessel, the intima and media will be seen to be divided in many places, but there is no curling or coiling up of the same; the edges of the ruptured membranes are seen to be perfectly on a level with the uninjured portions of the intima.

The experiments detailed below show, that when the internal and middle coats of arteries in the living body of animals are divided by a ligature tied round the vessel, there is scarcely ever any coiling up or inversion of the edges of the divided coats, but the results are in a great measure similar to what occurs in the dead body.

I am not aware of the existence of any disease of the arteries which effects such an alteration in the texture of their walls as to cause the internal and middle coats to curl up when divided, any more than they do in a healthy condition.

It is this idea of the curling up or inversion of the inner coats of arteries when divided all round the vessel, which has, I imagine, more than anything else, contributed to give rise to the supposition that arterial obstruction may be caused simply by rupture of the internal and middle coats of the vessel. Many cases of arterial obstruction are recorded in medical literature which have been attributed to rupture of the inner coats of the vessels. Before, however, referring to them, I shall first give an account of some experiments made by myself upon dogs, with the view of ascertaining the effects of rupture of the internal and middle coats of the arteries during the lifetime of the animals.

Last year, in the Pathological Institute of the Charité Hospital, Berlin, I instituted the following experiments:

EXPERIMENT I.

January 12th, 1858.—A large, smooth-haired mongrel dog, which had been some weeks previously much weakened by loss of blood after division of the optic nerve, was operated upon in the following manner. The left femoral artery was exposed to the extent of about three quarters of an inch, proceeding downwards from just below Poupart's ligament. A silk ligature was now passed round the vessel, and tied tightly in a single knot, loosened again immediately and removed, and the wound sewed up. The right femoral artery was afterwards tied in the same way, the ligature removed, and the wound sewed up. On removing the dog from the table, and placing him on his legs, he fell down, not able to stand. The weakness seemed to be chiefly in his hind legs.

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* Gesammelte Abhandlungen zur wissenschaftlichen Medicin, pp. 395—400.

January 13th, noon.—Pulsation can be felt below the points in both femoral arteries, where they had been tied by the ligatures, but the pulse in both is weak and thready.

January 15th.—The dog was killed to-day. First, the jugular vein was opened, and he lost about one pound of blood. Not dying then, he was dispatched by wounding the spinal cord in the neck.

Autopsy.—The wound over the right femoral artery was gaping, the edges being separate to the extent of at least an inch. The artery for about half an inch above and below the point where it had been tied by the ligature, was adherent pretty firmly to the surrounding tissue. On opening it, the intima and media were seen to be divided completely all round the vessel, but no coagulum of blood whatever was found in it, either at the point of division of the internal coats, or in any part from its origin to nearly its termination in the toes. The wound over the left femoral was nearly healed, otherwise the appearances were exactly the same on this side as on the right, the inner coats being seen to be divided all round the artery, and no coagulum whatever being found in the vessel.

EXPERIMENT II.

January 18th, 1858.—A small white mongrel-terrier bitch was operated upon in the following way. The right carotid artery was exposed to the extent of about half an inch, the same tied tightly in a single knot with a silk ligature, the latter loosened and removed immediately, and the wound sewed up.

January 20th.—The left femoral artery of the same dog was exposed to the extent of about three quarters of an inch, tied tightly with a silk ligature in a single knot, the latter loosened again and removed immediately, and the wound sewed up. The pulsations of the artery below the wound could now be distinctly felt on placing the finger over it. The dog was afterwards placed on his legs. Its left hind leg seemed to be somewhat stiff, but otherwise it ran about as usual.

January 22nd.—The right femoral artery was treated in the same way as the left was two days ago. On application of the finger below the point tied, the vessel could be distinctly felt beating immediately after the wound was sewed up.

January 23rd.—Killed the dog to-day, by opening the left carotid artery, and then by wounding the spinal cord in the neck.

Autopsy.—The wound over the right carotid was covered with a large scab, and the edges were found to be separate under this. The artery just in the neighbourhood of the point where it had been tied was not very much more adherent to the surrounding parts than it should have been. There was a reddish line to be seen on the outside around the artery where it had been tied five days before. On opening the vessel no coagula were found in it, but only a very little thin, liquid blood at the point tied; the intima and media were seen to be perfectly divided, and the two ends of the same were about one-eighth of an inch apart, the lower edge of the divided internal coats being turned downwards and protruding into the cavity of the vessel. The artery was traced and opened as far as possible towards the head, but nowhere was a vestige of a clot of any kind to be found. The inside of the artery, where it was deprived of its inner coats, and the edges of the divided coats, were of a reddish colour. The wound over the left femoral was gaping, the edges were hard and thickened, and there was some sanious-looking fluid in the cavity of the same. The artery was adherent pretty firmly to the surrounding parts at the point, and in the neighbourhood of, where it had been tied. A red line was also seen on the outside of this vessel, showing the point where the ligature had been. On opening the artery, the inner coats were seen to be completely divided, but the cut edges were only about half a line apart from each other; there was no inversion of the same, neither was there in any part of the vessel, from the aorta to below the knee, any trace of clot. There was only a little thin fluid blood. The wound over the right femoral artery was closed, but its edges were easily separated by traction. This artery also had a reddish line on the outside, showing where the ligature had been; but the vessel was not at all abnormally adherent to the surrounding parts at the point, and in the neighbourhood of, where it had been tied. On opening the vessel it presented much the same appearance as the left femoral. No coagula of any kind were to be seen in it, so far as I examined.

Experiment III.

February 3rd.—The animal operated on was a small, white, long-haired mongrel-terrier bitch. The left femoral artery was exposed to the extent of about half an inch, and then tied tightly with a silk ligature in a single knot. The ligature was removed almost immediately after its application, and the artery was then felt beating again strongly below the point where it had been tied. The wound was now sewed up, and the dog let go; she seemed to run about as well after this as before the operation.

February 11th.—Tied the right femoral artery with a silk thread a short distance below Poupart's ligament, as in the former case, and loosened the ligature again immediately. Directly the ligature was loosened the artery could be felt beating strongly below the point where it had been tied. The wound was sewed up again immediately after the removal of the ligature, and the dog let loose. She ran about afterwards as well as she had done before the operation.

February 20th, half-past one P.M.—After having etherized the dog, I exposed the right common carotid artery to-day to the extent of about two-thirds of an inch, on a level with the cricoid cartilage, and a little above and below this, tied the artery with a silk thread in a double knot, put a suture in the upper part of the wound to bring the edges partially together, and then let the dog go.

February 22nd.—At half-past one P.M. to-day, just forty-eight hours after having tied the right carotid artery, I put the dog again under the influence of either, opened the wound, and with some difficulty removed the ligature from the carotid; then sewed up the wound entirely.

March 4th.—The dog is very much emaciated since the last operation. The wounds over both femorals are completely healed, and the wound over the right carotid is also healed, but covered with a scab. At half-past two P.M. to-day, she was allowed to inhale chloroform until she died, which happened in about a minute after she began the inhalation.

Autopsy.—The left femoral artery was removed from the dog immediately after death, but was preserved for some hours in alcohol before being opened; when it was cut open, one or two long coagula of a bright red colour were found lying quite loose in the interior of the vessel; these had evidently been formed after death. At the point where the artery had been tied there was a line showing the rupture of the intima and media; the former was everywhere smooth and normal-looking, except that it presented this rupture; the latter was just at a point where the artery gave off a branch. The right femoral artery was opened immediately after the dog died, and the point where the ligature had been applied was indicated by a line showing where the inner coats were divided. All the rest of the intima looked perfectly smooth and healthy; no coagula were found in the vessel; under the microscope, the cleft between the intima and media was seen to be nearly filled up with connective tissue. The right carotid artery was very adherent to the surrounding parts at the point, and a little upwards and downwards from the point, where it had been tied. The knot of the ligature had been removed, but a portion of the thread had remained on the artery at the back part, but merely on the wall of the vessel, and not apparently diminishing the calibre of the same; the coats of the artery were very much thickened at the point tied. On opening the vessel the inner coats were seen to be completely divided, and the ruptured ends about half a line separated from one another. There were merely one or two long bright red coagula, not adherent to the walls of the artery, but lying quite loose in its interior. I conclude, therefore, that these clots must have been formed after death, as the artery was several hours in alcohol before being opened. At all the other parts of the vessel the intima looked perfectly smooth and healthy.

EXPERIMENT IV.

15th February.—A long-haired poodle, about eighteen inches high, was operated upon in the following manner: I exposed, at one P.M. to-day, about half an inch of the left femoral artery just below Poupart's ligament. Then tied the same tightly with a silk thread. I could feel the pulsation distinctly just above the point tied, but below this no pulsation was to be felt after the ligature was applied. I left the wound as it was until five P.M. of the same day, when I removed the ligature and sewed up the wound. It should be mentioned, that after the ligature was applied and the dog let loose, he walked about as well as before the femoral artery was tied.

18th February.—At eight A.M. to-day I exposed the right femoral artery just below Poupart's ligament, to the extent of about half an inch. Tied the artery with a silk thread in a double knot; then left the wound open and let the dog loose. He seemed to walk about as well as before the ligature of the femoral—Five P.M. Cut away the ligature from the artery, and closed the wound with one suture.

26th February.—At two P.M. to-day I exposed the right carotid artery to the extent of about one-third of an inch, and then tied it with a silk ligature in a double knot. Closed the lower part of the wound with two sutures, and then let the dog go.

27th February.—At half-past two P.M. to-day, put the dog under the influence of ether, removed the ligature from the right carotid artery, and sewed up the wound.

5th March.—Killed the dog to-day with chloroform.

Autopsy.—The wounds over both femoral arteries were completely healed; that over the right carotid was still open and suppurating. The left femoral artery, when opened, was seen to be pervious in all its extent; there were only one or two soft coagula lying quite loose in its interior. At the point of the artery which had been tied, the inner coats were seen to be divided all round the vessel. Under the microscope the division of the intima and media was very distinctly seen, but a portion of the cleft was filled up with connective tissue, which had formed, of course, subsequently to the application of the ligature. There was no inversion or coiling up of the inner coats of the artery at the point where these were divided. The right femoral artery was unusually adherent to the neighbouring parts at the point where it had been tied, and a little above and below this. On opening the vessel the cavity was seen to be perfectly free and pervious, and at the point which had been tied the intima and media were seen to be completely divided all round the vessel. There was, however, no inversion of the edges of the ruptured tunics. Under the microscope about the same appearances were seen as in the left femoral artery. The right carotid artery, at the point tied and a little above and below this, was very adherent to the neighbouring parts. On opening the vessel, the intima and media were seen to be completely divided all round the artery, and the divided edges were about a line apart. The cavity of the vessel was perfectly free and pervious, and no coagula were found in it. The intima everywhere else but at the point divided, was perfectly smooth and healthy-looking. Under the microscope the cleft between the divided intima and media was very distinctly seen.

By the preceding experiments we perceive that when the inner coats of an artery of a healthy living animal are ruptured all round the vessel by means of a ligature tied tightly around the latter and then removed, either immediately or at intervals of four, nine, twenty-four, and forty-eight hours after its application, the cut edges of the intima and media do not, as has hitherto been supposed, in all cases curl or coil up into the tube of the artery, but for the most part they remain quite smooth and adherent to the adventitia, except at the ruptured point. In only one case in all the preceding experiments—viz., in the right carotid artery of the second experiment—was the lower cut edge of the inner and middle coats slightly inverted, and projecting into the tube.

And further, we learn that this rupture of the inner coats of an artery is not of itself sufficient to cause coagulation of the blood in, and consequent obstruction of, the vessel at the injured point. It will have been noticed that in three of the arteries examined in the above experiments, some bright red coagula were found lying quite loose in the cavity of the vessels, but these had evidently been formed after death, for in the first two cases the arteries had been preserved in alcohol some hours before being opened, and thus time was given for the coagulation of the liquid blood in them, which was found in nearly all the arteries examined; and of course if these coagula had been formed any time before death, they would have been found more or less adherent to the walls of the vessels.

After having performed the above experiments, I learnt that in the year 1803-4, Dr. J. F. D. Jones had made, in Edinburgh, some very similar ones, but with a different result. In Dr. Jones's book, entitled 'A Treatise on the Process Employed by Nature in Suppressing Hæmorrhage from Divided and Punctured Arteries, &c. &c., London, 1810,' from page 126-36, the experiments are accurately described.

Dr. Jones made nine experiments in all, using four horses and two dogs for the purpose. His fourth experiment was a failure. Only once did he merely apply one ligature. In all the rest of the experiments he employed two, three, or four. He tied them sufficiently tightly to cause rupture of the inner coats of the arteries, and removed them again immediately after their application. He killed all the animals, except the last dog, three days after the operation. The dog in the sixth experiment he killed eight days after the last ligature was applied. In all the experiments he found the arteries completely obstructed at the points which had been tied, except in the third, where only one ligature had been used.

The following is an abridged account of Jones's experiments:

Experiment I.—"Three ligatures were applied to the right carotid artery of a dog close together, so as to cover nearly one-fourth of an inch of the artery, and removed again immediately. The circulation was seen to be perfectly restored in the vessel before the wound was sewed up. The animal died three days after, in consequence of profuse secondary hæmorrhage from one of his femoral arteries, on which an experiment had been performed.

"*Dissection.*—On cutting away the ligatures in the integuments covering the carotid, there was found a quantity of thin pus in the wound extending down to the artery, which, however, was covered and surrounded with a thick layer of lymph, not only at the part on which the ligatures had been applied, but also about an inch below and an inch and a half above. On opening the artery it was found completely obstructed and filled up with lymph, which not only adhered to, but appeared to form one substance with, the parietes of the vessel. The lymph at each of its extremities appeared rather of a dark red colour, in consequence of the adhesion of some red particles."

Experiment II.—"Four ligatures were applied to the carotid artery of a horse, and removed immediately. The circulation was in this case also seen to be perfectly restored before the wound was sewed up.

"The animal was killed three days afterwards.

"*Dissection.*—The ligatures with which the integuments had been sewed up still secured them; but, from the depending situation of the parts, there was a considerable cavity between the integuments and muscles. There was a small quantity of pus over and behind the artery, which was surrounded by a very considerable effusion of lymph. On cutting open the artery at its extremity next the heart, a long coagulum of blood was found filling up its canal, but not adhering to its internal surface; at the extremity of this coagulum a projecting portion of lymph cohered and indented it; on the artery being opened further, the projecting lymph was found to be only the extremity of a portion which completely filled up its canal, for the space of about half an inch, and adhered so intimately to the internal surface of the artery as to form but one substance with it; this lymph had on its surface four transverse lines, which seemed to mark where the ligatures had been applied. The other extremity of the lymph, or that next the head, also projected a little beyond the part at which it adhered to the internal surface of the artery; attached to this lymph was found a very long coagulum of blood, which appeared to fill up the canal of the artery, but did not adhere to its internal surface."

Experiment III.—"A small portion of the carotid artery of a horse was laid bare and tied with a single ligature. The latter was then removed, and the wound sewed up. The animal was killed three days afterwards.

"*Dissection.*—There was a small cavity between the integuments and muscles, owing to the depending position of the parts, and no means having been used to keep them in contact. There was a little pus in the cavity, and a very little behind and about the artery, which was covered with a coagulable lymph. The sides of the artery being minutely examined to account for the formation of this pus, a loose portion of the ligature was found in the pus. The ligature had been made of several threads, and in cutting it away, one of these had been detached and left behind. This led Jones to examine the parts around the artery on which the second experiment had been performed, and about which pus had also been found, and he there discovered a similar loose portion of the ligature. To guard against this accident in future, he determined to use as a ligature a piece of firm small twine. On cutting open the artery, a septum of lymph was found standing across its canal, and firmly adhering to its internal surface. There was not, however, any coagulum of blood on either side of this septum of lymph, and Dr. Jones therefore concluded that it must have had an aperture through which the blood passed; this aperture may, perhaps, have been rendered less distinct by the anterior portion of the septum having been injured in cutting through that part of the artery to which it adhered. The aperture is more distinct in the plate than in the preparation; but the greater thickness of the septum, near the sides of the artery than at the centre of the canal, and the narrowing of the artery at that part, are very correct."*

Experiment V.—"Two ligatures were applied to the carotid artery of a horse about one-eighth of an inch from each other, the same removed and the wound sewed up. The animal was killed three days afterwards.

"*Dissection.*—The canal of the artery was completely obstructed at the part on which the ligatures had been applied. Between the obstructed part of the artery and the heart was a considerable coagulum of blood, and also beyond the obstructed portion. What caused the obstruction was a small piece of lymph of the form of a heart, the base of the latter looking towards the head. Over the artery, and on each side of it, there was a considerable effusion of lymph, extending from rather more than an inch below the obstructed part to nearly an inch and a half above it."

Experiment VI., March 2nd, 1804.—"The right femoral artery of a dog was tied with two ligatures, the latter were removed immediately and the wound sewed up.

"*March 6th.*—The right brachial artery of the same dog was treated in precisely the same way as the preceding artery.

"*March 10th.*—The same experiment was performed on the left brachial artery.

"*March 18th.*—The animal was killed. On examining the arteries on which the experiments had been performed, their canals were found to be completely obstructed with lymph, which adhered so firmly to the internal surface of the artery as to form but one substance with it. On each side of the obstruction there were coagula of blood in the canal of the artery. Over its external surface, and in the parts immediately surrounding it, there was a considerable effusion of lymph.

"From the changes which we know that arteries begin to undergo very soon after a complete obstruction has been made to the passage of the blood through them, there can be little reason to doubt the efficiency of the obstruction produced in the manner described in these experiments, to occasion a complete obliteration of the artery to the first collateral branches above and below the obstructed part."

I must confess that I am at a loss to understand why, in the third experiment of Dr. Jones, where only one ligature was used, there was found on cutting open the artery, a septum of lymph in it, although he concludes that it must have had an aperture through which the blood passed. In my experiments above detailed, there was in no case any such thing seen in the interior of the artery which had been tied by the ligature.

With regard to the other experiments of Dr. Jones, where more than one ligature was employed, and where, consequently, the inner and middle coats were ruptured in more than one place, his results and mine differ still more widely, as will be shown by the following account of my repetition of Dr. Jones' experiments made a short time ago in this town.

EXPERIMENT V.

March 9th, 1859, seven P.M.—A white, rough-haired, mongrel-terrier dog, about sixteen inches high, was operated upon in the following manner:—After the animal was put fully under the influence of ether, the right femoral artery was exposed to the extent of about an inch and a half, beginning just below Poupart's ligament. It was tied at the upper part of the exposed portion with a firm twine ligature in a single knot, and sufficiently tightly to divide the inner and middle coats of the artery. It was also tied in the same manner, about one-eighth of an inch below the first point ligatured. The ligatures were then loosened, the circulation felt to be again fully restored, and the wound sewed up. The left femoral artery was now exposed in the same way as the right, and then tied in three places, each ligature being about one-eighth of an inch apart, or so that the three occupied three-tenths of an inch of the artery. A piece of twine was placed lengthwise on the vessel in this case, and included in the three ligatures, as I thought it would facilitate the removal of the latter. I did not find it do so, however, as they were loosened with some difficulty. The circulation was felt to be restored after the removal of the ligatures, and the wound was then sewed up.

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* See plate in Dr. Jones's Treatise above-mentioned.

March 12th, half-past seven P.M.—Poisoned the dog with hydrocyanic acid. In about ten minutes after death the body was examined.

Autopsy.—The wound in the right thigh was wide and gaping, the animal having apparently bitten away the sutures, and the edges of the same were hard, and elevated considerably above the level of the surface of the limb. The parts around the artery were so thickly matted together that the vessel could only be found by tracing it from above or below the wound. For an inch of its length the artery was very firmly adherent to the surrounding parts. On laying the vessel open from below, a considerable amount of fluid blood and a very little loose coagulum were seen in the cavity, but these were easily washed away with water, and the interior of the artery was seen to be perfectly free. The internal coats were divided all round the vessel in two places where the ligatures had been applied three days previously.

The arterial walls in the neighbourhood of the ruptured parts did not seem to be thickened or altered in texture.

The left femoral artery, where it had been tied, was also so matted to the surrounding parts that the vessel could only be found by tracing it from above or below the wound. It was strongly adherent for a full inch to the neighbouring tissue. On cutting open the artery from below, some red fluid blood and a small quantity of coagula were found in its cavity at the adherent part, but these could be easily washed away by a gentle stream of water, and the interior of the vessel, although certainly much contracted in calibre, was perfectly free. The internal coats were here seen to be divided all round in three places, about one-eighth of an inch from each other. The arterial walls in the neighbourhood of the ruptured portions were considerably thickened over an extent of about three-fourths of an inch.

EXPERIMENT VI.

March 12th, 1859, half-past six P.M.—A small, rough-haired, mongrel-terrier bitch, of a slate-grey colour, was put under the influence of ether, and then operated upon in the following way: the left femoral artery was exposed to the extent of about one inch and a quarter, beginning just below Poupart's ligament, and then four thin twine ligatures tied tightly in a single knot around the vessel at about one-eighth of an inch apart from each other, and removed again immediately. After the circulation was felt to be fully restored to the vessel below the point tied, the wound was sewed up. The right femoral artery was now exposed for about the same distance, and in the same situation as the left. A copper spatula was then passed under it, and several smart blows with a percussion hammer were made upon the vessel as it lay upon the spatula. Of course the artery was raised very considerably from its surrounding connexions by passing the instrument under it. The spatula was then removed and the wound sewed up.

March 22nd, half-past nine A.M.—The dog was poisoned with hydrocyanic acid.

Autopsy.—Both wounds were only partially healed. On cutting down upon the right femoral artery from above, it was found to be adherent to the surrounding parts for some distance. On cutting open the vessel from below, a little blood was found in its cavity, but no coagula of any kind. On washing away the blood with a little water, the internal surface of the intima was seen to be perfectly normal in appearance. Beginning about half an inch below the giving off of the internal iliac, and extending for about three-fourths of an inch down, the walls of the artery looked thickened. This thickened portion of the vascular tunics no doubt corresponded to the part where the vessel had been contused with the percussion-hammer.

On cutting down upon the left femoral artery from above, it was seen to be intimately adherent to the vein and the surrounding parts for the space of about three-fourths of an inch. On cutting open the vessel from below, some blood was found in its interior, but no coagula whatever. On washing away the blood with a little water, the internal surface of the intima was seen to be perfectly normal in appearance, except having four reddish-coloured lines, about one-eighth of an inch apart from each other, corresponding to the ruptures of the inner coats made by the ligatures. The first ruptured point began about three-fourths of an inch below the giving off of the internal iliac artery. There was a small bright red ecchymosed spot between the second and third ruptures of the inner coats, counting from above. The walls of the artery were very much thickened for about three-fourths of an inch of its length at the part where the ligatures had been applied.

In none of the arteries in the last series of experiments, where the inner coats had been ruptured by ligatures, were the cut edges inverted or coiled up into the tube of the vessel—on the contrary, they were perfectly on a level with the uninjured parts of the intima.

When the arteries operated upon were cut open *in situ*, the ruptured portions of the inner coats were separated from each other for a certain distance—perhaps a line—but when the arteries were cut across or removed from the body, and the vessels thus taken off the stretch, the cut edges of the intima and media were in close contact.

EXPERIMENT VII.

March 16th, seven P.M.—A large, black and white, rough-haired mongrel dog, about two feet high, was put under the influence of ether, and operated upon in the following way:—The left femoral artery was exposed to the extent of about 1½ inches, and raised upon the handle of a scalpel. Then with a percussion-hammer it was struck several times pretty smartly, so as to bruise the vessel considerably, between the handle of the scalpel and the hammer. The artery was afterwards replaced in its position, and the wound sewed up. The right femoral artery was now laid bare for at least 1½ inches, and four ligatures of the texture of shoemaker's thread were placed around it. The two upper were tied tightly in a single knot, within one-fourth of an inch of each other; the lowest was tied in the same way about ⅜ths of an inch below the second one. The third ligature was tied round the artery in a double knot, but so that the loop thus formed was larger than the artery, and consequently did not compress the latter at all. The three tightly tied ligatures were afterwards removed, but the third one was left. The wound was now sewed up.

March 19th.—The same dog was again put under the influence of ether, and operated upon in the following manner:—The left common carotid artery was laid bare to the extent of about 1½ inches, raised from its position by a ligature passed round it, and then pinched firmly several times between the blades of a dissecting forceps, so that the walls of the vessel were thoroughly bruised. The artery was then replaced in position, and the wound sewed up.

March 22nd, half-past nine A.M.—The dog was poisoned with hydrocyanic acid.

Autopsy.—Both wounds in the leg were partially open, their edges thickened and callous-looking, emphysematous and infiltrated with a dirty serous-looking fluid. The right femoral artery and vein and crural nerve were very firmly matted together to the extent of about an inch or rather more—so much so, that it was only by cutting that they could be separated from one another and from the surrounding parts. The ligature which had been left on the artery was still there, and around it was a small quantity of dirty pus. On cutting open the artery, a little fluid blood was found in its cavity, but no coagula at all, and on the blood being washed away by a gentle stream of water, the internal coat of the artery was seen to be quite normal in colour and appearance, except at the three points where the ligatures had been tightly tied, where three reddish lines were seen corresponding to the ruptured inner coats. The walls of the artery at the injured part, to the extent of about three-fourths of an inch, were very much thickened, and consequently much more rigid than natural, so that it was with difficulty that the vessel could be held open to enable me to inspect the state of the intima; the walls of the vessel had at this part a great tendency to roll inwards. Owing to the thickening of the arterial tunics, the calibre of the vessel was at this point considerably diminished. The left femoral artery and vein and crural nerve were on a level with the wound, matted together, and strongly adherent to the surrounding parts for the extent of about an inch, but not so firmly as the vessels of the right side. On cutting open the left femoral from below, the cavity was seen to contain some fluid blood, but no coagula of any kind, and on washing away the blood with a gentle stream of water, the internal surface of the intima was seen to be perfectly normal in colour and appearance. For the space of from one-half to three-fourths of an inch, corresponding to where the vessel had been bruised, the walls of the artery were much thickened—at least to twice their ordinary thickness—and so rigid that it was with difficulty on this side also that the divided walls could be held asunder, so as to enable me to inspect the intima. The wound in the neck was not gaping, but on removing the ligatures, the edges separated, and showed a large cavity with walls of a brownish-red colour, and smeared with a dirty pus. On cutting down upon the left carotid artery, it was found to be slightly more adherent than usual to the surrounding parts, to the extent of about three-fourths of an inch. Over the space of about half an inch, where the artery had been pinched between the blades of the forceps, the external surface of the vessel was slightly livid, so as to indicate where the bruising had been. On cutting it open, some fluid blood was found in its interior, but no coagula whatever. On washing away the blood with a little water, over the space of about half an inch, corresponding to the external slightly livid portion, the intima had several bright-red ecchymosed spots distributed over its internal surface, and the inner and middle coats were ruptured irregularly in several places, and not all round the artery. There was, however, no inversion of the edges of the ruptured coats.

Thus we see that when, in the preceding experiments, the inner and middle coats of the arteries operated upon were ruptured close together, and all round the vessel once in two, twice in three, and once in four places; in not one of the cases was there any inversion or coiling up of the ruptured coats into the tube of the vessel, nor any coagulation of the blood in or consequent obstruction of the artery.

Dr. Jones operated upon horses, and also upon dogs, and in both kinds of animals the results of his experiments were, as has been shown, very different from mine. It is difficult to explain this apparent contradiction. All that I can do is to lay a detailed account of my own experiments before the profession, and leave their right appreciation to the judgment of each individual.

The conclusions to be drawn from my experiments are the following:

1. When the internal and middle coats of a healthy artery of a *dead* human subject or of a *dead* animal are ruptured, either regularly all round the vessel, or irregularly at different places, there occurs, in by far the majority of cases, no inversion or coiling up of the cut edges of the tunics, but these remain quite on a level with the other portions of the surface of the intima.
2. Rupture of the inner coats of the arteries of healthy *living* dogs in the same way as described in the preceding paragraph is also, for the most part, unattended with any coiling up of the edges of the divided inner coats.
3. Rupture of the intima and media of the arteries of healthy living dogs—whether in one line all round the vessel, or in several circular lines closely apposed to each other, or irregularly at different parts of the interior of the artery—is not of itself sufficient to cause coagulation of the blood in, and consequent obstruction of, the vessel at the injured part.

4. Inflammation of the arterial walls, of such a degree as to cause great thickening of the same, and consequently a considerable diminution in the calibre of the vessel, is insufficient of itself to cause coagulation of the blood in, and consequent obstruction of, the artery at the point inflamed.

I may also venture to observe that it remains yet to be proved that there exists any disease of the arterial coats producing such an alteration in the texture and physical properties of the inner tunics as to cause the same, when ruptured, to curl up into the tube, and, by mechanically occluding the canal, induce coagulation of the blood at the injured part.

The objection to these conclusions may be made, that there are many well-authenticated cases recorded in medical literature of arterial obstruction caused by rupture of the inner and middle coats of the vessel. Of the fact that obstruction of the arteries did occur, of course there can be no doubt, but the alleged cause of the obstruction must, in my opinion, be called in question. In all such cases, save one, that I have been able to find recorded, the obstruction seems to me quite as easily explained on the embolic theory of coagula sent from a distance and becoming impacted in the arterial canal, as on the hypothesis of rupture of the inner coats of the vessel.

Thus Dr. Oke of Southampton, in the 'Provincial Medical Journal,' vol. iv. p. 51, 1842, relates a very interesting case of arterial obstruction, which he attributes to "spontaneous rupture of the internal coats of the artery, and the projection of its edges into the tube;" and in the third volume of the 'Transactions of the Medico-Chirurgical Society of Edinburgh, 1829,' there is a paper by Mr. Turner on Obstruction of the Canals of the Arteries, describing ten cases, the obstructing cause in all of which is referred to "rupture of the internal coats of the arteries."

For a detailed account of the above cases reference must be made to the original papers, as it would occupy too much space to reproduce them here. Suffice it to say that all the eleven above-mentioned cases, with the exception of the third and fourth, of Mr. Turner, are evidently cases of embolus, where the affected vessels have been suddenly obstructed by coagula or cardiac vegetations conveyed to them from a distance.

In cases of the latter kind it must be observed,

1. That it by no means follows that because there are no physical signs of cardiac disease, therefore the case cannot be one of embolus, for coagula may form, as they pretty frequently do, in the left auricular appendage of the heart, pieces of them may afterwards become loosened thence and conveyed into some distant arterial canal; or again, the blood may coagulate in the pouch of a cardiac aneurism, and portions of the coagulum may become separated and propelled into some of the peripheral arteries without their having been necessarily any symptoms of valvular disease of the heart in the person affected.

2. That more than one artery of the body usually becomes stopped up, and these vessels are generally affected at different times, *e.g.*, the main artery of one arm of a person may all of a sudden become obstructed, and then in a few days afterwards precisely the same symptoms may seize upon one of the legs, as in Mr. Turner's first case.

3. The obstruction caused by an embolus generally occurs at the part of an artery where the calibre of the vessel suddenly diminishes—viz., at the point of division into two or more branches.

4. When a piece of blood-coagulum or any other foreign body becomes impacted all of a sudden in an artery, the irritation of the same sets up inflammation of the walls of the vessel, the latter become thickened, adherent to the surrounding parts, and the intima dies and separates from the media. That the death of the intima is in such cases a consequence of the irritation of the foreign body, is shown by the fact of this tunic being usually destroyed only at the obstructed point, and being in the rest of its extent of quite healthy appearance.

Now, in all the above nine cases, which seem to me quite easily explicable on the embolic theory, the symptoms of obstruction came on suddenly; in most of them there were well-marked pectoral symptoms, either before or after the stoppage of the circulation in the affected arteries, thus showing that there was in all probability cardiac derangement, although perhaps not valvular disease to be detected by the ear during life; in five the obliteration took place in either more than one vessel or in more than one part of the same vessel, and in the two cases where there was a post-mortem examination of the affected vessels, the obstructions were found at points of division of the vessels into smaller branches, and in these two cases, everywhere but at the affected portions of the vessels, the intima was perfectly healthy in appearance, except in Turner's first case, where at one part of the popliteal artery there was a small crucial fissure or laceration of the internal coats, *but where, nevertheless, there was no coagulation of the blood in, or consequent obstruction of, the vessel.*

The third case mentioned by Mr. Turner appears to me to be one of embolus also, although I must confess that it is by no means so evidently so as the nine before mentioned. As the case is somewhat remarkable, I shall detail it here at length. It was under the care of Dr. Abercrombie, of Edinburgh.

> "The patient, a man named John Anderson, aged sixty-three, first complained on 6th November of a fixed pain about the top of the right thigh and groin, and after a day or two the thigh became numbed and weak, gangrene of the whole limb came on, and the man died on the 13th November.
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> "On examination of the body after death, there was found a great quantity of a dark-coloured fluid effused into the limb, and the muscles were universally dark and gangrenous behind the peritoneum; on the right side of the abdomen there was a large gangrenous cavity, containing much dark-coloured fluid and ill-conditioned pus. The femoral artery being laid open, there was found disease of the inner coats in several places. These were soft and separated at various points from the outer coat, so as to lie across the area of the vessel like valves. One of them was about a third down the thigh, and there were others less remarkable. On slitting open the external iliac artery, the inner coats were found soft and thick, and at one place completely lacerated in the whole circumference of the artery, and separated from the external coat for nearly an inch and a half, the portion thus separated having fallen down and coiled up, so as completely to obstruct the canal of the artery nearly at the place where it passes under Poupart's ligament. Between this and the origin of the internal iliac artery there were considerable lacerations of the inner coats. In two places they were slightly detached from the outer coat, and their edges projected into the tube of the vessel. Above the obstruction at the lower part of the iliac, produced by the more extensive laceration, there was a coagulum of blood in the artery. The aorta was in several places diseased, its internal surface ulcerated, and the inner coat partially separated. In some of the smaller branches of the arteries in the pelvis there was ossification, but none in the right iliac artery, where this singular disease was situated. The left iliac artery appeared sound. No other diseased appearances were detected. It is obvious that the obstruction to the circulation was produced by the torn and detached internal coats of the artery."

The description of the post-mortem appearances in this case is certainly very imperfect, so that the following remarks upon them are quite open to correction.

It is said that the inner coats of the femoral artery were in several places soft, and separated at various points from the outer coat, so as to lie across the area of the vessel like valves. There is no mention, however, of any blood-coagula being found in these portions of the affected vessel; now this must surely have been the case if, during life, the internal tunics lay like valves across its cavity; unless coagula had been there some time previously and had softened down, the artery meanwhile becoming obstructed higher up, and so the blood prevented gaining access again to the vessel after the breaking down of the first coagula. Neither is the structure of the blood-coagulum situated above the obstruction at the lower part of the iliac artery described, so as to enable one to form an opinion as to whether its nucleus was constituted by a thrombus or any foreign body conveyed from a distance, or whether the coagulum had formed entirely at the spot itself. Then the aorta was in several places diseased, and its internal surface ulcerated. Now, there is nothing improbable in the supposition that blood-coagula may have formed at the diseased portion of the aorta, pieces of them have subsequently become loosened thence, and been propelled into, and have obstructed, the femoral and iliac arteries in several places and at different periods, the gangrene and separation from the adventitia of the internal coats being a secondary affection caused by the irritation of the emboli. Under such circumstances, the coagula in the most distant parts of the artery from the centre would, of course, have been the oldest, and would perhaps have had time to soften and break down, while those higher up nearer the centre, being of more recent date, would have been found still entire; which indeed were just the appearances described by Dr. Abercrombie.

In Mr. Turner's fourth case, which came under his own observation, the cause of obstruction of the affected vessel is certainly involved in obscurity. The following is an account of the case:

> "A German of middle age, a strong, muscular man, fell down a steep stair when drunk, and sprained his right elbow-joint. On examination, there was found to be a great degree of swelling and tension of the soft parts around. There was no fracture or dislocation to be discovered, but the joint appeared looser than natural, as if some of the ligaments had been torn. Mr. Turner was surprised, on endeavouring to feel his pulse at the wrist, not to be able to discover any pulsation in the radial artery, neither could he detect any in the ulnar nor in any part of the arteries of the fore-arm. The pulsation of the humeral artery was distinct up to the bend of the elbow. Next morning, when sober, he complained of violent pain in the arm. The swelling and tension were rather increased. The absence of pulsation in the arteries continued. He was sent to the Edinburgh Royal Infirmary, and Mr. Turner did not see him for more than a fortnight after this. At that time the swelling and pain of the elbow-joint had almost entirely subsided. On examining the arteries of the fore-arm, no distinct pulsation was to be felt, and pulsation in the humeral artery could be distinctly felt to the fore-part of the elbow-joint, where it suddenly ceased. The fore-arm was pale, and the veins much smaller and more collapsed than those in the other arm, and when they were compressed they filled very slowly. Sensation and motion of the fore-arm and hand were perfect. The patient ultimately recovered perfectly."
>
> "Mr. Turner thinks that cessation of the pulse must be here accounted for in the same way as in the cases already related—that is, by rupture of the internal coats of the artery. The artery could not have been torn across, otherwise there must have been an extensive effusion of blood into the cellular substance of the arm and fore-arm, of which there were no symptoms; nor could the pulse have been stopped by pressure from the effusion into, and consequent tension of, the parts surrounding the artery. Usually no such effect is produced, and the stoppage would not have been permanent, but the pulsation would have returned as the swelling and tension abated, as they began to do very soon after the accident."

Even if rupture of the internal coats had been sufficient to cause obstruction in the artery, it is difficult to conceive how such an injury could have occurred in this case. The humeral artery might have been considerably bruised by the fall the man had, but mere bruising of an artery, unless with a sharp instrument like the teeth of a forceps,* is not sufficient to cause rupture of the internal coats, much less obstruction of the vessel. This is shown by my sixth experiment, where the right femoral artery, being raised on a copper spatulum, was bruised by striking it several smart blows with a percussion hammer without causing any rupture of the inner tunics; and also by my seventh experiment, where the left femoral artery of another dog was similarly treated, and with a like result.

The only explanation I can think of is that, previous to the occurrence of the injury to the elbow, there may have been obstruction of the lower end of the humeral artery, which had until then escaped notice.

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* See Experiment VII.

PART FOURTH.

Chronicle of Medical Science

HALF-YEARLY REPORT ON PHYSIOLOGY.

By Hermann Weber, M.D.

Licentiate of the Royal College of Physicians, Physician to the German Hospital.

I.—Digestive Organs.

1. Bouley and Trousseau: *On the Effects of the Ligature of the Œsophagus in Animals.* (Brown-Séquard's Journal of Physiology, vol. i. p. 777. 1858.)
2. Brown-Séquard: *Remarks on the Phenomena Consecutive to the Ligature of the Œsophagus.* (Brown-Séquard's Journal, vol. i. p. 799, 1858.)
3. Eckhard: *On the Influence of the Irritated Sympathetic Nerve on the Secretion of Saliva.* (See Sub. IV., Nervous System.)
4. Funke: *On the Endosmotic Qualities of the Peptons.* (See Sub. III.)

In July, 1856, Bouley stated before the Academy of Medicine, as the result of many experiments, that ligature of the œsophagus is by no means an indifferent operation, as Orfila had maintained, but that it causes very serious symptoms, and even death, and that the inferences drawn from experiments implying ligature of the œsophagus, can be accepted only with great discrimination. In consequence of this statement, the Academy named a commission, consisting of Bégin, Bouley, Jobart, Larrey, Renault, and Trousseau. The report of this commission, as communicated by Trousseau, contains the following inferences:—1. The application of a ligature round the œsophagus is constantly followed by certain symptoms, which require to be taken into consideration in toxicological researches. 2. The symptoms are more or less serious according to the amount of constriction of the œsophagus. 3. Permanent constriction is fatal in nine-tenths of the cases. 4. Death takes place in the majority of animals between the third and sixth day after the operation. 5. The symptoms characteristic of permanent ligature of the œsophagus are those of extreme prostration. 6. The lesions produced by permanent constriction of the œsophagus, are inflammation of the nerves (accompanying the œsophagus), and of the adjacent parts, either with or without purulent deposits. 7. Temporary ligature of the œsophagus is fatal only in three per cent. 8. As a general rule, the effects are less grave in proportion as the ligature was kept on less long, and the constriction less light. 9. The phenomena produced by ligature of œsophagus can lead to the supposition of poisonous properties in inoffensive substances. 10. The possible effects of ligature of the œsophagus ought always to be taken into account in toxicological experiments requiring the ligature of the œsophagus. Brown Séquard remarks that his experiments prove that mere irritation of the nerves of the œsophagus, without obstruction of the passage, cause the same symptoms as those produced by the ligature, though in a less degree; while the ligature round the œsophagus, after the previous section of the œsophageal nerves, is not followed by the phenomena described by Bouley and Trousseau, but only by those of starvation and of inflammation of the wound. Brown-Séquard ascribes some of the principal symptoms of the ligature to reflex action, and promises to prove this view in a future essay on the nutritive phenomena and reflex secretions.

II. BLOOD; CIRCULATION; RESPIRATION.

1. BROWN-SÉQUARD: *Experimental Researches on the Physiological Properties of the Red and Dark Blood.* (Brown-Séquard's Journal of Physiology, vol. i., p. 729, 1858.)
2. BERNARD: *Two Memoirs of the Variations of Colour of the Venous Blood.* (Brown-Séquard's Journal, vol. i., p. 649, 1858.)
3. DRAPER: *On the Modifications of the Blood-globules in the Spleen.* (New York Journal of Med., Sept. 1858; and Brown-Séquard's Journal, vol. i., p. 825, 1858.)
4. GERHARDT: *Researches on the Dulness of Percussion from the Heart, and the Change of Place of its Limits in Healthy Persons.* (Archiv f. Phys. Heilkunde, Jahrgang, 1858, p. 489.)
5. VIERORDT: *Law of Dependence of the Mean Durations of Circulation in various Species of Animals from the Mean Frequency of Pulsation.* (Archiv f. Phys. Heilkunde, Jahrgang 1858, p, 527.)
6. FUHRER: *On some Outlets (Auswege) of the Circulation of Blood.* (Archiv f. Phys. Heilkunde, Jahrgang 1859, p. 145.)
7. BROWN-SÉQUARD: *Experimental and Clinical Researches on some Points concerning Asphyxia.* (Brown-Séquard's Journal, vol. ii., p. 93, 1859.)
8. VALENTIN: *On Respiration after Suppression of Transpiration.* (See Sub. III.)

On a former occasion* we have mentioned Brown-Séquard's researches on the physiological properties of the red and of the dark blood. The author has arrived at the following further inferences in connexion with the same subject:—10. In the nervous and contractile organs, the persistence of the possibility of recovering the vital properties after they had completely disappeared, is found to show the following increasing series—brain, spinal marrow, urinary bladder, intestinal canal, uterus, heart, iris, sensitive nerves, motor nerves, muscles of animal life. The limits, hitherto found, were for the brain of an adult dog, twenty-two minutes; for the muscles of animal life of a dog, six hours. 11. To the oxygen contained in the blood, the power of regenerating the vital properties of contractile and nervous tissues must be attributed. 12. The contractile tissues of the principal organs of the animal economy, at all events in vertebrata, can be stimulated by blood saturated with carbonic acid. 13. Blood saturated with carbonic acid also acts as an excitant on nervous tissues.

Bernard has continued his researches on the change of colour of venous blood, derived from various glands, according to the physiological condition of the glands.† The experiments which form the basis of the present essay have been performed on the submaxillary gland of dogs. The glandular nerve of this gland appears to come from the fifth pair, but in reality the greater portion of it is formed, according to our author, by the chorda tympani. Galvanization of this nerve, through which the function of the gland, and thus profuse secretion of saliva, is excited, causes the venous blood proceeding from the gland to become of a bright red colour, like the arterial blood; at the same time the quantity of blood flowing from the vein appears much increased, and occasionally the motion of the blood is distinctly jerking. A similar result is obtained when the function of the gland is excited by means of substances placed on the tongue of the animal. During the state of rest, i.e., while the gland does not secrete, the venous blood issuing from it is dark. The opposite influence appears to be exercised by the filaments of the great sympathetic nerve, which accompany the glandular branches derived from the external carotid artery. Dissection of these sympathetic filaments is followed by a change from the dark colour of the venous blood into bright red, while the application of galvanism to the peripheric portion of the filaments renders the colour again dark, and diminishes the velocity of the circulation. Corresponding to these changes in colour and velocity, are changes in the diameter of the vessels. The glandular or tympanico-lingual nerve, as Bernard names it, renders the diameter of the capillary vessels larger; the sympathetic nerve, on the contrary, produces contraction. These two kinds of nerves are considered as being in constant antagonism, their contracting and dilating action is regarded as the cause of all the other phenomena; the physiological action of the nerves producing merely mechanical alteration, which necessarily induces chemical changes. The author dwells especially on the fact illustrated by these discoveries, that each organ or each portion of the body has, to some degree, its own circulation, influenced by modifications in the nervous action, but more or less independent of the general circulation.

In the second memoir, Bernard gives us the result of his chemical examinations, made by means of the oxide of carbon, that the bright red venous blood contains in every 100 volumes 17·26 volumes of oxygen, while the dark venous blood contains only 6·40 volumes, the arterial blood yielding 19·46, i.e. only 2·2 more than the bright red venous blood. Finally, the author points out the apparent difference between the muscular and glandular system, with regard to the colour of venous blood, the latter being darker and more deoxidized in proportion to the increased action of the muscle, more red and less deoxidized in proportion to the increased action of the glands. The author, is, however, inclined to consider our expressions, "functional action," and "state of rest," as not quite correct for the glandular system, the state of rest being, in fact, one of chemical activity, that of functional action one of principally mechanical activity.

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* Conf. this Journal, No. xlii. 1858.

† Ibid.

H. Draper supports Kölliker's view, that the spleen is an organ in which the destruction of blood-globules takes place. He arrived at this inference by microscopic examination of the blood of the splenic vein, compared with that obtained from the limbs. As we have not seen the original communication, we mention only, from the extract in Brown-Séquard's Journal, that the blood of the splenic vein contained among every 100 globules 83 of an altered shape, (retrogressive metamorphosis?); while blood from the limbs contained only 40 among 100.

Gerhardt's researches made at the University Hospital at Tübingen show that—1. In healthy persons the change of position from one side to the other, causes considerable change in the position of the heart's apex; the greatest variation observed was 9 centimetres, or 3·51 inches. 2. During in- and ex-piration the heart moves together with the diaphragm, and also exhibits a movement round its axis when the respiratory movements are of great extent. 3. The change from the erect to the horizontal position produces little or no change in the position of the heart: this fact renders probable the existence of an apparatus which prevents locomotion under these circumstances.

Vierordt publishes a considerable number of experiments performed on various animals, with the view to ascertain the connexion between the mean duration of the circula ion of blood and the frequency of the pulsations of the heart. The result of these experiments appears to corroborate the laws propounded by the same author in his monograph, "On the Phenomena and Laws of the Velocity of the Circulation of the Blood."* Vierordt had then drawn his principal inferences from the phenomena observed on the horse, the dog, the rabbit, and the goat. The experiments of the present essay were performed on one or more cats, rabbits, foxes, hedgehogs, dogs, squirrels, guinea-pigs, hens, geese, ducks, buzzards, ravens, and owls.

It will be remembered, that the first law of circulation propounded by the author in the work alluded to was, "The mean duration of circulation of a species of mammalia is equal to the average time in which the heart completes twenty-six to twenty-eight pulsations." This law is corroborated by his present researches, the average figure for mammalia being 26·3, and, if we except the hedgehog (in which counting the pulsations was extremely difficult) 26·8. It is also shown that birds are subject to a similar law, the average figure for them being 29·0.

Führer's elaborate article "On some Outlets (*Auswege*) of the Circulation of Blood," contains, besides original researches, the corroboration of very important communications made fourteen years ago by the late J. G. Lessing, in an essay hitherto almost unknown, bearing the title "On a Plasmatic Vascular System in all Tissues; but especially in the Bones and Teeth."† We are prevented from entering into the details of these researches, but cannot forego translating the principal conclusions arrived at by Führer.

"The doctrine of the circulation of blood, as described in our handbooks of anatomy and physiology, as a circle complete in itself, in which the blood passes from one system of sanguiferous vessels into the other, requires the following alterations:—

"1. A portion of the capillaries becomes incorporated with the organs, to form an essential element of their tissue." (p. 201.) The author adduces as instances the spleen, in the parenchyma of which new capillaries are constantly formed;‡ the iris and choroid, with their pigment-containing ramifications; the cornea, with its system of transparent tubes anastomosing with each other, which system, being adapted to the function of the cornea, is as essential to its nutrition as it is inseparable from its tissue. The tubuli of the teeth, and the cells (corpuscles) of bones, are adduced as other instances of the entrance of capillaries into the structure of organs and their various formations, according to their general texture and function. In cicatrices and in the productions of chronic inflammatory processes, we find tubular fibres communicating amongst themselves, which take their origin in sanguiferous vessels, and persist, under various appellations, as plasmatic (serous) vessels. In the connective tissue (areolar tissue, *bindegewebe*), and in the parenchyma of organs, a reticular structure is seen, presenting the appearance of nuclei, connective-tissue corpuscles, and fibres. These are plasmatic (serous) vessels, which, under normal circumstances, do not convey blood-globules, and have always been considered as essential constituents of the tissue.

"2. A portion of the capillaries, and of their terminations, does not directly return to the sanguiferous vessels, but forms the lymphatic vessels." (p. 203.) The lymphatic vessels are considered as collateral passages to the veins. The doctrine of a separate system of absorbents, with an origin distinct from that of the veins, would, according to this inference, be incorrect.

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* Conf. this Journal, No. xlv. 1859.

† Mittheilungen aus den Verhandlungen der naturwissenchaftlichen Gesellschaft in Hamburg, vom Jahre 1845. Herold'sche Buchh. 1846.

‡ Conf. this Journal, No. xxvii. 1854.

"3. Arteries, veins, and lymphatics possess a common capillary system, in which the terminations and organs of the various classes of vessels are connected among themselves by anastomosis. There is no real limit between lymphatic and blood-vessels." (p. 203.) The vessels called serous (plasmatic) vessels, are of intermediate nature, which, under strong pressure, admit blood, and thus become blood-capillaries, and in their further passage sometimes join veins, sometimes lymphatics. The preparations, represented as successful injections, show not only the injected capillaries of sanguiferous, but also the injected serous vessels. Führer's second proposition, concerning the lymphatics, appears to have been adopted by the old physiologists in the seventeenth and eighteenth centuries.

Brown-Séquard's experiments, performed on various kinds of animals, with regard to their power of resisting asphyxia, prove:—1. That in new-born animals the temperature of their body, at the time of the experiment, is of great influence on the time during which they remain alive under conditions causing asphyxia; which conditions, in the author's experiments, consisted in submersion under water of 25° Cent. (77° Fah.) Thus, of four young dogs, one day and a half old, being of the same litter,

One having	the temperature of	37½°	Cent. survived	19½	minutes.
"	"	33°	"	28	"
"	"	26½°	"	36½	"
"	"	19°	"	50½	"

2. Also, in adult warm-blooded animals, their temperature is of very great influence on the space of time during which they survive submersion. It appears, therefore, to be a general law, that the more the temperature of animals is lowered, the longer they are able to resist asphyxia. 3. The influence of the species of the animal on the time during which the asphyxiating agent can be endured, is much greater in young than in adult animals. On the whole, mammals, young and adult, survive longer than birds. 4. With regard to the influence of age, the author infers from his experiments, that it is less general than Legallois and Edwards had assumed it to be. In cats and dogs, it is true that the new-born animals survive three times as long as the adults; but the adult guinea-pig frequently survives longer than the new-born; the adult pigeon, too, survives in general as long as the new-born.

III. ABSORPTION; SECRETION; METAMORPHOSIS OF MATTER.

1. KÖHLER: *On the Doctrine of Absorption.* (Virchow's Archiv, vol. xiv., p. 401, 1859.)
2. FUNCKE: *On the Endosmotic Qualities of the Peptons.* (Virchow's Archiv, vol. xiii., p. 447, 1858.)
3. VALENTIN: *On the Respiration after the Suppression of the Transpiration, and on the Reviving Influence of Higher Degrees of Warmth.* (Archiv f. Phys. Heilkunde, Jahrgang 1858, p. 433.)
4. BROWN-SÉQUARD: *Note on the Existence of Rhythmic Contractions in the Excretory Ducts of the Principal Glands in Birds.* (Brown-Séquard's Journal, vol. i., p. 775, 1858.)
5. HOPPE: *On the Circum-polarizing Quality of the Constituents of the Bile.* (Virchow's Archiv, vol. xv., p. 126.)
6. KUHNE: *Contributions to the Doctrine of Icterus. First Communication.* (Virchow's Archiv, vol. xiv., p. 310, 1858.)
7. BERNARD: *On a new Function of the Placenta.* (See Sub. VII.)

Köhler's communication on the absorption of poisons is the result of numerous experiments performed at the Physiological Institution of Marburg, under the superintendence of Nasse. The author endeavours to show the difference in the rapidity of absorption between starving and fed animals. The animals experimented upon were rabbits, dogs, and pigeons; the substances used, strychnia, hydrocyanic acid, and ether; the channels of introduction, the digestive tube, the peritoneal cavity, the respiratory organs, and the subcutaneous cellular tissue of the back. The inference arrived at is, *that starving diminishes absorption and retards the symptoms of poisoning and death.* This result is contradictory to the views of many physiologists, but is analogous to that obtained by Kaupp in his experiments on the action of loss of blood on the phenomena of poisoning by strychnia. Köhler's experiments exhibit, it must be stated, frequent exceptions to the law he endeavours to establish, but in part these exceptions appear to depend on concomitant circumstances.

Funke proves by experiments on rabbits, that the absorption of the albumin-peptons in the digestive canal takes place with much greater rapidity than that of albumen. The author further shows that the quantity of albumin-pepton passing within a certain space of time and under otherwise equal circumstances, through a filter of animal membrane, is considerably larger than that of albumen passing through it. Funke's experiments regarding the endosmotic properties of the substances in question finally demonstrate that the faculty of diffusion of the peptons is much greater than that of albumen. The endosmotic equivalent of the peptons is calculated at eight, that of albumen at 120. The addition of dilute hydrochloric acid diminishes the rapidity of diffusion of the peptons, and raises the endosmotic equivalent considerably. Alkalies in larger proportion increase the rapidity of diffusion and lower the endosmotic equivalent; in smaller proportion they have the opposite effect. These facts are of practical value, if we take into consideration the acidity of the fluids in the stomach, and their alkaline reaction in the small intestines.

Valentin caused suppression of the function of the skin in rabbits by covering the greater extent or the entire surface of their skin by glue or albumen. The author's experiments especially show the change thus produced in the phenomena of respiration and calorification, as also the beneficial influence exercised by a high degree of temperature in cases of suppressed function of the skin. If a rabbit, covered with a solution of albumen or glue, is kept in a temperature below 20° Cent. (68° Fah.), it appears at first not to suffer, but after from two to six hours it loses the power of standing; the respiratory movements become rare and almost imperceptible; peripheric irritation causes scarcely any reaction; the temperature of the rectum sinks from 39° Cent. (102·2° Fah.) to 20° Cent. (68° Fah.), and lower, the sinking being more or less rapid in the ratio of the surrounding temperature; the animal takes no food, and shortly dies. The diminution of the respiratory movements is accompanied by a very marked diminution in the quantity of carbonic acid exhaled; a stay of four hours in a temperature of 18° to 19° Cent. (64·4° to 66·2° Fah.), caused a reduction in the exhalation of carbonic acid to ¼th of the normal amount, a stay of thirteen hours in a temperature of from 14° to 19° Cent. (57·2° to 66·2° Fah.) to $\frac{1}{10}$th of the normal amount, while the number of respirations had decreased to ⅜ths of the number before the application of the impermeable coat. The quantity of oxygen absorbed decreases likewise considerably, but not to the same degree as the exhalation of carbonic acid. Thus in one instance the proportion of oxygen absorbed to carbonic acid exhaled was (by weight) 0·72 to 1 before the experiment, while after the coating with glue and the stay in a temperature of 18° Cent. (64·4° Fah.), the proportion was 1·69 to 1. The urine contained in most cases albumen; the proportion of urea did not seem much altered. The post-mortem examination did not exhibit any striking changes; the serous effusions into the various cavities were by no means the rule (Fourcault); the glycogenic substance was found in the liver of only one animal. Remarkable is the change of phenomena under the influence of increased temperature. Animals coated with glue or albumen, which had offered almost the appearance of death, regain a great degree of liveliness, become enabled to stand on their legs, and often take again food; their respiratory movements increase in frequency and extent, their warmth rises considerably, the quantity of carbonic acid exhaled is found to be much larger, and the proportion between absorption of oxygen and exhalation of carbonic acid in general almost normal. Hitherto, however, Valentin has not succeeded in keeping any of the animals coated with glue or other impermeable substances alive. Yet it cannot be denied that the author's experience regarding the effect of artificial warmth may be beneficially employed in the treatment of extensive burns, and also other complaints connected with suppressed action of the skin.

Brown-Séquard confirms Bernard's observation,* that the ductus choledochus and the ductus pancreaticus of birds contract in a rhythmic manner. The author has found the same phenomenon to take place in the ureters of birds, and especially during spring, on the vas deferens of adult birds † The contraction commences at the origin of the ducts, and extends gradually towards their termination. The rhythm of the movements is not the same in the four different kinds of excreting ducts. The celerity of the rhythmic movements of these ducts, just as that of the heart's contractions, is frequently increased by asphyxiating the animal. The destruction of the spinal centre is not followed by immediate cessation of these rhythmic movements. In mammalia Brown-Séquard has never found the rhythmic contractions of the excretory ducts, with the exception of the ureters.

Hoppe found that cholesterin and the various acids of the bile possess the faculty of circular polarization. The following are the figures for the rotation of the different substances, as observed by the author:

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* Comptes Rendus de la Soc. de Biol., vol. i., p. 171. 1849.

† Conf. Sub. IV. of this Report.

	Red light.	Yellow light.
Cholesterin	— 27·5°	— 34·0°
Choloidinic acid	+ 31·3°	+ 33·3°
Cholalic acid	+ 24·6°	+ 27·7°
Taurocholic acid	+ 24·9°	+ 25·3°
Glycocholic acid	+ 27·2°	+ 29·3°

Kühne's contribution to the pathology of icterus contains many experiments of physiological interest. By means of Hoppe's method, for the description of which we refer to the original, Kühne proved, first, that icterus, caused by closure of the common duct, is always attended by the excretion of biliary acids through the urine, besides the colouring matter of the bile; while normal urine does not contain biliary acids. He further found that the urine of icteric patients affected with closure of the ductus choledochus, does not exhibit any traces of hippuric acid; as also that in each case the ingestion of benzoic acid does not lead to excretion of hippuric acid, as it does in health, but that the benzoic acid is excreted as such. This fact, first observed in man, is corroborated by experiments on dogs. In a former essay* the author has stated his view, that the transformation of benzoic acid into hippuric acid takes place by means of the glycocholic acid; the absence of this transformation in the cases in question lead him to the inference that the formation of glycocholic acid ceases after the closure of the common ducts—an inference which is supported by direct chemical analysis. Numerous injections of the salts of soda with the biliary acids into the veins of dogs had the constant effect of causing the passage of these salts, together with bile pigment, through the urine. In most cases the urine exhibited also traces of albumen, and in many hæmatin, without blood-globules. Some animals had, after the injections, convulsions of epileptiform nature. The regular appearance of the biliary pigment in the urine is ascribed by the author to the decomposition of blood-globules and the transformation of hæmatin into biliary pigment, through the influence of the biliary acids. This view, it will be remembered, is contradictory to that of Frerichs, who inferred from similar experiments that the biliary acids were sometimes transformed within the organism into biliary pigment. Kühne shows, at all events, that the biliary acids possess the power of decomposing the blood-globules, and his further experiments render also his theory regarding the origin of the biliary pigment very probable. It is evident that this view, if correct, would support the theory that the liver is an organ in which the blood-globules are decomposed. In the report of the cases of artificial icterus, by ligature of the common duct, two phenomena are mentioned, which appear as yet not sufficiently explained—viz., that the skin of the dogs had no icteric appearance, although the urine was loaded with biliary pigment, and that in one of the dogs the urine became after some time deprived of biliary pigment, although the fæces remained uncoloured; that therefore the liver seemed to have lost its function of forming biliary pigment.

IV. NERVOUS SYSTEM.

1. WAGNER: *Critical and Experimental Researches on the Functions of the Brain.* (Zeitschrift f. rat. Med.; Third Series; vol. v., p. 215, 1859.)
2. NONAT: *Facts tending to show that the Anterior Fasciculi of the Spinal Marrow are subservient to the Transmission of Sensitive Impressions.* (Brown-Séquard's Journal, vol. i., p. 752, 1858.)
3. BROWN-SÉQUARD: *New Experiments on the Transmission of Sensitive Impressions in the Spinal Marrow.* (Brown-Séquard's Journal, vol. ii., p. 65, 1859.)
4. VON BEZOLD: *On the Crossed Action of the Spinal Marrow.* (Zeitschrift f. wissensch. Zoolog., vol. ix., p 307; and Schmidt's Jahrbücher, vol. i., p. 28, 1859.)
5. LISTER: *Preliminary Account of an Inquiry into the Functions of the Visceral Nerves, with Special Reference to the so-called "Inhibitory System."* (Proc. of the Roy. Soc. of London, vol. ix, p. 32.)
6. PFLÜGER: *Experimental Contribution to the Theory of "Inhibitory Nerves."* (Archiv f. Anatomie and Physiologie;† Jahrgang 1859, p. 13.)
7. HABER: *On the Action of Curare on the Cerebro-spinal Nervous System.* (Archiv f. Anat. und Physiol., Jahrgang 1859, p. 98.)
8. BUDGE: *On the Centrum Genito-spinale of the Sympathetic Nerve.* (Virchow's Archiv, p. 115, 1859.)

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* Conf. this Journal, No. xliii. 1858.

† This journal, which is the continuation of Reil's, Reil's and Autenrieth's, J. F. Meckel's and Johannes Müller's Archiv, since J. Müller's death, is edited by the well-known C. B. Reichert and E. du Bois-Reymond.

9. Wagner: *Notes on some Experiments on the Cervical Portion of the Sympathetic Nerve in a Decapitated Woman.* (Zeits. f. rat. Med.; Third Series; vol. v., p. 381, 1859.)
10. Eckhard: *Preliminary Account on the influence of the Irritation of the Sympathetic Nerve on the Secretion of Saliva.* (Zeitsch. f. rat. Med.; Third Series; vol. v., p. 384, 1859.)
11. Pflüger: *On a New Re-agent exhibiting the Axis-cylinder.* (Arch. f. Anat. und Physiol., Jahrg. 1859, p. 132.)

Wagner urges the necessity of the combination of the clinical observation on man with experimental researches on animals, in order to arrive at a more exact knowledge of the functions of the various portions of brain. He especially requests pathologists to give more accurate descriptions of symptoms during life and appearances after death, pointing to the necessity of our universally using the same terms for the signification of the various portions of the brain. For experiments, the author recommends pigeons, and insists on the importance of ascertaining the weight of the whole brain and of the single portions; he proposes the following method;

"After the animal has been killed by suffocation, and the whole body has been weighed, the brain, with the origin of the olfactory nerves, and without the hypophysis (pituitary gland), is removed from the cranium by cutting through the chiasma of the optic nerves, and through the medulla two millimètres behind the calamus scriptorius. The brain thus taken out is then weighed, after which the following parts are removed and weighed separately:—1. The hemispheres of the cerebrum immediately before the thalami optici and immediately behind the commissura anterior, which remains attached to the hemispheres. 2. The mesencephalon (corpora quadrigemina, thalami optici, with the optic nerves cut off close to the origin of the oculo-motor nerves from the pons). 3. The cerebellum from the valvula cerebelli anterior (valve of Vieussens), and cut off at the base of the inferior peduncles (crura cerebelli ad medullam). 4. The remaining piece of the medulla oblongata, the trunks of the cerebrum (*Grosshirnstämme*), and the origins of the cerebral nerves. Finally, the corpora quadrigemina are removed in the sulcus surrounding them from the mesencephalon, and again weighed separately." (p. 228.)

Nonat read in 1837, before the Academy of Medicine, a memoir, in which he asserts, as the result of repeated experiments, that the anterior and posterior fasciculi of the spinal marrow do not exclusively serve either motion or sensation, but that both convey sensation as well as motion. Nonat's experiments, at all events, proved that section of the posterior fasciculi does not cause abolition of sensation in the parts below the section. The author maintains at present the same view which he expressed in 1837.

Brown-Séquard's latest series of experiments lead to the corroboration of two important inferences communicated by him in former essays,* but repeatedly contested since then by other physiologists (Chauveau, Schiff, &c).† In a vigorous rabbit the author cuts first the roots of all the anterior nerves of the lower extremities and of the other lumbar pairs, and having convinced himself of a high degree of sensibility in the lower extremities, he cuts through the right lateral half of the spinal marrow at the first lumbar vertebra; the result is "that the *right* posterior limb is even more sensitive than before, while the *left* posterior limb has lost its sensibility." Other experiments are—Section of both posterior fasciculi of the spinal marrow: result—the tactile sensibility and the other kinds of sensibility persist below the section: Section of the entire thickness of the spinal marrow in the dorsal region, with exception of the posterior fasciculi; result—the posterior limbs have lost the tactile sensibility, and all other kinds of sensibility. Brown-Séquard infers—1. That the conductors of the sensitive impressions of the limbs and trunk cross in the spinal marrow, and not in the brain, as was supposed; 2. That the posterior fasciculi of the spinal marrow do not seem to be subservient to the transmission of any kind of sensitive impressions to the brain, but that the central grey substance is subservient to the transmission of these impressions.

Bezold thinks with Ludwig that the principal cause of the great discrepancy in the results obtained by various physiologists regarding the functions of the spinal marrow, lies in the fact that different men have experimented on different classes of animals, and each of them has generalized his experience. The author has therefore instituted separate series of experiments on amphibia, birds, and mammalia. I. In *frogs* the results arrived at are:—1. Sections of either half of the spinal marrow from its commencement until immediately above the origin of the nerves of the lower extremities, exercise no influence on the motions of the limbs of the other side, nor do they produce any change in the reactions of the opposite side on peripheric irritations. 2. Sections of one half in a greater distance than one line from the origin of the nerves for the limbs below the section, do not exercise a marked influence on the movements of these limbs, nor on their reaction to irritation. 3. Section of either half immediately above the issue of the nerves for the limbs, paralyzes the motion of the corresponding limbs of the same side, but not the reaction to irritation. 4. Longitudinal sections through the median line of the spinal marrow do not impair the motion, but increase the reaction, produced by irritation of parts corresponding to the portion of the spinal marrow thus divided. The author infers from these results, "*that a crossed action of the spinal marrow does not exist in frogs.*" II. In *pigeons*, section of one lateral half of the spinal marrow, at whatever portion it may be instituted, in no way impairs the voluntary motion of the opposite side of the body, nor the reaction on peripheric irritation; it destroys the voluntary motion of the corresponding side below the place of section, but not the reaction of the same side on irritation, the reaction appears, on the contrary, rather increased. The inference therefore is again, that there is *no reason for admitting the view of crossed actions in the spinal marrow of pigeons.* III. The experiments on *mammalia* (rabbits, guinea-pigs, cats, dogs) showed, that section of either lateral half causes paralysis of motion on the corresponding side, below the section, and likewise on the same side increased reaction to irritation; while on the other side of the body neither the voluntary motion nor the reaction to irritation are perceptibly changed. Bezold concludes from this that there *does not exist a crossing of the motor conductors in the spinal marrow of mammalia*, and that the *crossed transmission of sensitive impressions is not proved.*

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* Conf. this Journal, No. xxxv. 1856. † Ibid., No. xli., (1858); and No, xliii. 1858.

The same author also examined the influence of section of either lateral half of the spinal marrow on the temperature, and found:—1. Constant increase of temperature in the lower part of the leg (or fore-arm), and in the foot of the corresponding side. 2. Constant and considerable decrease of temperature in the lower part of the leg (or fore-arm), and in the foot of the opposite side. These changes began about half an hour after the operation, and increased steadily for some hours to such a degree as sometimes to cause a difference of 15° Cent. (57° Fah.). 3. Decrease of the temperature in general. 4. Constant decrease of temperature (absolute and relative) of the upper part of the arm, of the thigh, and upper part of the leg of the corresponding side. 5. Less considerable diminution in the same parts of the opposite side. 6. Equal diminution on both sides of the thorax, as far as it is covered with the respiratory muscles. Bezold concludes that the vaso-motor nerves of the lower part of the leg and of the foot ascend in the spinal marrow on the same side up to the medulla oblongata; that a crossing of the vaso-motor nerves for the thigh and upper part of the leg is not proved by the experiments. Finally, the author expresses in the *résumé* that in *none of the classes of animals examined a crossing of the sensitive fibres is proved*, nor the converse.

Lister has examined the facts which have induced Ed. Weber, Pflüger, Ludwig, and others to establish the theory of inhibitory nerves. The author's experiments on rabbits corroborate that galvanic irritation of the pneumogastric and splanchnic nerves does produce retardation and quiescence of the movements of the heart and intestines; he thinks himself, however, entitled to infer, from the results of many experiments, that only stronger currents have such an effect, while the mildest currents, "just perceptible to the tip of the tongue," cause, on the contrary, accelerated contractions of the heart and increased peristaltic movements. With regard to the splanchnic nerve and intestines, Lister sums up as follows: "It appears that the intestines possess an intrinsic ganglionic apparatus which is in all cases essential to the peristaltic movements, and while capable of independent action, is liable to be stimulated or checked by other parts of the nervous system; the inhibiting influence being apparently due to the energetic operation of the same nerve-fibres which, when working more mildly, produce increase of function." (l. c. p. 372.) Concerning the connexion between the pneumogastric nerves and the heart, the author's view is, likewise, that these nerves have no claim to be called "inhibitory nerves," an expression which he considers "altogether objectionable, since there is good reason to think that the same fibres which check the movements, much more commonly enhance them." Lister differs, therefore, in his view from Schiff,* who is otherwise a no less energetic opponent to the theory of an "inhibitory system," and who considers the inhibiting influence as depending on nervous exhaustion. The very rapid recovery of the intestinal and cardiac actions, when the inhibiting galvanic currents are discontinued, appear to our author incompatible with the explanation by "exhaustion."

We cannot conclude our report on Lister's paper without mentioning that, in repeated experiments, he did not observe increased frequency of the heart's contractions after section of the pneumogastric nerves; a fact which is at variance with the results of other experimenters, of whom we need only mention Nasse† and Arnsperger,‡ who found acceleration of the heart's contractions after section of the vagi.

Pflüger defends the theory of the inhibitory action of the vagi and splanchnici against Schiff's just-mentioned view, by asserting that even the mildest currents cause retardation or quiescence of the respective organs, and by calling to mind that section of the vagi does not produce diminution of the heart's contractions, but, on the contrary, increased frequency. The author further describes Schiff's experiment on the thigh, on which this physiologist principally bases his view, and attempts to show that Schiff has misinterpreted the phenomena; our space, however, compels us to refer for the description to Pflüger's paper, and to Schiff's "Lehrbuch der Physiologie."

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* Schiff's Lehrbuch der Physiologie. Jahr. 1858.

† Conf. this Journal, No. xxxv. 1856.

‡ Ibid., No. xxxvii. 1857.

Haber's experiments with curare performed on frogs, under Reichert's superintendence, corroborate Bernard's and Kölliker's inference, that curare paralyzes the terminations of motor nerves; that the contractions of the muscles, caused by local irritation, after the poisoning by curare, depend on the irritability of the muscular fibres themselves, without the influence of the nerves (see Sub. VI.). The peripheric terminations of the motor nerves first become paralyzed, in general within twelve to fifteen minutes; after five to eight hours the spinal marrow appears paralyzed, while the sensitive nerves in their whole extent, and the trunks of the motor nerves (in opposition to their peripheric termination), retain their irritability for a long period (twenty, thirty-six, or forty-eight hours). The author attributes this difference in the several portions of the nervous system, not to differences in the structure and supply of blood, but to differences in the chemical constitution.

It is well-known that Budge attributes to the part of the spinal marrow situated between the sixth cervical and the third dorsal vertebræ the function of a centre for the cervical portion of the sympathetic nerve (*centrum cilio-spinale*), presiding over the influence of the sympathetic nerve on the movements of the iris and the bloodvessels of the head (regulation of temperature). The same author establishes a centre for the lumbar portion of the sympathetic nerve (the *centrum genito-spinale*) situated in rabbits in that part of the spinal marrow which corresponds to the fourth lumbar vertebra. Budge says that the two filaments of the lumbar portion of the sympathetic nerve are found between the two psoas muscles, behind the aorta and vena cava. Communicating branches are seen passing from one filament to the other, and in the region of the fifth lumbar vertebra a ganglion is constantly met with, which receives communicating branches from the third and fourth lumbar nerves. Galvanic irritation of this ganglion, and of the filaments below it, causes energetic movements of the ductus deferentes, while irritation above the ganglion does not influence them; it also causes increased movement of the urinary bladder, and of the lower part of the rectum. The portion of the spinal marrow which, when irritated, excites the movements of the ductus deferentes, is limited to a length of about three lines; while the centre for the urinary bladder and rectum appears to be rather less limited. If the sympathetic nerve of one side is cut, irritation of the genito-spinal centre causes energetic movements in the deferent duct of the side on which the nerve is entire; while the movements of the duct of the other side are slight, but not quite absent, on account of the communicating branches between the two nerves. Budge remarks, that these facts, as well as those connected with the cilio-spinal centre, speak against the independence of the sympathetic nervous system.

Wagner had the opportunity of examining the action of the cervical portion of the sympathetic nerve on the eye in the body of a woman who had been beheaded about twenty minutes before the commencement of the experiments. Irritation of the cervical portion of the right sympathetic nerve about 1¼″ below the superior cervical ganglion, by means of a magneto-electric apparatus, caused gradual opening of the lids of the right eye, previously closed; a result which was still obtained forty-five minutes after the decapitation, although then in a less perfect degree. Simultaneous with the opening of the eyelids was the dilatation of the pupil in a very marked manner; an effect which was still obtained, as late as forty-eight minutes after decapitation, when the head was handed over to other experimenters. Wagner adds, that the opening of the eyelids did not depend on the action of the levator palpebræ superioris, as it had quite the character of the action of organic muscular fibres—i.e., the muscular action followed the commencement of the irritation only after some minutes, and continued for some time after the cessation of the irritation.

Eckhard found that the saliva secreted under the influence of irritation of the sympathetic nerve, is different from the fluid obtained through irritation of the glandular branch of the fifth pair. The former is less clear and transparent, and, above all, more thick and viscid than the latter. A more accurate description the author promises on a future occasion.

Pflüger uses the following plan for showing the axis-cylinder. He removes the neurilemma by means of a pin, and spreads out the fibres on the glass slide, without at first adding any fluid. Afterwards he adds a drop of collodion, and covers the whole with a thin glass. Everywhere, the author says, the most distinct axis-cylinders immediately appear.

V. Senses.

Klaatsch and Stich: *On the Seat of Gustative Sensation.* (Virchow's Archiv, vol. xiv., p. 225, 1858.)

The greater portion of Klaatsch's and Stich's paper is historical. Their own experiments were made with extract of quassia, solutions of sugar, salt, and tartaric acid. These substances were applied by means of a very thin camel's hair brush, while the eyes of the person operated upon were tied. The authors infer: "A narrow border round the margin of the tongue, varying from two to four lines in breadth, is subservient to taste: in most persons it lies in the middle of the margin; in some it approaches the upper surface, in very few, it appears, the lower surface. The root and the posterior third of the tongue, as well as a part of the soft palate, are also subservient to taste. Other parts of the mouth or fauces, or of other organs, are not instrumental in this sense." (l. c., pp. 237, 8.)

VI. Organs of Locomotion.

1. **Haber**: *On the Action of Curare on the Cerebro-Spinal Nervous System.* (Achiv f. Anat. und Physiol., Jahrgang 1859, p. 98.)
2. **Von Wittich**: *On Peculiar Muscular Contraction caused by the flowing of Water through the Vascular System.* (Virchow's Archiv, vol. xiii., p. 421, 1858.)

Haber bases the view that the contractions of the muscular fibres observed after poisoning by curare (see Sub. V.) are independent of the motor nerves, especially on the character of the muscular contractions, and on the manner of termination of the motor nerves in the tissue of the muscles, as elucidated by Reichert.* This author found that the single fibres of the nerves are spread over a comparatively large space, thus comprising several muscular fibrillæ. "The whole peripheric distribution of the motor nerve-fibres," he reasons, "the frequent ramifications of the fibres entering the muscle, manifest the principle to bring each nerve-fibre in contact with as many muscular fibres as possible." From this distribution Reichert inferred, that the irritation, originated by a single nerve-fibre, is not restricted to a limited number of muscular fibres, but acts on the whole muscle, or at least a great portion of it. Experiments show that the local irritation of a muscle of an animal recently killed, causes contraction of a considerable number of muscular fibres; while, on the contrary, each local irritation in animals poisoned by curare, causes only a very restricted contraction of fibres, in fact, only the fibres actually irritated, the other part of the muscle remaining perfectly motionless. It appears from this that the *independent irritability of the muscular fibres is at last firmly established.*

Wittich studied the nature of the convulsions produced, as E. Weber and Liebig already had observed, by injection of water through the aorta of animals soon after their death. By causing a stream of water to pass through the aorta of frogs, convulsions are occasioned, first in the muscles of the head and neck, then in those of the arms and abdomen, and at last when the blood is altogether expelled through the veins, also in the posterior limbs. Distilled water caused, of all fluids tried, the most powerful and long-continued convulsions; spring and river-water less so; serum and saline solutions had scarcely any effect. Destruction of the nervous centres and section of the nerves had no influence on the appearance of the convulsions. Frogs poisoned by brucine and curare exhibited, when distilled water was injected, powerful convulsions, while galvanic irritation of the nerves had ceased to excite the muscles. Wittich further convinced himself that distilled water alone, when applied on a frog's muscle deprived of skin, causes contractions. The author infers that pure water alone, without the influence of the nerves, causes the phenomena in question, and that water is a powerful excitant for the muscles, while it appears to be an indifferent agent with regard to the nerves. The endosmotic interchanges between the tissue of the muscle and the distilled water offer the probable explanation.

Sexual Organs; Generation.

Bernard: *On a New Function of the Placenta.* (Brown-Séquard's Journal, vol. ii., p. 80, 1859.)

Bernard arrived at the following inferences:—1. There exists in the placenta of mammalia a function, hitherto unknown, which coincides with the absence of the glycogenic function of the liver during the first period of the embryonic life. This function is localized in a transitory glandular or ephithelial element of the placenta, which in some cases of animals is mixed with the vascular portion of that organ, but which in the ruminantia presents itself in a separated shape, so as to form on the amnios spots of epithelial appearance, the glycogenic nature of which has hitherto been unknown. 2. This temporary hepatic organ of the placenta, which permits the study of the formation of the glycogenic substance in an isolated anatomic element, confirms the author's view that the formation of amylaceous matter is a faculty common to the animal and vegetable kingdom. 3. The glycogenic function begins in animals with the commencement of their fœtal existence, and before the organ, in which this function is localized in the adult, is developed. The author further proposes the following question—whether the same glandular cells are charged with the double function of producing the glycogenic substance and of forming bile, or whether the liver is to be considered as a compound organ, in which are found mixed two isolated anatomic elements—the one destined to form glycogenic substance, the other to form bile. Bernard thinks that this question may now be decided by researches on the embryonic development and on lower animals. He has himself commenced such researches, and promises to communicate the result.

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* C. B. Reichert: Ueber das Verhalten der Nervenfasern bei dem Verlauf, der Vertheilung und Endigung in einem Hautmuskel des Frosches. (Archiv f. Anat. und Phys., Jahrgang 1851, p. 68.)

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

Licentiate of the Royal College of Physicians, and Physician to the Northern Dispensary.

I. *On the employment of Tannin in large doses in Albuminous Anasarca.* By Dr. P. Garnier. (Archieves Générales de Médicine, January, 1859.

Although the internal use of tannic acid is still very limited in France, its employment in large doses has been much recommended lately in other countries, and has been extended to numerous cases which, while proving its innoxious character, appear to exhibit it as possessing some totally new properties. It has been shown to be useful in all cases where it is required to arrest hæmorrhages, to give tone to organism, or to remedy morbid secretions. It has been employed, for example with great benefit, in albuminuria, diabetes, and serous infiltrations.

From these considerations, Dr. Garnier has been induced to employ tannic acid in the albuminous anasarca consecutive to scarlatina; and he adduces several cases illustrative of this mode of treatment, drawn from his own experience and cases recorded by other physicians. The cases all prove that in the general serous infiltration of the tissues complicated with albuminous urine, there is a rapid and simultaneous disappearance of these two morbid phenomena under the influence of tannin alone, administered in a large dose. The conclusions drawn by Dr. Garnier are that tannin, employed in doses of two to four grains a day (ʒss to ʒj), cures anasarca or œdema developed passively and occurring simultaneously with albuminous urine; that its curative action is manifested by abundant urine, gradually resuming its physiological characters, by perspiration, easy alvine evacuations, return of appetite, &c.; that these signs appear from the second day of the administration of the tannin; that given in solution in doses of twenty to fifty centigrammes at a time, tannin causes no unfavourable symptoms affecting the digestive passages; and lastly, that the action of tannin appears to be exerted primarily upon the fluids of the economy, the albuminous principles of which it coagulates and renders plastic, and that its consecutive action on the solids appears to be tonic and astringent.

II. *On the Employment of Veratria in Acute Diseases of the Chest.* (Bulletin Général de Thérapeutique, Jan. 30th, 1859.)

M. Aran has called the attention of practitioners to the remarkable effects produced by the internal use of veratria in febrile diseases, and especially pneumonia. In the 'Sardinian Medical Gazette' an article has appeared, in which Dr. Ghiglia, without any knowledge of M. Aran's researches, recommends the use of veratria in the same circumstances, except that he never employs this alkaloid alone, but associates it almost always with opium, sometimes in the form of pill, sometimes as a syrup. The dose of veratria is five millegrammes (·077 of a Troy grain) in a pill with the same quantity of opium, and the number of pills to be taken in the twenty-four hours varies from six to seven, and even twelve, according to the circumstances. In this dose, according to M. Ghiglia, vomiting rarely occurs, but nausea and the other depressing effects of veratria are present. The results obtained by M. Ghiglia in certain cases of pneumonia, bronchitis, and broncho-pneumonia have been sometimes most remarkable, but have been occasionally unfavourable, and the following are the results arrived at by this author: "1. The inflammation of the respiratory organs, when they have arrived at such a period as to produce disorganization of the parts, are not improved by the use of veratria. 2. The action of this substance is the more favourable in proportion as the disease is more recent. 3. The tolerance is very various, according to individual habits, and perhaps also according to certain peculiarities which are not yet well understood. 4. The more easily the tolerance ceases the more marked is the depression. 5. Veratria is in many respects a preferable medicine to others which are more constant in their action but less easy to take. And 6. It is perhaps prudent, in severe inflammations of the respiratory organs, to order a few bleedings before prescribing the veratria.

III. *On the Poisonous Properties of the Ethereal Oil of Elemi.* By Dr. Emil Mannkopff. (Virchow's Archiv für pathologische Anatomie und Physiologie und für Klinische Medicin, No. 15, 1859.)

The ethereal oil of elemi is obtained from the resin of elemi, brought from Brazil and Yucatan. It is found in the resin in very variable proportions, but on the average it may be considered to exist in the quantity of about six per cent. The oil is transparent, almost colourless, having a smell which is not unpleasant, and a somewhat acrid and bitter taste. It is insoluble in water, but is easily dissolved in alcohol or ether. According to Stenhouse and Deville, the analysis of the oil of elemi gave the formula of 5C 8H.

Dr. Maunkopff made a series of experiments on rabbits and frogs, with a view of ascertaining the operation of this oil upon the animal economy. In the case of the rabbits, the oil was injected into the stomach by means of an elastic catheter; in frogs the same operation was partially resorted to, but in some the oil was injected under the skin of the back. In a few cases the oil was applied locally on particular parts of the frogs.

The conclusions drawn by Dr. Mannkopff as to the operation of the oil of elemi on animals are the following: namely, that when applied to the intestinal tract, it produces hæmorrhagic erosions and numerous functional disturbances in the stomach; and in the intestines, increased peristaltic action and sensation of pain, succeeded by paralysis and anæsthesia. The oil acts in a similar manner wherever it is applied directly. After absorption it paralyses the sensitive portion of the nervous system and the nervus vagus. Hence arise, 1. Acceleration and increased strength of the movements of the heart, and consequently increased diuresis, which is connected with an inflammation of the kidneys to a greater or less extent, and perhaps of the bladder, produced by a specific irritation of the poison; and there is afterwards a sensation of thirst. 2. The diminution and gradual suppression of the respiration occasioned by atelectasis and emptying of the lungs, and diminution of temperature, with which deficient nutrition is associated as a cause. In the second place, when paralysis of the heart is at last produced, death ensues, probably due to the abolition of the functions of the brain.

In concluding his paper, Dr. Mannkopff introduces the question as to the use which may be made of the ethereal oil of elemi as an internal remedy. In this point of view three circumstances ought to be taken into consideration. 1. The increase of the heart's contractions. 2. The increase of diuresis. 3. The anæsthetic effects on the sensitive nerves. The oil might probably be given in the cases in which oil of turpentine is found useful, for both oils coincide very much in their medicinal properties. Perhaps the oil of elemi might be preferable from its less unpleasant taste. The question may be asked whether the oil of elemi might not cause inflammation of the kidneys when given in a sufficient dose; but this point can only be determined by experiments on the living subject. Still, since oil of turpentine produces a decided effect in neuralgia, as is proved by the operation of this oil on the sensitive nerves, Dr. Mannkopff believes that experiments with oil of elemi in similar cases would be completely justified.

IV. *Process for Ascertaining the Proportion of Quinine in Cinchona Bark.* (Bulletin Général de Thérapeutique, Oct. 30th, 1858.)

This process, published by M. Guillermond in the 'Gazette Médicale' of Lyons, is a modification of one already published by the same writer in 1847. The following is the process: Take twenty grammes of yellow bark, powder it without leaving any residue, and pour upon the powder alcohol at 76°, in sufficient quantity to form a soft paste, which is to be heated for a few minutes until the fibre is thoroughly penetrated by the liquid; then introduce into the paste ten grammes of hydrated lime in fine powder: mix thoroughly, so as to form a homogeneous mass, which is to be heated on a plate until all humidity is completely eliminated. This powder is afterwards to be treated with 100 grammes of rectified sulphuric ether, which will dissolve and remove all the quinine. This ether is then to be rapidly evaporated at the heat of boiling water, and the residue will contain only the quinine and a small proportion of a yellow colouring matter, which may be neglected. In order to determine the quantity of quinine obtained, either of the three following methods may be adopted: 1. Dry completely the ethereal residue. Its weight will give that of the quinine, plus the portion of colouring matter, the quantity of which is insignificant. 2. Dissolve the residue in a little alcohol and acidulate it with very dilute sulphuric acid, the saturating power of which for quinine is ascertained. For this purpose a graduated tube may be employed, so that a given quantity of the dilute acid corresponds to one gramme of quinine. 3. Weigh the sulphate of quinine which is obtained. It may be dried in a few moments by exposing it to the sun or to the heat of a stove.

This process, according to M. Guillermond, is recommended by its simplicity, and the celerity with which the result is obtained, for only about three hours are required in the operation: and the plan possesses the additional advantage of separating completely the whole of the alkaloid.

V. *On a Case of Scrofula Cured by Iodized Food.* (L'Union Médicale, Oct. 19th, 1858.)

M. Lebert, surgeon of the Hospital of Nogent-le-Rotrou, records a case of scrofula cured by the use of iodized food, as recommended by M. Boinet. The patient was a youth of sixteen, of a very lymphatic temperament, and belonging to a family some members of which had died of phthisis, and several others had presented symptoms of scrofula. He had suffered from his infancy from an enlargement of the cervical glands, almost always terminating in suppuration. When one swelling began to disappear, another developed itself in its vicinity, so that the neck was the seat of a constant discharge and of numerous unsightly scars. The disease had also attacked the mastoid process of the right side, and had produced a fistulous opening, which gave a passage occasionally to some fragments of bone. The general health was very bad, the appetite very irregular, the digestion difficult, the weakness very great, and the complexion extremely pale. The patient had been subjected for five or six years to active treatment, consisting of bitter infusions, iodide of potassium, and cod-liver oil, purgatives, blisters, and salt-water bathing, but without any good result. At last, M. Lebert had recourse to the use of *iodized bread*, as recommended by M. Boinet, and omitted all other medical treatment. From the period of the adoption of this plan a favourable change in all respects was observed in the patient, and after he had eaten the medicated bread for eight months, he could scarcely be recognised. Not only were there no more swellings or abscesses in the neck, but all the fistulous openings had remained closed for several months, and even the scars had become white and quite regular in appearance. The general condition of the patient was quite satisfactory, the appetite uniformly good, the digestion excellent, the face well-coloured, and the stature and the strength of the body quite natural; and, in fact, the use of the iodized bread had performed a perfect metamorphosis in the condition of the patient. M. Lebert remarks, that the superiority of this kind of treatment appears to depend upon the circumstance that chronic diseases yield only to the slow and gradual use of remedies, while medicines administered in the ordinary doses most frequently fail. In the latter case the stomach appears to be fatigued by the presence of the medicines, which it can no longer assimilate.

VI. *On the Employment of Chlorate of Soda in Mercurial Stomatitis.* (Gazette Médicale, Oct., 1858.)

From the experiments which have been made on the use of the chlorates of potash and soda, it appears that the action of these salts depends more upon the acid than upon the base, but that the greater solubility of chlorate of soda and its less marked taste ought to give it the preference. M. Mussat employed the chlorate of soda in six cases of stomatitis with very favourable results, and the patients stated that this salt had a less disagreeable taste than the chlorate of potash.

VII. *On the Local Use of Perchloride of Iron in the Treatment of Membranous Angina.* (Gazette des Hôpitaux, Oct., 1858.)

Perchloride of iron has been employed with some success in the treatment of membranous angina (*Angine Couenneuse*). M. Gigot, of Levroux, after having ascertained by experiment the effects of the styptic action of perchloride of iron on fresh pseudo-membranes recently removed from the throats of patients, employed this agent in a certain number of cases of diphtheritic angina, during a severe epidemic of this disease which visited Levroux. He applied the perchloride on the pharyngeal mucous membrane and the diphtheritic exudations by means of a sponge or a piece of lint. The first effect of this application was the immediate expulsion of mucous matters, which were coagulated by the perchloride, and either expectorated by the patient or left adhering to the sponge. The slender and slightly adherent pseudo-membranes were also immediately detached, but the most adherent were removed only in small fragments similar to portions of muscular fibre macerated in water. M. Gigot treated ten patients with this agent, of whom one, an infant four years old, died from the extension of the disease to the larynx. Of the other nine, there were two in whom the perchloride of iron was replaced, at the end of two days, by the bicarbonate of soda. In these patients the pseudo-membranes were reproduced between each application of the perchloride of iron, and were always easily removed. Lastly, in the seven others the angina was arrested in a few days. M. Gigot never applied the perchloride more than twice in the twenty-four hours. One of the cases was a very well-marked instance of membranous angina in a girl of seventeen, living in a locality where two children had recently died of that disease. There was great swelling of the glands of the neck, fetid breath, difficult deglutition, pulse 110, vomiting, and epistaxis. A greyish pseudo-membrane, of a fibrinous appearance, covered the whole of the right tonsil and extended all along the pillar of the velum palati. The perchloride of iron was applied three times, by means of a piece of lint, over the whole of the pharyngeal mucous membrane as far as the epiglottis, and at the second application the false membranes were detached, their volume was diminished, and they were shrivelled and dried by the perchloride. A gargle of bicarbonate of soda was employed on the same day, and after four days of this treatment the patient entirely recovered.

VIII. *On the peculiar Efficacy of Sulphate of Copper in Exciting vomiting in the Treatment of Croup.* By Dr. Missoux. (Bulletin Général de Thérapeutique, Dec. 30th, 1858.)

The importance of repeated vomiting in the treatment of croup is admitted by many practitioners, but the choice of an emetic is a point which has not hitherto been fully determined. Since vomiting has been considered by some as the mechanical act which induces the detachment of the false membranes, tartar emetic has been employed for the purpose. Others have preferred ipecacuanha, the dynamic action of which is less depressing than that of tartar emetic, but there its superiority ends. The sulphate of copper, in addition to its emetic action, possesses a very remarkable property of acting locally, and this peculiarity makes it superior to tarter emetic and ipecacuanha. With the latter substances, the patients derive benefit only from the mechanical act of vomiting, and when the false membranes are expelled, others are formed. The case is quite different with sulphate of copper, for when a solution of this salt is employed, the secreting surfaces are so modified, that no more false membranes are formed, or if they are formed, they no longer present the plasticity which renders them so adherent to adjoining parts. Dr. Missoux, after a practice of eighteen years, states that the sulphate of copper has been in his hands the most successful emetic agent in the treatment of croup. Its purifying action appears to him the more valuable, because diphtherite (croup) at its commencement is often localized in the throat, and by applying remedies early, the extension of the false membranes to the larynx may be prevented. He wonders that this topical action of the copper salt has not been hitherto observed upon plastic exudations which are visible to the eye, such as cutaneous diphtheria, and that of the vulva, the throat, and the nose, for its effects in these complaints would have induced a speedy conviction of its utility. The dose in which Dr. Missoux administers the sulphate of copper is rather larger than that prescribed by other physicians. For young children he dissolves a quarter of a gramme of the salt in 125 grammes of distilled water, and orders a teaspoonful to be given every ten minutes, until vomiting is produced. After the age of puberty, and in adults, he increases the dose to one gramme, without his ever having witnessed any poisonous effects. The more the solution is concentrated, the more frequently the doses are given, and the earlier its administration is resorted to, the more prompt and certain are the effects of the treatment. Out of thirty diphtheritic cases, Dr. Missoux lost only two. This result may surprise some readers, but he assures the profession that he has determined the existence of croup only after actually observing the presence of the false membranes in the bronchi, trachea, and larynx.

IX. *On the Action of Fatty Bodies in the Absorption and Assimilation of the Metallic Oxides.* By Dr. Jeannel of Bordeaux. (L'Union Médicale, Feb. 15th, 1859.)

Dr. Jeannel has made a number of experiments, from the results of which he is led to believe that he will be enabled to explain the very obscure question as to the part taken by the acid or neutral fatty bodies of the blood in the absorption and assimilation of the metallic oxides. These experiments will also, he thinks, contribute to support, in a novel point of view, the opinions offered by MM. Arthaud and Dupasquier as to the hygienic operation of the bicarbonate of lime, which is in fact a mineral aliment. They will at least prove this singular fact, that in the experiments of the laboratory, the bicarbonate of lime of the potable waters may become the medium of dissolving the metallic oxides in the fat oils. In fact, he hopes to prove that several important salts, as sulphate of iron, potassio-sulphate of iron, sulphate of copper, bichloride, biniodide, and protochloride of mercury, being decomposed by the bicarbonate of lime of the potable waters, by the alkaline carbonates or bicarbonates in a weak solution, or by the carbonates of the alkaline animal liquids in presence of the fatty oils, are dissolved in considerable proportion by the latter, especially at the temperature of the body. Dr. Jeannel observes that the metallic salts introduced into the stomach, if they resist the solvent action of the acid liquids in the first digestion, and if they are not absorbed with the water taken as drink, must necessarily pass into the intestine, and there undergo the decomposing action of the alkaline liquids in presence of the fatty matters with which the oxides form combinations susceptible of being absorbed under the form of emulsion. He also finds in liquid blood, which is at once alkaline and fatty, the elements which his experiments proved, when artificially united, to be capable of transforming the mineral salts dissolved in water into insoluble but deleterious oleo-stearates or oleo-margarates; and he is led to believe that the fatty salts are the definitive form assumed by the metallic salts, whether they pass directly into the blood in the form of a weak aqueous solution, or undergo decomposition by the alkaline intestinal liquids. It would therefore be under the form of fatty salts that we ought rationally to administer metallic agents, when we propose to moderate irritability of surface and to obtain constitutional effects.

From the results of numerous experiments, Dr. Jeannel draws the following conclusions:

1st. A solution of a metallic salt being decomposed by an alkaline carbonate in presence of a fatty oil in excess at the ordinary temperature, a part of the metallic oxide passes in solution into the fatty body. This reaction is favoured by a temperature of 40° Cent.

2. The bicarbonate of lime of the potable waters decomposing very weak metallic solutions, the oil which is shaken in the mixture seizes the metallic oxide, at least in part.

3. The alkaline animal liquids, as serum of the blood, milk, and white of egg, being placed in contact with a metallic salt in weak solution, and oil, the alkaline carbonate contained in the animal liquids is most frequently sufficient for decomposing the salt, the oxide of which is dissolved in notable proportion in the fatty body.

4. If it is supposed that an aqueous solution of a metallic salt having escaped digestion in the stomach, arrives as far as the intestine, it must be admitted that it is there decomposed by the alkaline animal liquids mixed with fatty matters, and that the metallic oxide enters in solution into the latter.

5. The same facts and reasonings lead us to admit that an aqueous solution of a metallic salt arriving at the blood undergoes at first a double decomposition, the final consequence of which is the formation of a fatty salt.

6. Calomel is decomposed by a weak solution of bicarbonate of soda; chloride of sodium, and probably bichloride of mercury, are formed and dissolved together. The presence of chloride of sodium retards this decomposition and solution.

7. Calomel being mixed with water containing bicarbonate of lime or bicarbonate of soda in solution, if oil is shaken with the mixture, it becomes charged with a notable quantity of mercury. All these reactions are favoured by a temperature of 40° Cent.

8. If in the administration of medicines from which a constitutional or dynamic effect is desired, we would endeavour to imitate the compounds formed naturally in the organism, we ought to prefer the form of fatty salts in the administration of metallic agents.

X. *On the Diuretic Action of Iodide of Potassium.* By C. Handfield Jones, M.D., F.R.S. (Beale's Archives of Medicine, No. 8.)

It appears reasonable to expect that the healing influence of a drug in certain morbid states may be shown to be explicable by its general mode of action, yet there are certain remedies which exert a very positive curative power, and yet afford no clue in their general mode of action to explain their special effects. Such a remedy, according to Dr. H. Jones, is iodide of potassium, which has certainly a strong controlling power over periosteal inflammations, whether syphilitic or rheumatic, as well as over rupial ulcers, which generally heal under its use. It is also more or less useful in inflammations affecting fibrous tissues in various parts. Dr. Jones has made a series of experiments upon the effects of iodide of potassium administered to patients, and has examined the urine in each case; and the general results are thus summed up:—1. That the quantity of water was greatly increased in three out of six cases; a little (one-sixth) increased in one; diminished in two. 2. Out of five cases, the acidity was increased in three, diminished in two. 3. Urea was increased in three, diminished in three. 4. Phosphoric acid was increased in four, diminished in two. 5. Sulphuric acid was increased in four, diminished in two. 6. Chlorine was increased in three, diminished in two out of five cases; in two the increase was very considerable. 7. Uric acid was diminished in four out of six cases, greatly increased in the remaining two. The most marked effects seem to be the increase of the water, of the phosphoric and sulphuric acids, and of the chlorine. But Dr. Jones adds that as far as these confessedly empirical results go, there seems to be no help or clue afforded to trace out any connexion between the empirical facts just noticed. A varying diuretic effect does not give any explanation of the *modus operandi* of the drug in curing a node or an ulcer. For the present Dr. Jones concludes that we cannot attain to more than an empirical acquaintance with the operation of iodide of potassium.

XI. *On the Therapeutical Action of the Acid Nitrate of Silver.* By Dr. Crocq. (Bulletin Général de Thérapeutique, Feb. 15th, 1859.)

Under the name of acid nitrate of silver, Dr. Crocq designates a solution of nitrate of silver in nitric acid. He thinks this preparation especially useful when it is desirable to modify certain surfaces more or less deeply without producing a deep destruction of the tissues; in such cases, in fact, as are usually treated by the solid nitrate of silver or by the acid nitrate of mercury. The acid nitrate of silver is preferable to the former, because it penetrates much better into all the sinuosities and anfractuosities of surfaces, and because its action can be rendered either superficial or deep. It is preferable to the second, because it does not act as a poison by absorption, however large may be the surface cauterized, while the acid nitrate of mercury may and does produce symptoms of mercurial poisoning. Moreover the action of the acid nitrate of silver may be immediately arrested, when it is applied to organs where its extension might become prejudicial, as on the eye, the vagina, and in the throat, for in these cases the injection of a solution of chloride of sodium renders it instantly inert. The acid nitrate of silver may be employed advantageously in cases of chancre, in simple and gangrenous ulcers, in some wounds, in lupus, in epithelial tumours, and cancroid ulcers; in ulcerations of the neck of the uterus, and granular affections of the cervix uteri and of the conjunctiva.

XII. *On the Treatment of Inflammation by Digital Compression*, by M. Vanzetti, Professor of Clinical Surgery in the University of Padua. (From the Giornale Veneto di Scienze Mediche, l'Union Médicale, Dec. 30th, 1858.)

We have already (Jan. 1859) recorded the results of two cases in which M. Vanzetti successfully practised digital compression in inflammation; and the following cases appear to establish the advantage derivable from this method of treatment:—

The first case was one of phlegmonous inflammation of the right leg, occurring in a man, aged thirty-eight. When he was admitted into the hospital the right leg was red and considerably swollen throughout its lower half as far as the metatarsal bones; the redness was most intense over the internal ankle, which appeared to be the most painful part, and the centre from which the inflammation extended to the rest of the limb. The pain was increased on pressure, especially at the internal ankle, and the finger left a well-marked depression on the leg in consequence of the acute œdema of the subcutaneous areolar tissue. Compression was immediately commenced—namely, at seven o'clock in the evening; and the pulsations of the femoral artery, which was compressed at the ilio-pectineal eminence, were so violent that it was necessary to employ some force in order completely to interrupt the circulation. M. Vanzetti saw the patient at eight o'clock, and found that the black thread with which the limb had been surrounded was already relaxed; the redness was much less intense, the pain had almost entirely ceased, and the patient said that he felt remarkably relieved. The compression was continued during the whole of the night, with some short interruptions, and on visiting the patient the next day, at half-past ten in the morning, a surprising change was found to have taken place in the state of the affected limb, which no longer presented any of the symptoms of phlegmon which had been so distinct on the previous evening, and it was therefore considered unnecessary to continue the compression. In order to assure himself that the affected parts had returned to their normal state, M. Vanzetti caused the patient to rise and walk several times along the ward; he did not complain of the slightest pain in walking, and he refused the aid of a stick which was offered to him. The man recovered completely, and on the third day after the commencement of the treatment he returned to his work.

The second case was one of traumatic phlegmon of the left hand, occurring in a girl of fourteen, who, having had a fall while holding a bottle, received a wound in the palm of the left hand from a fragment of glass. On the sixth day after the accident fever supervened, and acute pain was felt in the hand; and the next day the girl came to the hospital. The hand then exhibited, especially on its dorsal surface, an intense redness, with considerable swelling and pain on pressure, and the inflammation extended to the whole of the circumference of the fore-arm. The thumb and the index finger were also swollen—red, tense, and shining; the pulse was 108. Compression of the humeral artery was now resorted to, the pulsations of the radial artery at the wrist indicating the exact degree of the compression; but the patient was also able to explain whether the process was well or badly performed by the symptoms which she experienced. After only two hours the compression had produced a sensible amelioration; the thread which had been tied over the most projecting part of the hand already allowed the little finger to pass beneath it, and the heat and redness had diminished. In six hours afterwards the symptoms had diminished still more; the compression was continued without preventing the patient from sleeping, and the subclavian artery was occasionally compressed. On the next day the redness was found to be circumscribed to the back of the hand; the index finger was now easily passed beneath the thread employed to measure the size of the limb; the heat was less, and the pain had entirely ceased. The pulse was 100. Two days afterwards no trace of inflammation remained; but it was now ascertained that a small fragment of glass was imbedded in the tissues of the hand. It was extracted without difficulty, and in a week more the patient was quite well.

The third case was one of diffuse phlegmon of the left leg, occurring in a man, aged fifty-six. There was considerable swelling of the whole of the limb, the circumference of which exceeded that of the sound limb by three inches; the tissues were tense and elastic, but there was no œdema, except at the middle portion of the anterior surface of the tibia, which was also the seat of a very acute pain on pressure. Compression was practised, and during the first few hours which followed the adoption of this measure the patient already experienced considerable relief, and he said that he no longer felt the sensation of heat by which he was previously so much annoyed. Compression having been commenced at eight o'clock in the morning, there was so much amelioration at four o'clock in the afternoon that the index finger could be passed under the thread employed to measure the size of the leg. At nine in the evening the improvement still continued. During the night the compression was continued, but with short intervals, and the patient, satisfied with the relief afforded by the process, performed it himself, but interrupting it from time to time in order to sleep. The next day at ten o'clock in the morning, all the symptoms of the disease had entirely ceased; there was no longer any swelling or tension of the tissues. The pulse was 56. The patient could walk without difficulty or pain; and after remaining six days in the hospital he went away perfectly cured.

XIII. *On the Therapeutical Action of Solanine and Dulcamara.* (Presse Médicale Belge, September, 1858.

Professor Caylus, of Leipzig, has undertaken a series of experiments to ascertain the exact effects of dulcamara, and its active principle, solanine. These substances belong to the class of narcotico-acids, as they produce a paralysing action on the medulla oblongata, and an exciting action on the nerves. They cause death by producing paralysis of the respiratory muscular apparatus, by an action analogous to that of coneine and nicotine. They possess a therapeutical action in spasms and irritable conditions of the respiratory organs, in simple spasmodic cough, hooping-cough, and spasmodic asthma. Their therapeutical action in certain morbid conditions of the blood—as gout, rheumatism, constitutional syphilis, and perhaps in certain chronic diseases of the skin—may be due to their augmenting the excretion by the kidneys, of the constituent parts of the blood which have undergone combustion, and not to the excitement of cutaneous activity. Solanine and dulcamara may be given without danger in inflammatory conditions of the stomach and the intestinal tube, as they exercise no action on those organs. Inflammation of the respiratory organs presents no contra-indication to the employment of solanine and dulcamara; but they are contra-indicated in inflammation of the kidneys. The medium dose of solanine for an adult is from one to five centigrammes of acetate of solanine, a substance which M. Caylus prefers to the pure alkaloid, in consequence of its solubility. The most suitable form of administration is in pills, the solutions of the salts of solanine having a very disagreeable taste. The extract obtained from alcohol, and then washed with water to remove the alcohol, is preferable to the watery extract generally employed.

XIV. *Some Formulæ employed by the Medical Profession in Lyons.* (Bulletin Général de Thérapeutique, Jan. 30th, 1859.)

1. *Antidyspeptic Powder.*—Take of subnitrate of bismuth, twenty grammes; of hydrochlorate of morphia, five centigrammes. Mix, and divide into twenty powders; one to be taken immediately after each of two meals, in two tablespoonfuls of water.

2. *Collyrium in Chronic Ophthalmia.*—Take of water, one hundred and twenty-five grammes; of tincture of aloes, ten drops; of ammonia, four drops; of sulphate of copper, five centigrammes. The eyes to be bathed twice a day for two minutes. This wash is principally indicated in chronic scrofulous ophthalmia accompanied by obstinate ulceration of the cornea. It should only be employed after the cessation of acute symptoms of inflammation.

3. *Pommade for Ulcerated Chilblains.*—Melt sixteen grammes of yellow wax in thirty grammes of linseed oil; add eight grammes of tincture of benzoin and fourteen grammes of glycerine, and flavour with essence of lavender.

4. *Powder for Nocturnal Incontinence of Urine in Children.*—Take of carbonate of iron, fifteen centigrammes; of extract of belladonna, three centigrammes; of powdered nux vomica, three centigrammes. This dose to be taken every day. The employment of this remedy is said by Dr. Faure to be generally followed by a complete cure at the end of eight or ten days.

5. *Pommade for Acne.*—Take of lard, fifty grammes; of sublimed sulphur, four grammes; of tannin, four grammes; of cherry-laurel water, five grammes. Mix accurately. Dr. Rodet employs this pommade with success in all forms of acne, and has also found it serviceable in sycosis, when the inflammation has been subdued and the crusts have fallen.

6. *Laudanized Wine of Squills.*—Take of white wine a pint; of powdered squill, eight grammes; of laudanum, twenty-nine grammes. Dr. Teissier has obtained advantageous results from the employment of this wine in dropsies especially those which follow pulmonary emphysema.

XV. *On the Vermifuge Properties of the Chinese Ailantus.* (Journal de Pharmacie et de Chimie, March, 1859.)

The Chinese ailantus (*Ailantus glandulosa*) has been hitherto known only as an ornamental tree of a very elegant appearance and rapid development. During the last few months it has acquired importance by yielding a valuable vermifuge agent, according to some remarks published by M. Hétet, Professor at the Naval School of Medicine at Toulon. In the experiments which he describes, he makes use of the following preparations, namely, the powdered bark, the powdered leaves, the watery extract of the bark, the alcoholic extract of the bark, the oleo-resin, and the resin. These experiments were made on dogs, and afterwards on man. M. Hétet describes three cases, in all of which the powdered bark of the ailantus caused the expulsion of tapeworms. The powder of the bark was given at first in the dose of half a gramme, and the watery extract in the dose of one-fourth of a gramme; the oleo-resin in the dose of one fifth of a gramme: the resin in the dose of two-fifths of a gramme rarely occasioned the expulsion of fragments of the tænia. M. Hétet thinks that it is to the volatile oil of ailantus that we ought chiefly to attribute the phenomena of weakness observed in man and in dogs, since the resin alone does not produce them. He also observes a fact deserving of recollection, namely, that the effect of this essential oil is so well-marked, that it is necessary for persons to take great care of preserving themselves from its vapours during its preparation. According to this author the ailantus, taken in a vermifuge dose, does not exert any injurious effect upon the health, and does not distress the patient like the root of the pomegranate and kousso. The local effects are confined to slight colic, and sometimes a moderate degree of purging.

XVI. *On the Treatment of Chorea by Arsenious Acid.* By M. Aran. (Bulletin Général de Thérapeutique, March 30th, 1859.

In concluding the history of five cases of chorea treated and cured by the employment of arsenious acid, M. Aran remarks that he does not consider arsenic to be a specific in this disease. The arsenical treatment must inevitably fail in a certain number of cases; and M. Aran himself failed completely in four, where hysteria co-existed with choreiform movements. He considers that arsenic succeeds best in those cases which, without losing their affinity to the type of chorea, manifest themselves in a somewhat anomalous form; in other words, that the arsenious acid is an anchor of safety in anomalous forms of chorea, whatever may be the nature of the anomaly. Thus, one of his cases was complicated with hallucinations, a second was accompanied with incomplete hemiplegia, and a third with a very extraordinary agitation, occurring principally in the morning. But simple cases of chorea may also be advantageously treated by arsenic, and this medicine ought to have a fair trial in this disease, until it is proved to be inefficacious.

The question of the dose of arsenic to be administered in this disease is one demanding much consideration. M. Aran considers that for children of seven years old, the dose may commence at two or three milligrammes (a milligramme is about 0·0154 of a troy grain), and may be augmented to five milligrammes. In the adult the first dose may be five milligrammes, or one centigramme (·1543 of a troy grain), and he thinks it important to augment the dose with rapidity, so as to reach, in three or four days, to one centigramme, or one centigramme and a half in the child, and to two or three centigrammes in the adult. This rapid mode of administration is better than the long continuance of small doses, because the economy habituates itself to the latter, and the therapeutical effects may be lost; and this proceeding has the additional disadvantage of leading more easily to the saturation of the economy, and consequently to intolerance. If a cure does not take place in a short time, on the plan recommended by M. Aran, namely, by the rapid administration of increasing doses, or if there are not any favourable changes in the disease, then the arsenic does not agree with the patient, and it must be discontinued, and some totally different medicine substituted.

For reasons already given, M. Aran does not approve the practice of continuing the use of arsenic for a long time in small doses. It may have been harmless in a certain number of cases, but this does not prove its utility. In conclusion, he observes that the arsenical treatment is undoubtedly efficacious in a certain number of cases of chorea; that it appears especially applicable to obstinate and anomalous cases of that disease; that nothing proves that it may not be employed with advantage in the treatment of simple and recent cases; that, employed prudently and cautiously, it does not produce any serious symptoms; and that the cure, when it takes place, is generally obtained in a very short time. "Such," says M. Aran, "are the considerations which, as it appears to me, ought to recommend this treatment to the attention of the medical profession; I do not hope that the prejudices of physicians will completely disappear before so small a number of cases as I have just related; but let them try the remedy, and if they meet with results similar to those I have recorded, they will arrive, like myself, at the conclusion that this medicine is one of the most remarkable therapeutical weapons we possess against a very serious and obstinate disease."

XVII. *On the Influence of Mercurial Preparations upon the Secretion of Bile.* By Dr. George Scott. (Beale's Archives of Medicine, No. 3.)

Dr. Scott relates the details of some experiments made on dogs, with the view of ascertaining whether the preparations of mercury really increase the flow of bile, as has hitherto been generally believed. In these experiments the ductus communis choledochus was tied, so as to prevent any bile from reaching the intestine, and the gall-bladder was opened in order to allow all the bile secreted to escape externally. It was then collected in an apparatus constructed for the purpose, and calomel being given at different periods, the quantity of the bile secreted was carefully noticed. In the first place, however, Dr. Scott ascertained the normal amount of bile secreted by a dog's liver in the twenty-four hours. The quantity amounted on an average of three days to 2752·562 grains of fluid bile each day, the average weight of the dog being seventeen pounds, and the average amount of food being 7000 grains, and of drink nineteen ounces, or 8312·5 grains of milk.

In determining the effects of calomel on the secretion of bile, Dr: Scott calculated the average amount of bile secreted in twenty-four hours two days *previously* to the administration of the drug, and then the average amount secreted in twenty-four hours two days *after* the calomel was given. The calomel was given each time after the morning's bile was collected, and therefore the effect of the medicine was upon the bile of the day following that on which it was given. The four experiments performed by Dr. Scott all gave the same rather paradoxical result—namely, that *there was a diminution in the amount of fluid bile and bile-solids secreted after the administration of large doses of calomel.*

Although Dr. Scott considers that it would be rash to venture any decided opinion from the results of four experiments, yet these all point so much to one conclusion that if they should be confirmed by future and more varied trials, they will throw considerable doubt upon the generally-received opinion that calomel in large and purgative doses increases the flow of bile. It may be urged, he adds, that although calomel does not increase the secretion of bile in the dog, there is no reason why it may not do so in man, and that even if mercury does not excite the liver to increased secretion in a healthy state of the organ, it may yet do so in some of its diseased conditions. But if the first objection were true, the same could be urged against the results of experiments on the lower animals to ascertain the action of poisons or any articles of the materia medica. With regard to the second objection, nothing analogous occurs in the action of drugs upon other organs, and it seems difficult to suppose that anything which diminishes the flow of bile in a healthy condition of the liver, should increase it in a diseased state of that organ. Whether it be the mere purgative effect of calomel which causes the diminution in the secretion of bile, or some specific action, must be decided by further experiments. It is also a matter for further inquiry, whether small and frequent doses of calomel continued for a length of time, so as to produce the specific action of mercury upon the system, may not really augment the biliary secretion.

XVIII. *On the Use of Raw Meat in the Colliquative Diarrhœa of Children at the Breast.* By Dr. J. F. Weisse Director of the Children's Hospital at St. Petersburg. (L'Union Médicale, April 7th, 1859.)

Seventeen years have passed since Dr. Weisse drew the attention of the profession to the beneficial effects attending the use of raw meat in the treatment of the colliquative diarrhœa of children at the breast, and since that time numerous writers have confirmed the views originally advanced. Dr. Weisse now declares, after an experience of twenty years, that raw meat, reduced into a pulp by scraping, to the exclusion of all other treatment, is the true specific for this destructive form of diarrhœa. He cannot agree with the remarks of Mr. Charles Hogg, who recommends beef-tea in preference, for he finds in raw meat not only an aliment for the children, but also a remedy for the kind of diarrhœa in question; besides, he has never spoken of the juice of the meat, but has recommended the muscular substance itself, minced or scraped, so as to be swallowed and digested without difficulty. The object proposed is to introduce into the digestive tube the muscular substance of the meat, and the beef-tea has no controlling power over the diarrhœa, for by its mere fluidity it traverses too rapidly the intestinal canal. By giving the meat in a pulp, the solid parts remain longer in the intestine; they act by contact, and may, by exciting the intestinal mucous membrane, stimulate absorption; and it is probable also that this plan may contribute to neutralize the acidity of the gastric juice. The treatment of children's diarrhœa by raw meat has become general at St. Petersburg, and has been adopted rather by the establishment of the good effects which have resulted from it than by the publication of special memoirs. Dr. Weisse has employed the treatment in nearly 200 cases, and the result has been always satisfactory when the case has been taken in time. When the disease is too far advanced, and has assumed the character of gastromalacia, a cure is seldom obtained; but even in such circumstances it is possible to mitigate some of the symptoms felt by the patient, such as the inextinguishable thirst and the vomiting.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Seiveking, M.D.

Fellow of the Royal College of Physicians, Physician to, and Lecturer on Materia Medica at, St. Mary's Hospital.

I. *On Cerebral Deficiency and Hydrocephalus.* By Professor R. Heschl, in Cracow. (Prager Vierteljahrsschrift, xvi. Jahrg. 1859, Erster Band, p. 69.)

Professor Heschl has observed four cases of deficiency of the central gyri of the brain and of the adjoining parts. The first case occurred in an idiotic beggar, who died at the age of twenty-six, who had only had an imperfect power of executing the ordinary movements, and whose range of speech had been very limited. His left side was more powerless than the right. The right parietal bone bulged out, the most prominent parts being reduced to form one-half a line to one line in thickness, while the remainder of the cranial vault was from two to two and a half lines thick. The left cerebral hemisphere and its membranes were essentially normal, nor was any malformation perceptible in the cerebellum, medulla oblongata, and spinal cord. In the right cerebral hemisphere the whole middle portion of the centrum ovale, together with the convolutions appertaining thereto, both on the surface and at the Sylvian fissure, were absent, so that on removing the dura mater one at once looked into the patulous lateral ventricle. The space was occupied by a large bladder with very thin parietes, containing about a pint of clear serum. This bladder evidently consisted of the distended internal cerebral meninges of the defective portion; it was in close proximity externally with the dura mater, and was intimately united to the arachnoid and pia mater of the remainder of the hemisphere. The right corpus striatum and thalamus opticus was smaller than those of the left side; the septum was perforated at two points. The tissue of the brain was otherwise healthy, and nowhere showed any extravasation or traces of inflammation.

It is unnecessary to give the further details of this case, or to specify those of the remaining cases, which in their essentials are identical with the first. The author sums up the conclusions to which he is led by the analysis of these cases as follows:—

1. Idiots affected with hemiplegia exhibit a congenital defect in the brain, which gives rise to a communication between the lateral ventricles and the sac of the arachnoid. He terms this defect porencephalia.
2. This porencephalia is always associated with other defects in the formation of the hemispheres.
3. Porencephalia is probably not an arrest of development, but a true disease occurring during the growth of the brain.
4. It is sometimes associated with hydrocephalus, but as little as other cerebral defects explicable as the effect of intra uterine hydrocephalus.
5. Intra-uterine hydrocephalus is probably the *consequence* and not the *cause* of numerous malformations of the brain.

II. *On Partial Hypertrophy of the brain.* By Giacomo Sangalli. (Gazz. Lomb. 30, 1858, Schmidt's Jahrb., Jahrg. 1859, No. 4.)

Some doubt existing as to the reality of partial hypertrophy of the brain, and such cases at all events being very rare, the following deserve attention. It is right to add that we have not seen Dr. Sangalli's own account nor his illustrations, we therefore give the cases without further comment:—

1. A youth, aged nineteen, well formed, with unimpaired mental powers, had been out of health for some months; this became worse, and after a fortnight's serious illness he died comatose and icteric. The autopsy revealed acute yellow atrophy of the liver. The dura mater was yellowish, the pia mater normal. Convolutions of usual size; no change in the vascularity, consistency, or colour of the brain. On exposing the centrum ovale, the surface of the corpus callosum was found abnormally curved, with its convexity upwards. In the lateral ventricles there was a drachm of clear serum, and the left thalamus opticus was manifestly much larger than the right one, which was of normal size. The left thalamus retained the usual form; its long diameter was 47 millimetres (1·83 inch), its transverse diameter at the thickest part was 27 millimetres (1·05 inch), while the corresponding dimensions of its

fellow were 37 (1·44 inch), and 18 millimetres (0·70 inch). No material change was observed in the tissue of the part. The author is of opinion that the condition must have been congenital, or have been developed soon after birth, because there was another congenital defect—viz., material contraction of the entire aorta.

2. A peasant, aged twenty-seven, died of pleuro-pneumonia. On raising the anterior half of the brain a protuberance was discovered, connected with a convolution of the posterior part of the left middle lobe. It was of the size of a small hazel-nut, shaped like a wart, rather softer than the cerebral tissue, not covered by pia mater, which ceased abruptly at the commencement of the projection. The grey investment (of the protuberance) was 1 millimetre (0·039 inch) thick, and passed into the adjoining grey cortical substance; the subjacent medullary matter was normal. The microscopic examination showed few nerve fibres with double outline, many fine fibres of cortical tissue, and between them molecular granulations, nuclei with nucleoli, and some transparent nerve cells without remarkable prolongations. The other portions of the brain were perfectly normal, and neither the dura mater covering the protuberance, nor the bone above, were in the least altered. The man had never exhibited any cerebral disturbance.

3. A man, aged sixty, had died of severe diarrhœa, accompanied by severe abdominal pain, without manifesting marked cerebral disturbance. There was extensive suppurative enteritis, besides other abdominal lesions. In the brain was found an oval white-grey transparent body, of the size of a small bean, and with a smooth surface, situated on the middle of the right corpus striatum, external to the tænia semicircularis. Its longest diameter corresponded with that of the part from which it was developed. It was invested by a thin membrane traversed by delicate vessels. The tissue of which it was composed resembled the brain of a child; exhibited nothing to distinguish it materially from the remaining cerebral tissue, and like it, was traversed by capillaries. No other abnormity could be discovered in the brain, but the larger arteries exhibited a medium degree of fatty degeneration.

III. *On the State of the Mental Powers in Chorea.* By M. Marcé. (Archives Générales de Médicine, May, 1859.)

In a memoir, read before the Academy of Medicine, on the above subject, the author advances the following conclusions:—

1. The moral and intellectual functions are very commonly affected in choreic patients, at least two-thirds show some affection of the kind; the immunity enjoyed by the remaining third cannot be explained either by the age or the sex of the subjects, by the acuteness or chronicity of the disorder, nor by the extent or intensity of the convulsions.

2. Four morbid elements, which are sometimes isolated, but most frequently associated, should be studied together in the mental condition of chorea patients.

a. Derangements of the moral sensibility, consisting in a notable change of character, which becomes irritable and capricious, and may be unusually animated, but is more frequently depressed.

b. Derangements of intellect, characterized by a loss of memory, by too great a flow of ideas, and by the impossibility of fixing the attention.

c. Hallucinations which occur in the state intermediate between sleeping and waking, rarely in the morning and on waking; these hallucinations are commonly limited to the sense of sight, and rarely extend to hearing or general sensibility; they occur most frequently in hysterical subjects.

d. Chorea may from its commencement be complicated with maniacal delirium; this frequently terminates in death, or, if recovery takes place, intellectual disturbance remains.

IV. *Cases illustrating the Pathology of Mental Disease arising from Syphilitic Infection.* By John B. Chapin, M.D. (The American Journal of Insanity, Jan., 1859.)

The author argues that the defective nutrition of the brain resulting from the syphilitic diathesis perverts its healthy nutrition in such a way as to induce insanity. The perverted nutrition induced by the long-continued action of the morbific cause, the loss of sleep, the pain and other circumstances accompanying constitutional syphilis, cause, first, a change in the temper, affections, and morals of an individual, which gradually pass into the graver forms of mental disease. The mental disease may not alone arise from the effects upon the brain of this general cachexia, but from a direct syphilitic affection of some of the intra-cranial structures. "The fibrous structure of the dura mater, the fibro-serous tissue of the arachnoid; the periosteum and the cranial bones, are severally and collectively liable to syphilitic inflammation, presenting in itself no different characteristics from those observed when fibrous structures in other parts are affected. Yet, from their proximity and the relation they hold to the nervous mass, no serious lesion of either can occur without influencing the latter." The author dwells upon the effects of syphilitic inflammation upon the dura mater, as the part most frequently involved; he has no cadaveric evidence to offer, however. His views as to the pathology of some forms of mental disease are supported by a series of cases, partly culled from his own experience, partly from that of other writers, in which the subsidence of the mental alienation was coincident with the effect of the mercurial or other treatment directed towards combating the syphilitic symptoms which had manifested themselves.

V. *On Ataxie Locomotrice Progressive.* By Dr. DUCHENNE (de Boulogne). (Archives Générales de Méd., Jan., Feb., March, April, 1859.)

In our last number (p. 530) we gave a brief summary of Dr. Duchenne's first essay on the disease to which he has given the above name. He continues the subject in detail in the four first numbers of the Archives of the present year. Those who have read our short abstract may have felt that the disease described resembled in its symptoms what has been described as tabes dorsalis; Dr. Duchenne adverts to the circumstance, but observes that one cannot decide whether in tabes dorsalis the loss of balancing power depended upon a loss of sensibility or a lesion of the psychical faculty which controls movement; besides, muscular weakness has been demonstrated in these patients, a circumstance that does not accompany the ataxie locomotrice, in which the muscular power remains intact.

Dr. Duchenne reserves his remarks about treatment for a future time; but as the following case is the only one in which he has yet had the opportunity of instituting a post-mortem examination, we give it in full:

M. Demay, a painter, aged twenty-eight years, came to consult Dr. Duchenne in May, 1858. He then ascertained him to be affected with ataxie locomotrice in the third stage—viz., double but incomplete paralysis of the sixth pair; characteristic boring and flying pains, recurring especially at night; integrity of the muscular force, contrasting with the complete loss of co-ordination of the lower limbs, which rendered walking impossible, even when assisted by the eyesight; formication and numbness of the two last fingers of each hand, of only a few months' date; sensibility in the feet and legs much diminished; electro-muscular contractility intact. The ataxie locomotrice dated back two years, and had followed the usual course. The apparent cause was a syphilitic disorder contracted in 1849, which had been treated with the protiodide of mercury, corrosive sublimate baths, &c. Dr. Duchenne advised him to go into the Charité, where he died in 1858, of an intercurrent affection. At the autopsy, the brain and spinal cord were examined with the greatest care, but presented no appreciable lesion whatever.

The author establishes three stages. During the first, the diagnosis is doubtful; spontaneous strabismus or diplopia is a common symptom of incipient ataxie, especially when associated with amaurosis; the blindness sometimes supervenes before the co-ordination of movement is disturbed to any great extent. Flying, circumscribed, boring pains, attacking all parts of the body, are characteristic; they sometimes precede the actual disease for several years. In the second and third stages, which the author does not clearly distinguish, the disturbance in the co-ordination of the movements makes its appearance; and, if following upon the indications belonging to the first stage, leaves no doubt as to its nature. The disease is generally of long duration.

VI. *Case of Rupture of the Œsophagus.* By Dr. JOS. MEYER. (Preuss. Ver. Zeitung, N.F. i., 39–41, 1858; and Schmidt's Jahrbücher, Jahrg. 1859, No. 2.)

The following rare case occurred in Professor Shönlein's clinical wards. A shoemaker, aged thirty-eight, habitually intemperate, robust, suffered from occasional dysphagia in swallowing solids, brought on in childhood by the application of caustic alkali. The attacks gradually increased in frequency, and the last one occurred in February, 1858, when the patient was swallowing a piece of sausage. Violent attempts at vomiting failed to throw it up; a considerable quantity of blood was ejected; great anxiety and dyspnœa, and pain in the epigastrium, followed. An hour after the occurrence the right side of the face became tumefied. A surgeon administered several emetics, and introduced a probang without effect. The symptoms became more urgent, and on the following day he was admitted into the Charité. He was first seen sitting, bent forwards, with a pale, rather cyanotic complexion, cutaneous emphysema of the face, neck, and anterior half of the thorax. The auscultation of the heart and lungs was everywhere normal, except impaired vocal resonance at the posterior base; the pulse 142, small; respirations 40. There was severe pain extending from the xiphoid cartilage to the vertebræ, which was increased by the erect posture. A rupture of the œsophagus, with moderate pleuritic exudation at the right base, was diagnosed. In the course of the night all the symptoms increased; the emphysema spread over both arms; liquids could be swallowed, but only in small quantities, on account of the dyspnœa. Death ensued fifty hours after the commencement of the illness. The autopsy showed the œsophagus to be healthy, except a patulous ulcerated surface, one and a quarter by three-eighths of an inch in dimension, on the anterior walls of the œsophagus, three inches above the cardiac orifice of the stomach. The ulcer had perforated all the coats; the edges were sharply defined, and the surrounding parts healthy. Just above the cardiac orifice there was some narrowing, the muscular tissue being hypertrophied, but without cicatricial tissue. In front of the perforation there was a large accumulation of foul pus with necrosed tissue and the remains of food. To the right there was a less extensive purulent infiltration, mixed with gas, and from here there proceeded an extensive emphysematous distension of the mediastinum. The pleural cavities contained much discoloured fetid exudation, the pleuræ were invested with thick, fibrino-purulent masses; there were no adhesions to the healthy lungs, nor any lacerations of the latter. There could be no doubt that the perforation was quite recent, both from the appearance of the ulcer and the absence of thickened walls of an abscess. Dr. Meyer has only been able to find two analogous cases; one recorded by Boerhaave,[*] the other by Dryden,[†] in both of which the symptoms were analogous to those recorded.

VII. *Communications regarding some Cases of Laryngeal Disease, examined by means of the Laryngeal Speculum.* By Dr. LUDWIG TUERCK. (Zeitschrift der Gesellsch. der Aerzte zu Wien, 1859, No. 11.)

We have on former occasions[‡] directed attention to the practical utility of the laryngeal speculum in the diagnosis and treatment of diseases of the larynx. The following are a few brief memoranda illustrative of the uses of the instrument, taken from the details given by Dr. Türk in the above memoir:

1. A girl, aged thirteen, after recovery from lupus of the upper lip and left cheek, was attacked with temporary aphonia and renewal of the labial ulceration. The free edge of the epiglottis was seen to be much thickened, its mucous membrane much tumefied, and of a pink colour; at the anterior edge there was a loss of substance; the mucous covering of the arytenoid cartilage was much swollen, and the true chordæ vocales showed a white investment at their free edges.

2. A man, aged thirty-three, suffering from hoarseness and occasional aphonia. The chordæ vocales remained separated at their middle part, and did not show the tremulous movement seen in the healthy subject, when he pronounced A (ah); in coughing, the artenoid cartilages approached one another, and closed the false chordæ vocales, a condition which the author has often observed in aphonia dependent upon laryngeal catarrh. The autopsy revealed diptheritic (?) ulcers on the posterior surface of the trachea, on the parts of the larynx subjacent to the glottis, and between the arytenoid cartilages.

3. A pedlar, suffering from syphilitic aphonia, recovered from all his affections but the aphonia. The speculum showed no loss of substance in the epiglottis; the region of the left arytenoid cartilage was somewhat flattened. The true chordæ vocales did not join properly, especially at the posterior angle, when A was pronounced, nor when the patient coughed. On deep inspiration the cords did not separate properly; the true and false chordæ could only be distinguished on the right side.

4. A female, aged forty-seven, had been affected with complete aphonia for eight months. Frequent examination of the larynx never exhibited any catarrhal affection; in whispering A the chordæ vocales closed only at the anterior and posterior angle, and were wide apart in the remainder of their extent, and did not exhibit the normal vibrations; in making a more powerful effort at pronouncing the A, the space between the chordæ increased to from one to one and a half lines. In doing this the corpuscula Santorini at the apices of the arytenoid cartilages crossed one another, so that the right one came to lie in front of and below the other.

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[*] Atrocis nec descripti prius morbi historia. Lugd. Batav., 1724.

[†] Medical Comment, of Edinb., vol. iii. 1788.

[‡] See British and Foreign Medico-Chirurgical Review, Jan. 1856, and Jan. 1859.

5. An artificial flowermaker, aged thirty, had suffered for many years from aphonia. At the anterior edge of the glottis the two chordæ vocales were of normal appearance; behind them there was on both sides a pale red excrescence, nearly two lines long; a narrow third excrescence was occasionally projected between them by coughing; another growth of a similar kind was attached to the left cord as well as to the right, and the two last were considerably separated when the attempt was made to pronounce A.

6. A case of œdema glottidis in a young man, following typhus, giving rise to great dyspnœ, exhibited a perfectly smooth surface in the place of the upper and lower vocal chords, and at the inner sides of the arytenoid cartilages were two transparent bladders, which only left a linear slit between them.

7. A female, aged thirty-eight, suffering for three months from hoarseness and dyspnœa, exhibited a translucent tumefaction almost covering the right chord as well as the ventricle; the inner surface of the right ary-epiglottic fold was also prominent; the same parts on the left side were normal.

For further particulars we refer to the original. The above may suffice to prove the diagnostic value of the speculum laryngis.

VIII. *On Contraction of the Aorta on a Level with the Ductus Botalli.* By E. Leudet. (Gaz. de Paris, No. 4, 1858; and Schmidt's Jahrb., 1859, No. 3.)

After a review of the history of this rare malformation, the author relates the following case, in which a very unusual complication increased the difficulty of the diagnosis. F. G——, aged thirty-seven, female, had been regularly menstruated till eighteen months before coming under observation, when the catamenia ceased suddenly; dyspnœa supervened, especially upon going up stairs, and also at night. A slight œdema of the inferior extremities disappeared under the use of purgatives. After twelve months the symptoms became more urgent; severe pains in the upper sternum and middle of left scapula supervened. A few months ago dysphagia and aphonia occurred; there was no pain in the course of the respiratory passages. The dyspnœa had increased, but there was no palpitation; there was no cough, little appetite, rarely vomiting, no diarrhœa, but such a loss of strength that the patient was obliged to intermit all work. There was oppressive headache on the left side of the forehead, there was no vertigo or pulsating headache. On admission there was no enlargement of the superficial veins, but on the middle and anterior surface of the thorax there was an enlargement of small arteries, as well as in the left intra-spinal fossa; numerous arterial branches were found at the posterior margin of the left armpit. The normal impulse of the heart was felt in the fifth intercostal space, a little to the left of the nipple; there was no purring tremor; the jugulars and carotids were normal, both radial arteries beat exactly alike; there was a slight systolic blowing murmur at the base of the heart on a level with the aortic valves, which did not extend to the left margin of the heart; it was increased along the course of the aorta, and attained its maximum on a level with the second rib. There was no impulse or enlargement. The abnormal bruits were not heard over the abdominal aorta. The beat of both femoral arteries was weak, but equal. There was marked dulness over the left lung, with diminished respiratory murmur and bronchophony, but no râles. Right lung normal. Urine pale, not albuminous. From the 25th of July to the 3rd of August no change occurred; rigors, sweats, and cough then set in; the emaciation increased, and the dyspnœa became more urgent. The dulness of the left lung ascended up to the left intra-spinal fossa, and the respiratory murmur ceased posteriorly; the dulness anteriorly mounted up to the left clavicle, respiration being almost inaudible. Sudden severe hæmoptysis on the 16th of August proved fatal. Autopsy: larynx, trachea, and bronchi full of fluid blood. The left bronchus was adherent to an aneurismatic sac of the descending aorta, with which it communicated by two openings. The left pleura contained about three pints of a serous, slightly opaque liquid. The whole left lung was in a scirrhous condition, and when cut showed a large number of small lobular masses, from which blood and pus exuded. There were traces of old pericarditis on the visceral layer of the pericardium. The heart somewhat enlarged; pulmonary artery and its branches normal; mitral and aortic valves healthy; the endocardium of the left ventricle somewhat thickened; the walls slightly hypertrophied. The ascending aorta was perfectly normal; the innominata somewhat enlarged, as well as the left carotid. The left subclavian was much dilated; the coats unaffected; immediately below the origin of left subclavian the aorta was constricted in a funnel-shaped form, so as to allow the passage of a common probe, but not of a female catheter; the coats contained no deposit, but were much hypertrophied, especially the middle and external coats. Below the constriction the aorta was enlarged aneurismatically, and the coats entirely degenerated. Anteriorly, and communicating with the left bronchus,

lay a sacculated aneurism, the walls of which were incrusted with salts of lime. The descending aorta was in direct communication with the aneurism, but presented no peculiarity. The internal mammary arteries were dilated, and anastomosed with dilated branches of the epigastric artery. The lumbar arteries and posterior scapular arteries were double their usual size.

IX. *On Thrombosis of the Ductus Botalli (Ductus Arteriosus); in Communications from the Pathological Institution of Professor Rokitansky.* By DR. KLOB. (Zeitschr. der k. k. Gesellsch. der Aerzte, 1859, No. 1.)

The closure of the ductus botalli takes place between the fourth and eighth day of life, by the formation of the inner coat of areolar tissue, with considerable nuclear growth, and the consequent contraction of these tissues. In the following case the involution appears to have been prevented by the formation of a thrombus in the artery, or by some previous morbid process which induced the latter.

A female infant, aged eight days, died in the Lying-in Clinical Wards; she was well developed, slightly icteric; brain normal, except some yellow-coloured serum in the ventricles; thyroid gland small, tracheal mucous membrane pale; thymus gland rather small, both lungs slightly œdematous and containing a frothy yellow serum. The pericardium contained about three drachms of clear pale yellow liquid; the heart of normal size and form, contracted; the tissue pale brown; the cavities and large vessels containing loosely coagulated blood; the endocardium and inner coats of the vessels were stained with blood. The ductus botalli was uniformly three lines in diameter. The orifice in the pulmonary artery was blocked up by a soft plug, dark red at its commencement, further on pale yellow, closely adhering to the coats of the vessel. At the aortic orifice the plug was abruptly broken off. There was no trace of contraction at the aortic orifice. The liver was dark red, the gall bladder contained brown bile. In the mesentery there were branched extravasations following the course of the arterial branches, and reaching as far as the attachment of the omentum to the intestine. The superior mesenteric artery was found blocked up in its smallest divisions by minute particles of coagulated fibrin; from these points a thrombus had formed, extending towards the trunk; in some parts the internal and middle coat had got detached so as to resemble a dissecting aneurism.

Dr. Klob states that he has examined many infants of the same age, but has failed to discover the ductus botalli closed in the same way. He is of opinion that the thrombus was not the cause of the persistence of the duct, but that it was caused by a species of inflammatory process in its coats, which caused the contained blood to coagulate. The author is further of opinion that the blocking up of the mesenteric artery was caused by the breaking off of a portion of the plug in the duct at the aortic termination, which was carried along by the current of blood until arrested in the divisions of the mesenteric.

X. *Case of Acute Miliary Tuberculosis running its Course in Thirty Hours.* By C. A. WUNDERLICH. (Archiv für Physiologische Heilkunde, Jahrg. 1859, Heft 2.

The learned professor relates this case as an instance of what he regards as a very acute case of miliary tubercle, its commencement and termination occurring in thirty hours, though he admits that it may also be interpreted as a case of acute tuberculosis which remained latent until shortly before death.

A type-founder, aged twenty, living in easy circumstances, had almost invariably enjoyed good health, never having shown symptoms of lead poisoning. At Christmas he had been poorly for a few days, and on the 6th of February of the present year he was also slightly indisposed, but was perfectly well afterwards. On the 11th of February he went to work as usual, and ate his dinner with a good appetite. After dinner he vomited once, but was able to do his work. At five P.M. he complained of vertigo, and on going to the water-closet became comatose. He was brought to the Leipzig Hospital in this state, was undressed with difficulty, and when put to bed lay on his right side doubled up; opened his eyes at times when spoken to, but did not answer questions. His face had the usual colour; the pupils acted well; there was a moderate blue line round the teeth. The front surface of the chest could not be examined. There was no dulness posteriorly. Respiration was vesicular throughout, except at the left scapula, where it was slightly bronchial. Respirations, twenty; pulse, ninety-two; heart-sounds normal. Nothing abnormal about the abdomen. General nutrition good. Insensibility persistent. Constant jactitations increased every half-hour paroxysmally, but without spasms or screaming. On the following morning the face was purplish, and the patient swallowed nothing. After a warm bath, with cold applications to the head at mid-day, he was attacked with universal convulsions, alternating with tetanic extension; the face became cyanotic; there was froth at the mouth. After an hour and a half's interval the attack was renewed; then after half an hour, and subsequently they returned every five minutes. The pupils now became contracted; coarse and fine mucous râles were audible over the lungs; respiration became irregular; the tracheal rattle supervened, and the convulsive paroxysms increasing in frequency; he succumbed to one at one A.M. the next night. *Post-mortem:* Cranium and dura mater normal; the meninges normal throughout, except that at the left posterior lobe there were a few small opaque spots, with two distinctly projecting, translucent, greyish, miliary tubercles. The left posterior lobe of the cerebrum was softer than the remainder of brain, but without extravasation or congestion; ventricles not enlarged, without serum; the septum pellucidum and fornix softened; the remainder of the brain healthy. Both pleura adherent The right apex showed cicatricial contractions, and besides small, almost cylindrical, bronchial dilatations and old tubercular granulations, a large number of fresh, roe-like, prominent miliary tubercles. In some parts these were densely aggregated, but there were large interspaces between the groups; these also extended into the middle and inferior lobe. In the left lobe the deposit was confined to the upper lobe. Heart, spleen, liver, peritoneum, and kidneys, were normal; in the lowest part of the small intestine the solitary glands and Peyer's patches were somewhat enlarged; in the stomach were some hæmorrhagic erosions.

XI. *On the Occurrence of a Blowing Sound in the Pulmonary Artery, associated with Affections of the Lung; on the Sounds of the Artery in Health; and on the Effect on them and on the Heart of the Act of Inspiration.* By J. Da Costa, M.D. (The American Journal of the Medical Sciences, Jan. 1859.)

The author brings forward eight cases in which he detected a distinct blowing murmur in the place of the normal first pulmonary sound, at the second left costal cartilage, or in the second left intercostal space close to the sternum. The murmur was limited to this spot, and did not pass upwards or downwards. In the first case, a man, aged thirty-five, there was confirmed phthisis; besides cough and yellow expectoration, the upper right lung was dull on percussion, with crackling and prolonged expiratory murmur; on the left side a slight dullness existed anteriorly at the upper portion, and the respiration was harsher than normal. While under treatment, the author repeatedly noticed a blowing sound, at times low, at times of a higher and more whistling pitch, in the interspace between the second and third ribs on the left side; it was synchronous with the heart's impulse, and was followed by a distinct second sound. The other cardiac sounds were healthy, and no arterial or venous murmurs were noticed. The murmur was not always present in this case, especially when the heart's action was slow and the breathing calm. Case 2, a lad, aged eighteen, also phthisical, had slight dulness at upper right lung, with cracking and prolonged expiration; a friction-sound at upper left lung anteriorly near the second rib: dry râles were heard over the entire left lung, and part of the right. Later on in the case, the phthisical symptoms continuing, a rather short blowing sound, at times of a high whistling pitch, then again of a lower note, was heard in the second intercostal space, and synchronous with the heart's impulse. The heart was otherwise normal. The sound was best heard when the patient held his breath after expiration. The next four cases are precisely similar to those given; one of them, however, proved fatal; the blowing sound was distinctly heard a few weeks before death. The autopsy showed extensive disease of the left lung. It was throughout the seat of tubercular deposit, and contained cavities. The heart was not enlarged, the valves were healthy. No abnormal state of the pericardium, nor of the vessels proceeding from it, was noted. The exact position of the pulmonary artery to the deposit was unfortunately not specially studied. In the two remaining cases the author states that there were no phthisical symptoms; but in one, the upper left lung was duler than the upper right; the inspiratory murmur throughout the left lung, but especially at the upper portion, harsh, and expiration there prolonged. In the other there was relative dulness, with a higher pitch at the upper portion of the left lung, especially between the second and third ribs, where there was also more resistatnce and a slight sinking; respiration was harsher and stronger than normal at the left apex. In both these cases the cardiac sounds were normal, with the exception of the pulmonary systolic bruit.

Dr. Da Costa in the commentary dwells upon the anatomy of the parts, and shows that the sound could not have originated in any other part of the circulating system than the pulmonary artery. He discards the view of the murmur being anæmic from the absence of other signs of anæmia. The rarity of lesions of the pulmonary valves he regards as a proof against the murmur being explicable upon this view. He considers that local changes or obstruction in the pulmonary artery gave rise to the murmur, and concludes that it was produced by pressure upon the artery.

1. Because deposits in the coats of the pulmonary artery are uncommonly rare.
2. Because, the cases having occurred mostly in young persons, the age of the patients excludes deposits.
3. Because the murmur was usually soft, but became sometimes of a more whistling character.

The author holds that anything which tends to fix the pulmonary artery or impress it, as infiltration of the surrounding textures, pleuritic adhesions of the upper lobe, and the like, may induce the murmur.

He would have satisfied himself still more completely that the cause of the bruit does not in these cases reside in the artery, but is altogether external to it, had he tried the effect of pressure with the stethoscope, or the changes in the sound on change of position; thus, with regard to the former, external pressure in our experience, much intensifies the sound, while the erect position diminishes it. It has also appeared to us that enlarged bronchial glands might at times encroach upon the calibre of the pulmonary artery, and thus induce a murmur such as that described in Dr. Da Costa's interesting memoir.

XII. *A Case of Hæmoptysis, Entrance of Air into the Veins and Discharge of Air by a Venesection.* By M. Piedagnel, Physician to the Hôtel-Dieu. (L'Union Médicale, 1859, No. 45.)

A gentleman, aged forty-two, of vigorous constitution and strong muscular development, had been affected for four years with a spinal disease, which, however, disappeared under treatment. For two months before coming under treatment he had influenza, with much cough, and occasional violent efforts at expectoration. On the 18th of February, 1858, while coughing, he suddenly fell down insensible, and discharged a considerable quantity of blood. The hæmorrhage ceased, but consciousness did not return. When seen by M. Piédagnel he was lying on his back, perfectly insensible, face pale, eyes immovable, pupils the same, but distended; hearing gone; no movement or sensibility. The whole skin pale and insensible, to stimulants. Respiration noisy, but does not resemble that of cerebral congestion, being active in inspiration, and at the end of expiration as in very feeble children. There was a slight râle on the right, but a strong and very moist one to the left; the percussion was less clear on the left, but no dulness either before or behind. Percussion of the cardiac region only causes a doubtful dulness; on auscultation a dull but tumultuous sound of the heart-beats was heard. The radial arteries were imperceptible; all the subcutaneous veins were empty. The diagnosis was doubtful; it could not be apoplexy; it might be laceration of the lungs or rupture of the heart.

A variety of stimulants were applied; after about half an hour there were symptoms of returning animation; the cutaneous circulation reappeared. A venesection being proposed by M. Vivier, was performed on the median-basilic vein. A little blood dribbled out; to the great surprise of the bystanders bubbles of air were then seen to issue from the opening in the vein, at first one, then several, passing out so as to form a sort of wreath on the skin, between the opening in the vein and the lower part of the fore-arm. On the blood and the air ceasing to flow, some light frictions along the course of the vein caused a new issue of air-bubbles; two, four, eight issued successively, then the flow stopped; the frictions were repeated several times; all precautions were taken to avoid error, and each time the same result ensued. At last blood and air ceased to appear; the patient did not improve, and death took place soon after. No autopsy was allowed. But the physicians were of opinion that a rupture of the lung had taken place, causing an entrance of air into the bloodvessels.

XIII. *On Puncture of Hydatid Cysts of the Liver with the Capillary Trocar.* By Dr. J. Moissenet, Physician to the Lariboisière Hospital. (Archives Générales, Fevrier, Mars, Avril, 1859.)

Having had the misfortune to lose a patient affected with a considerable hydatid cyst of the liver by peritonitis, resulting from a palliative puncture with the capillary trocar, the author enters upon a minute inquiry relative to the different methods which have been employed for the purpose of evacuating the liquid contents of the tumour and the subsequent destruction of the hydatids. He finds that experience justifies the simple puncture, provided there is no escape of the fluid contents into the peritoneal cavity. Récamier, Legroux, and Laugier, Owen Rees, Aran, Boinet, Robert, Cloquet, and others, have obtained successful results by puncture with a fine trocar. Cruveilheir, in speaking of Récamier's practice, warns against its general employment unless adhesions can be proved to exist, and the tumour presents a decided tendency to push outwards. Dr. Moissenet brings forward several other cases besides his own which proved fatal. The first series of general conclusions that his analysis brings him to are:

1. That the hydatid liquid, whether limpid or puriform, when poured into the peritoneum, whether as the result of accident or of an operation, induces acute or chronic inflammation, which is almost always, if not invariably, fatal.
2. That capillary puncture, though commonly not injurious, may induce effusion into the peritoneum of hydatid fluid, when there are no adhesions between the cystic and abdominal parietes; and that this effusion has taken place when the puncture has been made for exploration or palliation only; that is, when the cyst has been imperfectly emptied.
3. That the puncture of hydatid cysts, whether made with a capillary or an ordinary-sized trocar, may prove fatal by inducing inflammation of the cyst itself.

The second series of conclusions drawn by Dr. Moissenet are:

1. That capillary puncture of an hydatid tumour, made even without the existence of adhesions, may be curative, when followed by as complete an evacuation of the liquid as possible.
2. That this result may be obtained by a single puncture, or by two or three successive punctures.
3. That the treatment commenced by capillary puncture must sometimes be completed by another method, as in the case of Dr. Owen Rees,* in which a larger trocar was used at the third puncture, and a gum-elastic sound left in the orifice.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Foreign Bodies in the Urethra and Bladder.* By Professor Pitha. (Wein. Medicin. Wochenschr., 1858, Nos. 50, 51, 52.)

The immediate cause of this communication was an interesting case, in which, on account of the introduction of foreign bodies into the bladder, the operations of lithotomy and lithotrity were successively performed on the same individual. He was a soldier, aged thirty-four, who came into the hospital in consequence of a piece of lead-pencil about three inches long, and pointed at either end, having slipped into the bladder while he was trying to pass it, as a substitute for a bougie. He had suffered excessive torment for a week, and the pencil (which was found divided into two parts) was removed by the lateral incision. He did very well. In two years he returned to the Clinic suffering from intense cystitis, brought on from the presence of a piece of sealing-wax which had entered the bladder during his manipulation with it. The patient protested against the repetition of lithotomy, which, indeed, would have hardly been advisable in the inflamed condition of the bladder, and it was resolved to have recourse to lithotripsy. He was brought under the influence of chloroform with great difficulty, and never to an extent sufficient to subdue the irritability of the bladder, which instantly rejected the smallest quantities of water which were thrown into it. The preliminary injection of the bladder had therefore to be dispensed with. The foreign body was seized with the greatest ease, and not to pursue the details, was crushed and entirely removed in the course of three *séances*, at intervals of two or three days. The patient completely and rapidly recovered. This case is very interesting, from the fact of there being no means of anticipating the amount of resistance which wax that had lain in the bladder for four weeks would offer; and further, by showing that lithotripsy may be safely performed, notwithstanding violent general reaction and the greatest irritation of the bladder, producing complete intolerance of the presence of any water whatever. Hitherto one of the most received axioms has been not to undertake the operation in an empty bladder. It may surprise some that the narcosis was not pushed to the extent of appeasing this irritability of the bladder, but repeated experience has shown that the highest doses of anæsthetic agents will not effect this.

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* Guy's Hospital Reports, vol. vi. Oct. 1848.

The case is also interesting as exhibiting an example of the unexpected slipping of foreign bodies into the bladder having occurred twice in the same individual. This accident has not excited the attention its frequency and importance demands. If experience upon this point were collected, surprise would be excited at the extraordinary character of various objects found in the urethra and bladder,* of some of which it would be difficult to explain how they could be forced through the urethra, to say nothing of their slipping into the bladder. But even with respect to the commoner objects, most of which have some resemblance in shape or size to the catheter, such as pen-holders, pencils, glass tubes, metallic rods, pieces of wood, &c., one can scarcely conceive how, once introduced into the urethra, they should escape from the fingers and slip into the bladder. This may be intelligible enough as regards the straight, short, female urethra, but not so as to the male urethra, whose long curved canal is traversed with difficulty by a well-oiled catheter in inexpert hands. Some have sought for an explanation in a suction-action or a peristaltic movement of the urethra, but ample experience in introducing instruments does not favour this view. On the contrary, a powerful expelling influence is often exerted by the urethra, the instrument being forcibly ejected when it has reached the neck of the bladder, this resistance being in fact met with more or less in the widest urethræ. The walls of the canal are naturally closely applied to each other, and its entire mechanism is directed so as to favour the passage from within outwards, and not the reverse of this. Demarquay has furnished this natural explanation, that the entrance of the body takes place at the period of erection, and especially of ejaculation. The urethra is then elongated, its walls are expanded and smooth, and its canal is gaping and well lubricated, while its curvatures are diminished. The penis in relaxing forces the body deeper inwards, and the efforts of the person to prevent the occurrence by forcing back the penis, in order to shorten it, only add to the mischief. This seems the most rational explanation, for in fact these cases usually occur in onanists, who in order to induce ejaculation, penetrate deeper and deeper into the urethra, having already blunted the sensibility of the anterior portion. In the performance of catheterism, too, we sometimes find, in spite of the greatest skill, the passage of the instrument is obstructed. Presently, the repeated attempts induce erection, and the instrument at once passes on. Sometimes the catheter glides at once to the membranous portion, where it meets with insurmountable resistance. Believing that there must be a fold or a stricture, or that a faulty direction has been observed, we seek to withdraw the instrument. This, however, is impossible without violence, so fast and immovably is it held; and now a phenomenon which astonishes the inexperienced is observed—viz., the spontaneous deeper penetration of the instrument, as if propelled by some unseen power, a circumstance which has probably given rise to the suction theory. It is, however, a mere mechanical effect of the elastic return of the penis (after having been forcibly drawn forwards), during which the muscular walls, closely embracing the instrument, are also carried backwards. So strong may this spasmodic action of the muscles be, that nothing but rude force will overcome it, unless we wait for minutes or hours, until the spasm subsides. It may play the chief part where foreign bodies slip into the bladder during erection, at all events detaining them in the membranous portion of the urethra; and it will be the more certainly brought into action the more pointed and irritating the body be, especially as the state of erection increases the irritability of the urethra. It is well known that catheterism after coitus or pollution is much more difficult, and that in such case a seizure of the instrument most readily takes place, even in patients accustomed to the operation—a fact of some importance in the treatment of stricture.

For the removal of foreign bodies of a roundish shape from the urethra, such as beads, beans, calculi, &c., Professor Pitha has long been in the habit of using delicate long-bladed forceps (*Kornzange*), which can be easily manœuvred with one hand, while the fingers of the other fix the foreign body from behind. Several such forceps of different lengths and widths should be at hand, in order to choose from in particular cases. When the body is seated very deeply, as at the neck of the bladder, the instrument should have a gentle catheter-like curvature given to it; and such an instrument the author finds admirably adapted for the removal of fragments from the urethra after lithotrity. When the foreign bodies are too voluminous, they should be first broken up by means of Segalas' urethral *brise-pierre*, and when not capable of being broken, they must be removed by the button-hole operation, the wound in these cases always readily healing, even when situated in the perineum. In the case of sharp, pointed bodies, such as needles, awls, and the like, special manipulation is required. When a needle, e.g., is still in the region of the penis, we must ascertain by touch its exact position, and then by pressure on the blunt end, force the point through the walls of the urethra, until it can be seized by a forceps. Pins should be thrust through in the same manner, and so manœuvred that the head is directed towards the mouth of the urethra, and then removed by an urethral forceps. When the needle is implanted lower down in the urethra, we must act in the same manner with the fingers placed in the rectum. In bodies having yielding stems and blunt points, such as hair-pins, these procedures are impossible, and for the removal of such, an instrument contrived by Matthieu, of Paris, answers admirably. Leroy has modified the instrument, so that it now resembles the *brise-pierre à pignon.* Smooth, cylindrical bodies, such as needle-cases, pen-holders, bougies, &c., soon pass into the bladder. Before this they may usually be removed with ease, providing their progress backwards be at once prevented by compression. Even if the body has reached the membranous portion of the urethra, it can be removed by pressing it forwards through the rectum, while the curve of the urethra is diminished by traction of the penis. The cases wherein an instrument breaks in the urethra may be easily managed in this way with patience and presence of mind.

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* See a paper by M. Denucé: Brit. and For. Med.-Chir. Rev., vol. xx.

When the foreign bodies have reached the bladder, they are extracted with greater difficulty the longer and thinner they are. After they have remained in the bladder, too, they become incrusted, and are more difficult of removal, especially when the bladder has been rendered irritable. It is a matter of great difficulty to seize them in the longitudinal direction, but the extractor of Luer is admirably adapted for regulating the direction in which the body shall be removed, and with its aid, our difficulties are reduced in these cases to the discovery of the foreign body.

II. *On the Treatment of Hernia by Electricity.* By Dr. Clemens. (Deutsche Klinik, 1858, No. 34.)

In the first of a series of articles upon applied electricity, the fruits of ten years' investigation, Dr. Clemens gives an account of his employment of it in the treatment of hernia. He commenced its use in 1850, by endeavouring to produce a diminution in the size of the hernial apertures in a case of large double inguinal hernia. One of the poles of the battery, a massive metallic knob, was introduced deep into the canal, pressing a flap of skin inwards, and a moderately strong galvanic stream, increased daily, was passed along this during six minutes. At the end of a week the herniæ protruded less easily, and the apertures had become narrower. When the examination was made before and after each *séance*, a great difference was always found in the accessibility and size of the sac—an observation since repeated hundreds of times. The application of the galvanism also has the good effect of increasing the peristaltic movements, which in becoming more energetic effect a favourable change in the position of the intestines, by altering the situation of the portion which had so long remained opposite the aperture, and had consequently become relaxed. A more complete evacuation of the contents of the canal is also brought about. The impaired vitality of the intestine, of the hernial canal, and of the abdominal coverings is always renovated through this application of electricity. When the hernia is recent, no means of treatment is so certain and so exempt from all danger; and even when the hernia long has protruded it has often been returned under the influence of the galvanic stream or the electrical flask. Dr. Clemens has usually preferred friction-electricity to galvanism, as its operation is more rapid and its effects are more energetic. Among twenty-seven patients so treated, none have complained of the least unpleasantness; but, on the contrary, they have found many inconveniences disappear under its influence, and especially obstinate constipation. In very sensitive persons diarrhœa may follow an energetic *séance.* When a hernia has been recently produced, as by a fall, lifting, &c., the success of the method is often surprisingly rapid, and in marked contrast with the slow progress of treatment by trusses, &c. A double hernia thus produced was cured without any bandage in twenty *séances*, and has remained so now for two years—the treatment only commencing a week after the accident.

Dr. Clemens states that for large hernia, which can only be kept up imperfectly by any ordinary truss, he has contrived a galvanic truss, which operates with remarkable efficacy. It is constructed of copper and zinc plates, or pieces of copper and silver money, having felt or leather interposed, which is kept moistened by the saline solution necessary for the excitement of the pile. He again dwells upon the importance of exciting peristaltic action, not only in hernia, wherein it may often prevent strangulation being produced, but in various other affections due to a sluggish movement of the intestine. In numerous experiments upon animals, in which powerful shocks were employed, no ill effects resulted from the increased intestinal action produced.

III. *Isolated Fracture of the Last False Rib.* By M. Legouest. (Gazette des Hôpitaux, 1859, No. 17.)

This rare form of fracture of the ribs occurred to a soldier, aged forty-five, who while playing fell with his side against the corner of a table. Violent pain, an imperious desire to cough, and difficulty of breathing, revealed the nature of the accident. Examination detected the exact seat of the fracture to be at the junction of the middle with the posterior third of the last false rib. Crepitation was plainly felt at this point, but this was at some distance from that at which he had been struck. The corner of the table had forced the moveable end of the rib inwards and backwards, and the fracture had taken place in consequence of the excessive bending thus produced. No bandage was here applied as in ordinary fracture of the ribs. Such a bandage, having in view the limiting thoracic respiration and the rendering it abdominal, is useful in fracture of the true ribs (except the seventh), but is injurious in fracture of the false ribs. In point of fact, in the space comprised between the base of the transverse process of the first lumbar vertebra and the point of the last false rib, the circumference of the diaphragm is attached by an aponeurotic arch, one end of which is fixed to the base of the first lumbar vertebra, and the other to the lower edge of the last false rib; the remainder of the anterior circumference of the muscle rising up to be inserted into the cartilages of the false ribs and the seventh sternal rib. Wherever by inducing abdominal respiration the contractions of the diaphragm were rendered more energetic, the patient's sufferings were only increased; and he was left to his own instincts, which very soon taught him the manner he could breathe with least inconvenience.

IV. *Case of Spontaneous Fracture of the Femur, with Consolidation.* By M. Robert. (Gazette des Hôp., No. 18, 1859.)

A porter, aged fifty, of small stature but robust constitution, and in good health, with the exception of having suffered for about two years from pains of the lower limbs, supposed to be rheumatic, was quietly descending a staircase when he felt a cracking in the right thigh, and fell down; the fall being induced by the fracture and not the reverse. After two months and a half treatment the fracture, situated just below the trochanter, had completely consolidated, there being an abundance of callus, and shortening to at least the extent of four centimetres, giving rise to considerable lameness.

M. Robert, commenting upon the case, observed that, as far as he knew, it was unique. Every one is aware that the bones are sometimes spontaneously fractured when they are the seat of serious disease. In the case related by Dupuytren of a lad who had fractured the humerus while throwing a stone, a hydatid in the medullary canal was found to have reduced the bone to a mere shell. Tubercular deposit in the medullary cavity would be expected to produce the same effect; and cancer induces also the absorption of the osseous tissue. M. Robert has himself met with four examples of fracture of the humerus or femur in this disease. The cause in such cases giving rise to the fracture would likewise prevent its consolidation. In explaining the occurrence of this accident in the present case, M. Robert has recourse to the hypothesis of syphilitic disease, inasmuch as the man about twenty years ago suffered from chancre, but has never since had any symptoms of constitutional syphilis. He states that he has seen other examples of apparent cure, in which the individuals, remaining fifteen or twenty years without any consecutive manifestation, have still, after such a lapse of time, exhibited evident signs of syphilitic cachexia. Syphilis appearing at so late a period, gives rise to indeterminate accidents which cannot be classed among those which are manifested in the secondary and tertiary periods. These ultimate accidents may be almost designated as those of the plus-tertiary period, and they always are observed affecting either the osseous or fibrous tissues.

[The occurrence of fracture from muscular contraction independently of the cancerous diathesis, is not so rare an occurrence as M. Robert seems to suppose. We would refer him to Dr. Van Oven's case, related by himself to the Medico-Chirurgical Society;* to the collection of cases published by Dr. E. Gurlt, in the 'Deutsche Klinik,' Nos. 25–29, 1857; and to cases related by Mr. Henry Smith, in 'Medical Times,' vol. xxxvi. 1857.]

V. *On Calculous Diseases in Hungary.* By Professor Balassa. (Wien. Medicin. Wochenschr., 1858, Nos. 25 and 26.)

This article is the substance of a reply made by Professor Balassa, of Pesth, to a circular asking for statistical information, issued by Professor Gross, of Louisville, U.S. Owing to the absence of rural hospitals in Hungary, Professor Balassa observes, almost all the cases of stone which occur in Hungary are brought to Pesth for treatment; and in his clinic at the hospital, he has treated, in the twelve years 1843–55, 135 cases. The ages of these patients were as follow:—

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* Medical Times, vol. 36, p. 602. 1858.

From	1	to	7 years	21
„	8	„	15 „	32
„	16	„	25 „	47
„	26	„	50 „	27
„	51	„	60 „	6
„	61	„	70 „	2
				135

The employments and conditions of these patients were as follow:—Peasants, 82; artizans, 39; tradespeople, 7; *employés*, 2; landowners, 2; students, 2; teachers, 1.

Thus calculous affections are met with in the Pesth clinic with by far the greatest frequency in the young, inasmuch as out of a total of 135 cases, 100 of the patients had not reached their twenty sixth year; and when it is added that in most of the cases the disease had long existed, its origin must be referred to a still earlier age. Moreover, during the twelve years there were also forty-nine children treated for stone in the Pesth Children's Hospital. So, too, the immense proportionate prevalence of the disease in the peasant (60·74 per cent.) and in the artizan (28·889 per cent.) classes is to be remarked. These classes, indeed, almost exclusively furnish the examples of the disease occurring amongst the young, as the author's private practice has taught him. Dr. Ivanchich, too, in his statistics of 100 cases of stone, comprises 33 natives of Hungary, but only 2 of these are as young as eighteen. The conclusion is, then, that stone prevails especially among the young of the poorer classes; and this leads us to consider some of the influences that are at work in its production. The nature of the diet and mode of life can alone explain these differences. It is the custom in Hungary to feed children when weaned, or even while suckling, upon the same articles of diet as are employed by adults, and while these in the wealthy classes may be nutritious and digestible, among the poor they consist of unwholesome matters, as fruits, vegetables, pork, and bacon. The latter, then, are fed with a diet that is difficult of digestion, and containing by far too large a proportion of the carbonaceous element; and the importance of this statement will be seen when the chemical constitution of the calculi has been considered.

For various reasons, the author has only been able to preserve 83 calculi in his cabinet, but all these have been carefully examined, and he furnishes the details of the results. 63 of the calculi were of homogeneous composition, and in 20 the nuclei and external portions were of different composition. The general result is, that while uric acid was the most frequent constituent, it was not the most frequent chief constituent, for while it was found more or less in 72 calculi, it constituted the chief portion of these only in 23. The oxalate of lime was the chief constituent, for it formed the chief mass of 20 calculi of homogeneous composition, and formed the nuclei in 12 out of the 20 stones of non-homogeneous composition. It is evident, then, that the formation of stone in Hungary is much due to the richness of the urine in the oxalates—i.e., in the carbonaceous combinations furnished by the defective aliment employed. Of the 20 stones having nuclei of different composition to the surrounding parts, in 12 these consisted of oxalates, and in 8 of urates. It is owing to the prevalence of these in the urine that the first impulse to the formation of calculi is given, while their enlargement much depends upon the presence of phosphates. These were present either as a chief or partial constituent in 45 calculi. These considerations lead to the conclusion that the most effectual means of preventing the formation or recurrence of calculi, would be to act upon the oxalic or uric acid formations of the urine.

Of the 135 cases, 13, on account of disease of the urinary organs or the condition of the entire economy, were deemed unsuitable for operation. Some of the 122 operations were performed under very unfavourable circumstances, in consequence of the presence of chronic disease of the urinary organs.

Of the 122 cases operated upon, 92 were submitted to lithotomy, and 30 to lithotrity. Of the former, 11 patients (11·95 per cent.), and of the latter 5 (16·66 per cent.) died. Among the 11 fatal lithotomy cases, there were 5 individuals who suffered from severe chronic disease of the kidney, and 1 who died of typhus during an epidemic. There were, therefore, only 5 cases in which death took place in from the second to the fifth day, from inflammation of the bladder or peritoneum, consequent upon the operation. This reduces the strict mortality from the operation to 5·43 per cent. Among the 5 fatal lithotrity cases, in 1 death was due to phthisis, in 2 to old suppurative nephritis, and in 1 to recent nephritis, this last and one other death being alone directly referrible to the operation, i.e., 6·66 per cent. Rectal fistula and urinary infiltration were never met with in any of the lithotomy cases. The difficulty in extracting the stone was considerable in many cases on account of its size, and in several of these inflammation of the bladder and peritoneum was set up, this proving fatal in 5 instances. Irritation and inflammation of the bladder frequently also followed lithotrity, leading to considerable delay in the repetition of the operation.

With the exception of 7 cases, the stone was always removed entire. The largest calculus measured two inches five lines in diameter, in 5 calculi the diameter was above two inches, and in 24 between one and one and a half inch The heaviest, removed from a boy ten years old, weighed one ounce and a half and ten grains, and the lightest seventeen grains. In 8 patients there were two stones, and in one three. In 2 children union by the first intention took place, and they left between the eighth and tenth day. The other patients were discharged between the twenty-first and sixtieth days. Relapse occurred in 2 instances after lithotrity, and in 1 after lithotomy.

Of the whole 135 cases only 1 occurred in a female, upon whom lithotrity was performed.

As to the mode of performing lithotomy, Professor Balassa makes an aperture into the bladder with a convex scalpel sufficiently large to admit the index finger of the left hand, and then enlarges it by means of a straight probe-pointed bistoury or Heister's knife. He lays great stress upon the wound being made sufficiently large, and in cases of voluminous calculi frequently makes a bi-lateral incision. After bleeding has been arrested by the injection of iced water (performed while the fingers maintain the wound of the bladder open), some small strips of oiled linen are carried along the index finger to the wound in the bladder (especially when extraction has been difficult), in order to prevent the urine penetrating the swollen edges of the track of the incision. They are removed after two or three days, and the author attributes the non-occurrence of infiltration principally to their employment.

VI. *On Colouring the Lips by Tattooing after Cheiloplasty.* By Professor Schuh. (Wien Medicinische Wochenschrift, 1858, No. 47.)

Two years since Professor Schuh performed cheiloplasty in the Vienna Clinik, upon a girl in whom one-half of the nose, together with the vomer and the whole of both lips, were wanting. The flaps for the lower lip were supplied from the region of the lower jaw and the neck, and that for the nose from the forehead, while the skin of the arm was employed for the upper lip. The connexion of the flap with the arm was divided on the tenth day, and all went on well, excepting that the new upper lip, at its lower edge, owing to the cicatricial process, was covered with corion. The red lip-colour was wanting to give the mouth an agreeable appearance; and Professor Schuh determined to endeavour to imitate this by tattooing. He first of all tried cochineal as a colouring material, but this produced a too pale red, and he then had recourse to cinnabar, which gave rise to a surprisingly natural colour.

The following is the procedure: the cinnabar is made into a thin paste with water, and the limits within which the pigment is to be applied are traced with a pen and ink, in imitation of the direction of the natural redness of the lips. For forcing the pigment into the organic substance, a bundle of sharp-pointed pins is employed, each pin being wound round with waxed silk from its head to within four lines of its point. Ten or twenty such pins are tied into a bundle with thread, dipped into the colouring substance and repeatedly forced two or three lines deep into the lip. The margin marked by the ink is first to be coloured, and then the other portion, dipping the points into the pigment again as this is wiped off. Only a slight bleeding ensues, and the pain is very little, in consequence of the diminished sensibility of transplanted parts. Any of the pigment remaining on the surface should be left there until next day, and if any part is found to be less red than the others, this can be easily remedied.

How long this redness will remain unchanged must be determined by further experience. In Professor Schuh's case it had become nowise paler at the end of a year and a half; and he believes that the introduction of the process of tattooing into the field of plastic surgery is not to be despised.

VII. *On Adhesive Plaster in the Treatment of Fractures of the Lower Extremities.* By Drs. Gilbert and Shrady. (American Journal of Medical Science, April, p. 410; and New York Journal, March, p. 288.)

Dr. Gilbert, as the result of a multiplied experience, now extending over many years, states, that of all the means which have been devised to effect extension and counter-extension in fractures of the lower limbs, the application of adhesive plaster is by far the best. He refers to several cases in proof of this, which he has published in prior volumes of the 'American Journal,' and in the present number gives the particulars of three additional cases.

Dr. Shrady, speaking of the practice of the New York Hospital, gives the following account of the mode of treating fracture of the thigh there:—"When a patient is admitted, there being generally a good deal of swelling present, the limb is placed upon the double inclined plane, until this has subsided. Then he is prepared for the application of the straight apparatus. The first step consists in the preparation of a double band of adhesive plaster, about three inches in breadth, which is cut long enough to extend from below the point of fracture on either side of the limb, forming a loop underneath the foot, a sufficient distance from the sole to allow the introduction of a square block. This block is a little broader than the foot, and serves to prevent the pressure of the adhesive bands over the ankles, and also affords a firm point to which is attached a short cord for extension. The adhesive strap is applied smoothly to the sides of the limb, and a bandage over it, leaving the loop free, extension being in the meantime kept up by an assistant. The body belt and perineal strap are next adjusted, after which the coaptation splints are applied in the usual manner around the seat of fracture. Then the upper end of the long splint is placed in a pocket of the body-belt, the limb drawn down as far as possible, and kept extended by means of a stout cord over the foot-piece; this cord is attached to a ring in a large wooden screw, which plays through the block standing out from the internal surface of the splint. Next, the inside splint, extending from the groin to the malleolus, is applied; pads of blanket being stuffed in on either side, to adapt the splints to the inequalities of the limb. The last step consists in binding the splints together. Three strips of bandage are passed at equal distances from each other behind the limb; and their ends are brought forward between the limb and the splints, carried over the anterior edges backward over the outside of the apparatus, crossed behind, and finally brought forward around the whole again, and tied in front. By this arrangement a sling apparatus is made for the whole limb, at the same time that the splints are nicely secured to each other. The extending force is regulated by means of the screw in the block. This block, I should say, slides in a fenestrum, being secured at any part by a screw arrangement, by which means the proper distance between it and the foot can be regulated, in order to have the full advantage of extension."

The results obtained by this treatment may be judged of from the following statement of 74 cases which involved the shaft of the femur, exclusive of either extremity. These cases were taken in the order of their occurrence. In 19 of the 74 there was no shortening, and in 55 it averaged less than three-quarters of an inch. The ages ranged from three to sixty-four There were 57 patients more than, and 17 less than twelve. Of the 57 there was no shortening in 13 cases; but in the remaining 44, it was a fraction over three-quarters of an inch. Of the 17 under twelve, there was no shortening in 6, and in the remaining 11 it averaged less than half an inch.

VIII *On the Secale Cornutum in Disturbance of the Accommodation-power of the Eyes.* By Professor WILLEBRAND. (Graefe's Arch. für Ophthalmologie, B. 4, Ath. 1, s. 341.)

When local hyperæmia is dependent upon a laxity of the walls of the blood-vessels, advantage attends, Professor Willebrand of Helsingfors, states, the employment of secale cornutum. He was induced to use it in these cases by the expectation that a means which acts so specifically upon the unstriped uterine muscular fibre must excite some power over the analogous structure of the arteries, and in which its hæmostatic action proves, in fact, that it does. During his investigations he soon became struck with the fact that the heart of persons employing it soon underwent contraction in all its dimensions, and that even within the first twenty-four hours—a circumstance which he has frequently verified since. The first case that came under his care was an example of exophthalmos, accompanied with enlargement of the thyroid gland and hypertrophy of the heart. After a few weeks' use of the secale the hypertrophy of the heart and thyroid, as well as the projection of the eye, much diminished. The patient, however, left off the medicine, and the exophthalmos returned worse than ever. Since that period he has employed the secale in various cases in which increasing the contractility of the muscles of the blood-vessels or other tissues seemed to be indicated. It was found of especial advantage in a disturbed state of the accommodation-power of the eye, especially induced by over-taxing the organ on small objects with an insufficient amount of light. Children from some of the schools have furnished the author with many instances, and they have always been relieved by the secale. He relates a case in which impaired vision was always brought on by sewing or reading, and wherein the signs of some amount of chronic congestion were visible. Relief rapidly followed, and when the affection recurred some months after, it was as speedily relieved. He has also found the secale of great use in several cases of acute or chronic inflammation of the eye, and especially in blepharitis and the pustular conjunctivitis of children, the case proving much more rapid, and relapse being much less rare, than when local means alone are relied upon. No benefit has been derived from it in granular conjunctivitis and trachoma.

Proceeding upon the theory of its stimulant action upon the vaso-motoric nerves, the author has extended the employment of the secale to other local disturbances of the economy; and, as already observed, he has had frequent occasions of observing its transitory influence in hypertrophy of the heart, without having any reason to believe that it is of any permanent utility in affections of this organ, the heart always returning to its former size soon after the use of the secale has ceased. In many cases of both chronic and acute hyperæmia it has proved of great service, and especially in cases of galactorrhea, and in indurations, tumefactions, and catarrhal affections of the uterus. Also, it has been very useful in enlarged spleen from intermittent fever, and when large doses of quinine have failed. It is especially indicated in the cases of relapsing intermittent depending upon enlarged spleen. In erysipelatous affections, it has often done good service applied externally as a cataplasm. The author formerly gave ten grains ter die, but now gives but five, combining it with magnesia, or, when chlorosis is present, with iron.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.

Physician to the Royal Maternity Charity, Assistant Obstetric Physician to the London Hospital, &c.

I. The Non-Pregnant State.

1. *Case of Vesico-Vaginal Fistula.* By Henry Thorp, M.D. (Dublin Quarterly Journal of Medical Science, February, 1859.)
2. *Death through an Injection of Carbonic Acid into the Uterus.* By M. Scanzoni. (Beiträge zur Geburtsk. tome iii., 1858; and Arch. Gén. de Méd., March, 1859.)
3. *A Case of Chancre of the Uterus.* By C. Kollock, M. D. (Charleston Med. Journ. and Review, March, 1859.)

1. Dr. Thorp contributes a practical paper on Vesico-Vaginal Fistula. He believes that the shield recommended by Bozeman has no advantage over the quilled suture when properly constructed and applied, and that the latter has the advantage of simplicity. The case he relates is that of a woman delivered of her second child, in March, 1856, after a tedious labour. A month afterwards a slough came away from just behind the neck of the bladder, leaving an opening of an oval form, through which a catheter passed from the urethra into the vagina; it measured an inch from before backwards, and three-quarters of an inch from side to side. The first operation was performed on the 30th of May, 1856. It was unsuccessful, but the opening was narrowed one-half. In August the actual cautery was applied, but the opening remained. In December, 1857, she was again delivered. A second operation was performed in July, 1858. The opening now was as large as at first. The operation was that of Dr. Hayward, of Boston, the object being to obtain extensive raw surfaces for immediate union, by separating the vagina from the base of the bladder to the extent of half an inch around the fistulous perforation—that is, splitting the vesico-vaginal septum into two laminæ, and so adjusting the fresh cut surfaces of the respective flaps, that when turned upon themselves and retained in contact by quilled suture, they shall adhere and unite by the first intention. Dr. Thorp thinks it essential that the needle should penetrate the vesico-vaginal septum, so as to enter about two lines in front, and crossing the area of the fistulous perforation, emerge at the same distance behind the line of division of the membrane into two flaps, and should in its course precisely hit off this angle anteriorly and posteriorly, so that the ligatures when tied shall cause the quills to press together the cut surfaces at their furthest point from the abnormal opening—that is to say, at the divisional line already spoken of.

2. The observation of M. Scanzoni shows the hazard of gaseous injections into the uterus. In this case it had been determined to amputate the neck of the uterus in a woman who was pregnant, the pregnancy being masked by attendant circumstances. The father of the patient, himself a physician, wished to practise for a few days injections of carbonic acid into the cavity of the neck, hoping by this means to produce a contraction of the vessels and to obviate the hæmorrhages which so often complicate amputations of the uterine neck. He tried a first injection with the aid of an elastic reservoir; but scarcely had two or three cubic inches of gas penetrated the gaping mouth of the neck, when the patient cried out that she felt air entering the abdomen, head, and neck. Immediately afterwards she was seized with general teta-

nic convulsions; respiration became laborious and stertorous; the pulse rapid, small; the extremities grew cold, and death followed at the end of an hour and three-quarters. The autopsy revealed nothing but considerable pulmonary œdema. The uterus, much thinned, contained a four months' fœtus; it seemed that the hypertrophy, of which the body of the uterus is the seat in normal pregnancies, had been entirely expended on the neck. The mode in which death was caused is not clear; but the case suggests that extreme reserve should be used in resorting to injections of carbonic acid, whether for the purpose of producing anæsthesia or premature labour. [For the latter purpose it is altogether superfluous, surer and safe means existing.—R. B.]

3. Dr. Kollock's case adds an instance to the rare observations of uterine chancre. It occurred in a woman aged thirty-two. Two well-defined ulcers were observed—one on the parietes of the vagina, about an inch from the vulva; the other occupying the anterior lip of the uterus. They had every characteristic of the Hunterian chancre—greyish colour, excavated, with margins irregular and elevated; the vaginal discharge was puriform. In order to test this character, Dr. Kollock took pus from the chancre on the uterus, and inoculated both thighs. Genuine Hunterian chancres were the result. All were cured by the internal use of bichloride of mercury and the topical use of caustic nitrate of mercury.

II. Pregnancy.

1. *On the Cervix Uteri in Pregnancy.* By J. Matthews Duncan, M.D. (Edinburgh Med. Journ., March and April, 1859.)
2. *On the Normal Hypertrophy of the Heart during Pregnancy, and on its Pathogenic Importance.* By Dr. Larcher. Memoir addressed to the Academy of Medicine, 6th April, 1857. (Arch. Génerales de Médecine, March, 1859.)

1. Dr. Matthews Duncan discusses the prevalent notion as to the progressive shortening of the cervix uteri during pregnancy. With Stoltz and Caseaux, he shows the error of this doctrine. He says the length of the cavity of the cervix uteri undergoes little or no change during pregnancy; the evidence of this is based on the inspection of gravid uteri, and on vaginal examinations. He gives outline sectional views of four cervices of different stages of gestation, one taken from Coste, one from William Hunter, and three from dissection, which establish his proposition. With regard to the evidence from vaginal examination, Dr. Duncan cautions against being misled by the frequent obliteration of the vaginal portion of the cervix, this being only a fictitious, not a real shortening; he advises to measure the length of the cavity by gently intruding the finger through the external os uteri, as can generally be done in advanced pregnancy, in multiparæ, and frequently in primiparæ.

Dr. Duncan further affirms that, 2, the capacity of the cervical cavity becomes gradually greater as pregnancy advances; and this is effected by an increase of its diameter, or breadth, advancing from below upwards—that is, from the external to the internal os of the cervix. 3. The length of the vaginal portion of the cervix, or the amount of its projection into the vaginal cavity, generally diminishes as the uterus rises into the cavity of the abdomen. 4. The softening of the cervix uteri, already commenced superficially during the menstruation preceding conception, continues, and extends more deeply into the substance of the cervix, as pregnancy advances; and the process is generally completed two or three months before the end of utero-gestation. This softening is attended by a considerable increase of bulk. 5. In vaginal examinations during life, after the middle of pregnancy, the finger of the obstetrician feels the uterine cervix as if it were gradually shortened according as pregnancy still further advanced.

[Similar views to the above are advanced by Dr. Arthur Farre, in the article Uterus, 'Cyclopædia of Anatomy and Physiology;' and in the April number of the 'Edinburgh Medical Journal' is another sketch of the cervix of a gravid uterus, taken from a dissection forwarded to Dr. Duncan by Dr. Barnes.—Reporter.]

2. The memoir of Dr. Larcher points out a most important physiological consequence of pregnancy. He affirms that, in the human species the heart is normally hypertrophied during the course of gestation. The proposition is so interesting that it is desirable to cite in detail the evidence upon which it is based. M. Larcher was *interne* to the Maternité at Paris in 1826 and 1827. His observation bore mostly upon women aged from eighteen to thirty-five, and their number was so great as to present in turn every imaginable variety of temperament and organization. Some had been ill a long time before delivery, some for a

short time; but the greater number had preserved perfect health to that event, and had almost all succumbed to puerperal fever. This is to say, that no internal disorder, no lesion, had preceded or excited in them hypertrophy of the heart. Hence the conclusion is rigorous that we were witnessing a physiological condition, spontaneous, necessary—a condition which could derive its explanation only from the new conditions produced by pregnancy. One hundred and thirty observations were made. Taking for standard the relative proportions of the ventricles of the heart laid down by Laennec, it is assumed that the walls of the left ventricle have naturally a little more than double the thickness of those of the right. In pregnancy this is no longer so; the aortic ventricle is manifestly hypertrophied, the thickness of its walls is increased by a fourth at least, by a third at most; the right ventricle and the auricles preserve their normal thickness; the left ventricle only becomes thicker, firmer, and of a deeper red.

In the physiological condition, the heart thus temporarily hypertrophied in pregnant women imparts to the circulating movement a greater energy, which is revealed to auscultation by the *bruit de soufflet*, as is the case in every other hypertrophy of the same order; and it is this greater muscularity of the red-blooded heart that enables it to provide at the same time for two beings.

On the other hand, in the diseased condition this normal hypertrophy becomes a danger, and may determine or augment various functional disturbances.

The observations of M. Larcher were cited by Dr. Ménière, in the 'Archives Générales de Médecine,' in 1828; who, discussing the subject of cerebral hæmorrhage during pregnancy, referred to this normal hypertrophy of the heart as perhaps the cause of the cerebral effusions.

In 1837, Dr. Jacquemier, then *interne* at the Maternité, established the bellows-sound in the precordial region during pregnancy; and M. Larcher declares that he has observed the constant relation between this sound and the hypertrophic condition of the left ventricle, always verified by dissection.

In 1843, Dr. Beau, having regard to his own researches on the sound of the arteries, seeking to verify Dr. Larcher's fact, requested M. Ducrest, then *interne* at the Maison d'Accouchements, to take the measurements of the walls of the heart in a certain number of women who might die after labour. M. Ducrest drew up a statistical return based upon the examination of 100 women, mostly aged from twenty to thirty, who had died in childbed; in all the measure of the walls of the heart was taken at the thickest part of the left ventricle. The maximum of this thickness is 0·018 millimetres in 5 cases; it rises even in one case to 0·022 millimetres; the lowest figure is 0·011 millimetres in 8 cases; in the greater number the thickness is 0·016 millimetres; the mean is 0·016 millimetres. If this mean be compared with that of 0·010 millimetres, given by M. Bizot as representing the normal thickness of the left ventricle in women, we find an excess of 0·005 millimetres. Hence it follows, says M. Beau in 1846, that the observations of M. Larcher are correct.

M. Larcher next insists upon the markworthy *law of coincidence between the hypertrophy of the heart and of the uterus during pregnancy;* and that the plethora of pregnant women is not an accident, but a constant physiological necessity. M. Larcher objects to the view of Andral, which assigns the relative diminution of the red globules as the cause of the bellows-sound in pregnant women. He observes that Andral's researches are based upon examinations of *venous* blood only.

The Pathogenic Importance of the Normal Hypertrophy of the Heart during Pregnancy.—This condition quite coincident with health, when existing within its natural limits, contains a necessary predisposition to sanguineous congestions and hæmorrhages. It may happen that this hypertrophy may gradually disappear after labour, or it may be otherwise. If pregnancies rapidly succeed upon each other, this condition of hypertrophy may be so kept up as to obtain permanence. It is quite conceivable that bronchitis, so common during pregnancy, takes its character of persistence from the hypertrophy of the heart. It is not to the same cause that we must attribute the greater frequency and severity of pneumonia in pregnant women, and as a consequence, the greater frequency amongst those so suffering, of abortions and premature labour. This condition also predisposes to epistaxis, hæmoptysis, various forms of metrorrhagia, and lastly, to cerebral hæmorrhage.

Lastly, the author observes that this normal hypertrophy of the heart does not cease abruptly with gestation, but survives for a time, in relation perhaps to that afflux of blood to the breasts now become necessary for the lacteal secretion.

[The Reporter has long had his attention arrested by clinical facts to the relation between pregnancy, labour, and pneumonia and bronchitis, but thinks it admits of doubt whether the condition of the heart noticed by M. Larcher offers a complete solution of this relation. With reference to the disposition to floodings, it is interesting to remark that Dr. Robert Ferguson, in his comments upon Gooch's works lately published by the New Sydenham Society, expresses his opinion that the peculiar form of hæmorrhage occurring with contracted uterus may be due to cardiac disease. Lastly, this observation of normal hypertrophy of the heart suggests further anatomical researches into the development of the cardiac nerves during pregnancy, more especially since Dr. Robert Lee has sought confirmation of his views as to the enlargement of the uterine nerves from dissection of the heart of mammals.—R. B.]

III. Labour.

1. *Report of a case of Cæsarean Section, with Observations.* By Edward W. Murphy, M.D. (Dublin Quarterly Journal of Med. Science, Feb. 1859.)
2. *Removal of the Placenta in the Early Months of Pregnancy by Evulsion.* By O. C. Gibbs, M.D. (American Medical Monthly, Dec. 1858.)
3. *Statistical Researches on the Action of Ergot in Parturition.* By M. Deville. (Arch. Gén. de Méd., March, 1859.)
4. *A Contribution to the Knowledge of Rupture of the Uterus and Vagina.* By Dr. Lehmann. (Monatschr. f. Geburtsk., Dec. 1858.)

1. Dr. Murphy relates an interesting case of Cæsarean section, and follows it by a valuable discussion on the operation. The case is briefly as follows: On the 11th of July, 1858, he saw, with Dr. Frazer, a woman, aged thirty, who had given birth to seven children at the full time; the last, two years before. The advance of mollities ossium seems to have dated from this labour. When in labour in July, 1858, it was found impossible to introduce two fingers between the pubic rami; by pressing back, however, against the coccyx, Dr. Murphy was able to get two fingers within the brim of the pelvis. In order to do so it was necessary to bend them forwards and upwards, to avoid the strongly projecting promontory. The pubic bones were doubled back so much that the space in the brim seemed hardly two inches in the antero-posterior measurement; on the left side it was contracted to half an inch, and on the right was open to about two and a half inches. Passing the finger round the brim, the space seemed scarcely larger than a florin, through which protruded the os uteri and membranes; the head could just be reached. Some time was given to see what the uterus would do to bring the head within reach of instruments. In consultation with Dr. West, the Cæsarean section was determined upon. The section of the uterus caused considerable hæmorrhage, which was soon controlled by contraction. The patient sank about fifty hours after the operation. Lymph was found on the peritoneum, both lining the abdominal parietes and the intestines. Near the wound the small intestines were of a dark red colour, and injected. The uterus presented a dark red surface exteriorly; the divided edges were everted and widely separated, without any attempt at union. The pelvis was brittle throughout; all the articulations were loose, and the pubic and ischiatic portions of the coxal bones moved on each other. Both iliac bones were much distorted and very carious, the left being perforated in several places, and as thin as tissue paper; the right ilium was also very thin and diaphanous. The horizontal rami of the pubis were parallel, the pectineal eminence on the left side almost touching the promontory of the sacrum; the space by measurement was half an inch, but the bones were quite easily pressed together. The descending rami were also closed in, the left being carious and eaten through. The acetabula were also eaten through, and the head of the femur was in a carious state.

2. Dr. O. C. Gibbs discusses the propriety of forcibly removing the placenta from the uterus in cases where, hæmorrhage attending, it still remains adherent after abortions. He advocates the practice, recommending the use of the fingers, if sufficient: but otherwise instrumental aid. He prefers an instrument described and figured by Dr. Carey in the 'American Medical Monthly,' vol. vii. He relates two cases. In one, that of a woman who had miscarried at three and a half months, the placenta could not be reached by the finger, and manual efforts failed. Very alarming hæmorrhage occurred. Cary's instrument, which has a claw-shaped extremity, somewhat curved, the concave surface being rough, the convex smooth, was introduced, and made to sweep the entire cavity of the uterus, carefully keeping the polished surface of the convexity of the claw towards its walls. Then dragging the placenta down by the claw, and using a finger to fill the office of a second blade to the forceps, it was removed. The hæmorrhage immediately ceased, and the patient did well. In the second case, the patient was supposed to have reached six weeks of pregnancy. She had flooded for three days. It was concluded that the ovum could not be saved; it was removed by Carey's instrument with the same success as the first case.

3. M. Deville, in tracing the effects of ergot upon fœtal life, sets out by affirming that it is

almost always easy to determine the causes which have destroyed an infant *in utero*, such as premature delivery, vicious presentations, &c. When, he proceeds, none of these causes exist, and a mature, healthy child is born dead, presenting all the appearances of asphyxia, it may be declared that ergot has been given. Analyzing various documents collected during the years 1845 to 1848, in Paris, M. Deville arrives at this result: that out of 515 still-born children, 72, or about one-seventh, perished from ergot. Researches continued by M. Deville since 1848, show that the number of still-born children tends to increase every day, and that this increase is owing to the administration of ergot or to induced abortions.

4. Dr. Lehmann contributes a very valuable essay, accompanied by 11 cases, on rupture of the uterus. As to the *frequency* of this accident, it is reported to have occurred in 8 cases out of 10,387 labours by J. Clarke, in 1 out of 2947 by Mauriceau, in 31 out of 16,654 by Collins, in 3 out of 4180 by Paraud. According to Burns, the proportion is 1 in 940; Churchill, 1 in 650; Fritzel, 1 in 621; Keewer, 1 in 430; Bluff, 1 in 466; Ingleby, 1 in 300 and 1 in 4000; McClintock and Hardy, 1 in 737; Ramsbotham, 1 in 4429. In Dr. Lehmann's own experience in the Amsterdam Lying-in Hospital, which includes nearly 7000 labours in seventeen years, rupture occurred three times only. He has, however, seen 8 other cases to which he was called in consultation. As to the *etiology*, Dr. Lehmann says that rupture happens far more frequently from the violence of contractions without, than with, a pre-existing pathological alteration of the uterine tissue, as in the case of an obstruction too great to overcome. He has on several occasions convinced himself that the powerful contractions exerted to overcome a mechanical obstruction, are followed by a partial rent in the uterus or vagina only when the tissue, through violent pressure against the promontory of a rachitic pelvis, the crista pubis, or the sharp ileo-pectineal line, has undergone a kind of bruising, softening, or gangrene.

Dr. Lehmann then relates his cases, which we will here condense:—

Case I.—On the 24th of September, 1844, he was called by a midwife to a multipara, who felt her first pains at the end of her sixth month. Her previous labours had been lingering. During pregnancy she had suffered much from abdominal cramps. The waters escaped about noon, and the head presented. At 5 p.m. the patient was suddenly seized with violent pain, extending from the back to the abdomen, during an uterine contraction. From this time contraction ceased, and the presenting head disappeared. At 8.30 p.m. Dr. Lehmann found the abdomen much distended, and very painful on pressure, especially in the epigastric region. The abdominal walls were unusually thin; the fœtus seemed quite external to the uterus. The os uteri was flaccid, but partly closed again. The pelvis seemed normal. Considerable hæmorrhage had set in, and the patient's state was that of collapse. Delivery by turning was effected with great facility. The seat of rupture was not observed. The uterus contracted only moderately, it was soft, and a little blood appeared externally. The patient sank in a few hours. At the autopsy, thirty-six hours after death, the uterus was well contracted in the pelvis. Its anterior aspect seemed healthy; the posterior showed a rent, beginning at the fundus and running in an oblique direction for more than three inches down to the neck. The tissue near the rent was, as well as through the entire posterior wall, soft and anæmic, here and there œdematous: the edges had a livid, black colour, and were partly sphacelated. Behind the uterus, in the pelvic cavity, a large quantity of coagulated and fluid blood was poured out. The pelvis was quite normal.

Case II.—A cachectic multipara, aged thirty-six, was a fourth time pregnant, having previously been normally delivered; had been complaining for some months of rheumatic pains in the abdomen and legs. On the 22nd of December, 1845, at the normal term of gestation, pains set in. On the 23rd the os was partly open, head presenting. Irregular pains, and slow progress. The os was fully expanded, and the head had descended into the pelvic cavity, when the patient suddenly uttered a loud shriek, and exclaimed that her body was torn within her; the pains hereupon ceased, and collapse set in. When seen by Dr. Lehmann the abdomen was excessively distended, and painful to pressure. The head was still in brim, and some blood came from the vagina. On trying to apply the forceps the head receded. The hand introduced to turn; a rent across the anterior and left side of the cervix was felt. The child was extracted in two minutes. The uterus contracted immediately, and remained as a hard ball above the pubes. The child was still-born. The patient was removed to hospital, and partially rallied; but next day symptoms of anæmia and collapse returned, and carried her off three days later. The abdomen contained much blood. The uterus was well contracted; there was a rent running in an oblique cross direction, so that the anterior wall of the vagina where it is united to the uterus was nearly quite torn away. The texture of the uterus near the rent was pulpy and discoloured, and the uterus in several places was soft and pasty. The pelvis was generally too small; the conjugate diameter was 3·50″. The ileo-pectineal ridge was very sharp and strongly developed.

Case III.—On the 12th of May, 1846, Dr. Lehmann was hurriedly called to see a pluripara, aged forty She had been dead half an hour when Dr. Lehmann arrived. He learned that the woman had borne children quickly after one another; and that during the last months of her seventh pregnancy she had continually complained of pains in the abdomen, with fever. Labour-pains had come on the night before, but progress was tedious. In the morning the os was partly open, and the head presented. The enormous distension of the abdomen raised the belief of twins or excess of the liquor amnii. About midday, pains being light and irregular, the patient suddenly uttered a shriek, and became corpse-like, vomited a black bloody matter, and died in a few minutes. The abdomen was greatly distended, and the parts of the child could be easily felt through the thin walls. The pelvis seemed normal. Autopsy not permitted.

Case IV.—On the 19th of July, 1847, Dr. Lehmann was called to a woman, aged thirty-two, cachectic, and pregnant for the fourth time She was already dead. The first pains had appeared the night before; they were weak, irregular, and painful. In the night ergot was give on account of this *torpor uteri*, but no more active labour ensued. When the os was fully expanded, and the head depressed in the pelvis, the forceps was applied. During the tractions the patient had suddenly risen up and fallen back dead, whereupon the forceps was withdrawn. On the right posterior side, where vagina and uterus unite, there was a large oblique rent, through which the hand passed to the promontory, so that nearly the entire posterior wall of the vagina was torn away from the uterus. A large quantity of blood followed as the hand was withdrawn. The pelvis seemed contracted in the first degree; the conjugate diameter being 4″. The abdomen was much distended, and as if divided in two unequal parts. The child being possibly alive, the forceps was again tried, but the head slipped away on traction. It was extracted, still-born, by version. Autopsy not allowed.

Case V.—On the 9th of July, 1852, a woman, aged twenty-six, who had been rachitic in childhood, only beginning to walk at seven, was taken into the hospital, and delivered of her first child by cephalotripsy. She returned on the 22nd of August, 1853, again pregnant, to have premature labour brought on. She was very small in stature, and deformed. Pelvis kidney-shaped; conjugate diameter 2·75″ The labour was fixed upon at about thirty-four weeks of gestation on the 1st of September. For this end a wax bougie was inserted, and repeated on the 3rd; next day pains appeared; the os opened somewhat; head presenting. At 11 P.M., 5th, the waters escaped in great quantity; on the 6th, labour being tedious, a borax mixture was given; on the 7th, labour advanced but slowly; strong pains set in; the head was slowly driven quite into the pelvic brim; signs of exhaustion were increasing. Whilst waiting for the forceps, the patient complained, after a great exacerbation of the pains, of headache, syncope, oppression, and began to vomit copiously. Collapses followed. The child was quickly delivered by forceps; when the head passed much blood came, which was attributed to the separation of the placenta. A few minutes later the placenta was removed, whereupon the uterus contracted slowly, remaining larger than usual. The eight-months' child was dead; the skull was much compressed. On the next day, there having been no reaction, the abdomen was more painful and much distended, tympanitic; peritonitis followed, and ended fatally on the eighth day after delivery. A large quantity of purulent exudation was found in the abdomen. The uterus was well contracted; its peritoneal covering being clothed with an exudation-layer. On the fore part of the lower segment and cervix was a cross rent three inches long, the edges of which were uneven and discoloured; the whole posterior wall of the bladder was also torn and gangrened, forming with the rent in the cervical portion of the uterus a common cavity. The tissue of the uterus, and also of the bladder in the vicinity of the rupture, was very pulpy. Much fluid and congested blood in the abdominal cavity. The inner surface of the uterus was of a greyish-black colour (*endometritis gangrenosa*), the musculature of the fundus was normal, without fatty metamorphosis. The conjugate diameter of the pelvis (which is preserved) was 2·50″. The brim was kidney-shaped.

Case VI.—A mother of five children, pale and cachetic, complained much of pains in the abdomen during her sixth pregnancy. She had ague. On the 18th of April, 1855, term of gestation ended, pains set in; labour progressed very slowly during the two succeeding days; the os was open about an inch, and the breech presented; the pains ceased, and exhaustion appearing, Dr. Lehmann was sent for on the 21st. The abdomen was unusually distended; uterus lax, but exceedingly painful to touch. The child had not been felt to move for some days; auscultation revealed nothing. The child was felt quite moveable at brim; right shoulder presenting; hand introduced; a large quantity of coagulated blood was found in the uterus, and on the right hinder side at the fundus, a large rent in an oblique direction, through which the lower extremities of the child had escaped into the abdominal cavity. The child was delivered still-born and putrid. After the placenta was removed, during which a large quantity of blood flowed, the uterus contracted moderately and remained soft and large; collapse followed, and in two hours death. Autopsy not permitted.

CASE VII.—A healthy multipara, of ordinary stature, aged thirty-seven, was, on February 3rd, 1857, at the end of her fifth pregnancy. According to her statement her first two children were born dead from funis presentation. Her third child was born alive The fourth labour was conducted by Dr. Lehmann in the hospital; it lasted four days, and resulted in a dead child. A narrowing of the pelvic brim was recognised as the cause of this. In the fifth pregnancy the abdomen was of very large circumference and greatly overhanging. The fœtal heart was heard on the left side. The os uteri was open 2″; head at brim, and moveable. The waters broke on the 4th, and the os expanded fully all but a thick border. Towards evening the pains had quite ceased, and exhaustion progressed; but the abdomen was free from pain, and the fœtal heart was still heard. It was then determined to deliver by turning. This was effected with some labour. The child was dead, weighed eight and a half pounds, and showed a depression on the left parietal bone caused by pressure against the promontory. After removal of placenta, the uterus contracted, but remained of considerable size. Collapse followed; and death early the next day. In the abdominal cavity was a considerable quantity of fluid blood. The musculature of the uterus was soft, flaccid. At the posterior side above the left Fallopian tube was a large rent, stretching obliquely from the fundus to near the neck. The walls of the uterus were very thin, and here and there infiltrated with serum. The uterine tissue at the seat of rupture appeared fatty. The pelvis was kidney-shaped at the brim, and showed also a considerable scoliotic curving of the sacrum. The conjugate diameter was 3″. The rent had taken place at the part where the head had pressed it against the promontory. Probably, says Lehmann, an inflammatory softening had ensued from the continuous pressure against this spot during the latter period of gestation. The slight power evinced by the uterus during labour seemed due to the diseased condition, which was verified by dissection.

CASE VIII.—On March 5th, 1858, Dr. Lehmann saw a woman, aged thirty-six, pregnant for the ninth time. The waters had escaped two days, and the patient, who had been quite well during this pregnancy, experienced slight pain in the lions and abdomen. A hand was presented. Time was lost in fetching Dr. Lehmann; the shoulder was driven down; attempts to turn were now rendered vain by the strong contractions. Dr. Lehmann found the patient exhausted; abdomen much distended, and painful to touch. Pains had now ceased. This symptom had occurred suddenly, and had been followed by collapse. Turning was now effected with unusual facility. Child still-born. The placenta removed, the uterus contracted and remained as a hard body. Little blood escaped. Without rally, the patient died on the following morning. Autopsy not allowed. The rent was not felt during life. The diagnosis rested on the symptoms.

CASE IX.—In May, 1854, Dr. Lehmann was called to a multipara, aged thirty-eight, on account of a shoulder presentation. An obstetrist called in had failed in turning on account of the strength of the contractions. The patient was collapsed, abdomen much swelled and exceedingly painful; the uterus loosely drawn around the child. The pains had entirely ceased, and the fœtal heart could not be heard. The presenting shoulder was easily pushed back, and the version and extraction effected. Child dead. The placenta with a great quantity of blood followed spontaneously. The uterus contracted well, but collapse increased, and death speedily followed. Autopsy refused. Diagnosis rested on symptoms.

CASE X.—In November, 1856, Dr. Lehmann was called to a primipara, aged twenty-six, on account of retention of the placenta. The breech had presented and had been expelled by the uterus; the head had been delivered by the forceps. Child dead. The patient was much exhausted, and the uterus contracted. Blood flowed continuously. In passing the hand for the placenta, Dr. Lehmann found a large cross-rent in the lower segment of the uterus where the vagina is connected behind, through which the entire hand passed into the abdominal cavity. The pelvis was contracted at brim, the conjugate diameter being scarcely 4″. The visceral convolutions projecting through the rent were replaced, and as efforts to open the os uteri could only endanger the widening of the rent, the separation of the placenta was left to nature. This was effected on the fifth day. Endometritis followed, and malignant puerperal fever carried off the patient on the tenth day after labour.

CASE XI.—On the 2nd May, 1858, a woman aged thirty-five, was in labour in the Lying-in Hospital with her second child. Gestation had been normal. The belly showed an unusually strong pronation of the uterus. The fœtal heart was heard in the right side. The os was open 2″ with a thick, soft edge, the head presenting but lying much over the symphysis. The labour went on tediously, notwithstanding strong pains. At 10·30 of the 3rd May, the os was fully expanded, but the head had not altered for many hours; a miscoloured, stinking fluid escaped from the vagina; the pains had ceased. The uterus was strongly contracted round the fœtus; the abdomen was of irregular form, but free from pain. With the lever the head was first brought into the pelvic brim, and then delivered by the forceps, dead. The uterus contracted well. For the first few days the woman did well. The uterus, however, preserved a large size, but remained painless. The labia pudendi began to swell, and gangrene appeared in the mucous membrane of the vulva. Sloughs fell. On the seventh day a light fever appeared, with shivering resembling ague. This was successfully encountered by quinine. On the tenth day, when the patient seemed doing well, the uterus having sunk into the pelvis, a copious flooding suddenly came on and destroyed the patient. It was concluded that, through a gangerous softening at a higher part, vessels had been injured, and caused the flooding. The uterus was found at the neck and the upper part of the vagina, especially at the left, and behind, quite gangrenous, and a large round hole with sharp edges quite through. The muscular fibres were fatty. About 1·50″ from the neck was a second smaller hole; in this divided vessels were seen which had given rise to the fatal flooding. At the posterior side of the body of the uterus which had lain against the promontory, was an incomplete rupture of 1·50″ long. The pelvis was narrowed at the brim; the conjugate diameter measured 3·20″. The symphysis pubis was very thin and softened; the ossa pubis through diastasis separated more than one inch. Purulent fluid escaped from the articular cavity. The ossa pubis and the innominata were moveable. [One cannot help observing that several of these women were in labour too long, and that delivery by perforation or turning at an earlier period would have saved the lives of some.—REPORTER.]

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Analytical and Critical Reviews.

Review I.

Die Cellular-Pathologie in ihrer Begründung auf Physiologische und Pathologische Gewebelehre. Von Rudolf Virchow, Prof. der Pathol. Anatomie der Allgemeinen Pathologie und der Universität.—*Berlin*, 1858. pp. 440.

Cellular Pathology, as the Foundation of Physiological and Pathological Tissue-Doctrine. By Rudolf Virchow.

This work consists of a series of lectures and demonstrations, delivered at the request, and for the benefit, of the practitioners of Berlin, who were anxious to be made acquainted with the progress of pathology, as taught by Rudolf Virchow. Expressly composed, therefore, for the benefit of busy men, who have long left the schools, and who have not been able to keep pace with all the microscopical debates of the day, this work is remarkably pleasant reading, for it proceeds from simple to difficult propositions, and from old to novel doctrines, with so easy a step that no one will have any difficulty in following it. It gives, too, at one view, a general summary of those opinions which Virchow has spent many years in working out, and which were previously scattered through a host of writings, and are not even adequately represented in the two large volumes of his collected essays which were published some three years ago. We were therefore glad to meet with an advertisement a few weeks since, announcing that a translation of the Cellular Pathology will soon be published; and we anticipate that this translation will excite much interest.

Few authors of our day have written so voluminously as Virchow, and it is but justice to say, that few have been so largely read. Another man would have tired the public with his incessant dissertations, but Virchow has thrown into every essay so much observation, such numerous facts, and has looked at these facts with so much of the instinct and fire of genius, that we know not one of his works we should desire to see cancelled.

Let us, however, at once say, that while we appreciate his extraordinary powers, and admire the independence with which he looks at every fact and doctrine with his proper eyes, and without borrowing the spectacles of other people; and while we acknowledge most completely his many services to pathology, we yet hesitate to acknowledge in Virchow the coming man who is to gather up into one consistent doctrine the *disjecta membra* of the old medical creeds, so ruthlessly shattered by the application of the new methods of inquiry. It may be, to use his own simile, that he has shared in the flight out of Egypt; but we are afraid he is not the Joshua who is to lead us into the land of milk and honey. At any rate, before we get there, we can perceive that there are stern and determined enemies in the way; Philistines and Amalekites in walled cities, not at once, we suspect, to be laid defenceless by the mere blast of the trumpet. But let us put the rival hosts in presence, and we shall better see to which side victory will incline.

We had at first intended to present a succinct but complete analysis of this work; but on consideration, and bearing in mind that a translation will soon be made, we have determined rather to select the most original and fundamental points for discussion, and not to attempt an exhaustive account. The work is itself very much condensed, and we could hardly do justice to some parts of it in our space.

With regard to cells, animal and vegetable, of which a good account is given in the first chapters,[*] Virchow, following Remak, entirely discards the theory of formation given by Schleider and Schwann. He acknowledges no development of a cell in an amorphous blastema, by the successive formation of a nucleolus, a nucleus, and a cell wall; and refuses assent to the doctrine that a cell originates in an aggregation of molecules, which then undergo a differentiation, so that some cohere to form a nucleus, and others form the outer wall by a still more intimate fusion. In the place of these views, Virchow advocates the principle, that in every case, physiological and pathological, in the vegetable as in the animal kingdom, *a cell arises only from a pre-existing cell.* As regards animals, so also with cells, there is no *generatio æquivoca*, no spontaneous generation; one cell springs from another by endogenous growth, or by fissure and cleavage of nuclei and cells.

This doctrine, if true, would at once necessitate an entirely different reading of many pathological phenomena. We are in the habit of saying that, in inflammation, for example, an exudation of albuminous or fibrinous substance is poured out between the tissues or on the free surfaces, and that this exudation then organizes itself into cells by spontaneous generation; and it has also been a creed that the said exudation, in virtue of, or from the absence of, special physical organizing powers, might either form perfect cells, or might develop into fibres, or (on the contrary) be unable to form cells in consequence of deficient or low *a-plastic* power. The whole of this doctrine Virchow rejects as an entire mistake; and he refuses even to use the term exudation in the sense in which it has been used by the Vienna school, and adopted in England—i.e., as an effusion which may become organized. He does not, of course, refuse to admit the existence of transudations, but these he considers to be quite distinct from the plasmatic effusions to which the term exudation has lately been restricted.

Our readers will no doubt at once say, "Are not pus cells formed on free surfaces —such as mucous membranes or the pleura, or in the interior of solid organs; and is not this from transformations of what was at first an amorphous exudation?" "Not so," reply Virchow and his followers; "every pus cell arises from a previous cell, it in no case originates *de novo;* your notion of a fibrine-looking plasma poured out on a mucous membrane or on the pleura, and forming pus by internal changes, is an entire mistake. *Omnis cellula e cellulâ.*"

How, then, does pus originate? we may ask. The answer of Virchow's school can be given, not only by the work we are reviewing, but by a reference to an essay on the doctrines of the origin of pus by Professor C. O. Weber, which has been published since the Cellular-Pathology, and may be said to have received Virchow's implied sanction.[†]

To make it clear, however, to our busy readers engaged in practice, who are not familiar with the present discussions on histology, we must make a short digression, and refer to Virchow's opinion on the anatomy of areolar tissue. This opinion lies at the basis of many of his new views, and it must be explicitly understood.

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[*] We would refer also to an article by Professor Huxley, in a former number of this Journal, on the Cell Theory. Virchow quotes from, and has evidently appreciated, this admirable essay.

[†] Zur Entwicklungsgeschichte des Eiters, von Prof. C. O. Weber, in Bonn: Virchow's Archiv für Pathol. Anat. Band xv. p. 465. 1859.

If our readers will look at the last edition of Quain's 'Anatomy,' they will find a brief but perfect account of the views of Reichert and Virchow on the areolar tissue. Instead of regarding areolar tissue as a mass of individual fibres, Reichert described it in 1848 as an homogeneous substance, the fibrillation of which was produced by manipulation. In 1851, Virchow,* and, almost at the same time, Donders,† described as dispersed through the homogeneous substance of the areolar or connective tissue certain cellular bodies, similar to, or identical with, the cells of cartilage and of bone (*Bindegewebskörper*). These bodies are described as round or spindle-shaped cells, separated by intercellular homogeneous substance, and from them proceed exceedingly fine canals, which anastomose with the canals coming from other cells, and thus is supposed to be formed throughout the areolar tissue a vast communicating system of vessels, through which nutrient fluid can pass; and which, in fact, form a system of circulation subsidiary and complementary to the circulation in ordinary bloodvessels.‡

This view has given rise to great controversy; and has been especially combated by Henle, whose immense experience and assured judgment necessarily give his opinion the greatest weight; while it has been more or less completely admitted by Leydig, Kölliker, and others, and has been adopted without hesitation not only by Virchow's immediate school, but by many (Professor Weber, of Bonn, for example) who cannot be considered as the special followers of Virchow.

Now, this view of the nature of the connective tissue is, we may say, a vital point in the doctrine of Virchow, for these corpuscles of the areolar tissue are made to play a most important part in pathology. From them are supposed to arise many, if not most, of the morbid growths; from them spring in many cases pus cells, and from them, in act, is made to date the commencement of a vast number of pathological processes.

If this doctrine should be overturned, much of the newest pathological teaching rolls with it in the dust, and it is obviously of the greatest importance to be certain of the safety of so cardinal a point. In this country, the general opinion of the teachers of minute anatomy seems, as far as we know, to be one of hesitation and uncertainty rather than of denial or assent, and we presume that in Germany this feeling is also widely shared. Some of the latest observations on the development of areolar tissue appear not to give support to Virchow's opinions,§ and in spite of the familiarity with which Virchow's pupils speak of seeing the changes in the connective-tissue cells, it must be admitted that it is difficult to suppose the appearances can be so obvious when so many good observers are unable at present to convince themselves of the accuracy of the descriptions.

Still Virchow has no doubts, and as we are explaining his views, we shall express ourselves from his point of view, merely guarding ourselves from the imputation of giving more certainty to this view than can really be at present assigned to it.

As already said, the "connective-tissue bodies" are made to play so large a part in pathology, that it looks as if, like Aaron's rod, they were to devour all previous hypotheses. To say nothing of the changes in allied structures, such as cartilage cells, so well investigated some years ago by Redfern, and lately by Virchow and Weber, or the cells of the cornea as described by His,‖ or those of bone, the pathological conditions of the cells of the connective tissue proper are now made to embrace so wide a range, that if these views are confirmed, the usual descriptions of inflammatory conditions, and of tumours and growths of almost all kinds, will have to be modified. We shall see this more completely hereafter.

To return to the question of the origin of pus. As early as 1852, Virchow had shortly expressed his present views, and at a later period (1855 and 1856) had still more explicitly stated that pus is "always a product of a continual tissue-development." He now briefly recapitulates his opinions as follows:

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* Identität der Knochen, Knorpel, und Bindegewebskörperchen: Würzburg Verhandl., Band ii. 1851.

† Siebold and Kölliker's Zeitschrift, Band iii. p. 848.

‡ See especially Wittich, in Virchow's Archiv, Band ix. p. 185. 1856. His experiments, however, have not succeeded with others.

§ Baur: Die Entwicklung der Bindesubstanz. 1858. Virchow has replied to late objections in a recent number of his Archiv (Band xvi. p. 1. 1859.)

‖ Beiträge zur normalen und pathol. Histologie der Cornea, von Dr. His. Basel, 1856.

> "We see daily the formation of pus on surfaces, as well on the outer skin as on mucous and serous coats. We can most safely observe the formation where strata of epithelium naturally exist. If you follow the formation of pus on the outer skin without ulceration, you will see that the suppuration regularly proceeds from the rete Malpighii. It consists in a growth and development of new elements in the same. In proportion as these elements grow, a separation of the harder layers of epidermis occurs, and a vesicle or pustule arises. The place where the suppuration principally occurs corresponds to the superficial layers of the rete, which are already passing into epithelium; if the membrane of the vesicle is detached, these (the layers) usually remain still on the upper skin. In the deeper layers it can be seen that the cellular elements, which originally have simple nuclei, gradually divide, the nuclei become more numerous; in the place of a single cell, many are present which are themselves provided with again dividing nuclei. This has generally been explained in this wise; it has been received that an exudation first occurs in which the pus is formed, and it is well known that many of the investigations on the formation of pus were made on these fluids. It was very conceivable, so long as the '*discontinual*' cell-growth generally was not doubted, that the young cells should without more ado have been looked on as independent new formations, and it should have been believed that germs arose in the fluid, which gradually becoming more numerous, formed pus. But the fact is this, that the longer the suppuration lasts, so the more completely one range of cells after the other is implicated in the process of growth, and that while the vesicle is being elevated, the quantity of growing cells in it becomes even greater. When a small-pox pustule is formed, a drop of clear fluid is first present, but nothing arises in it; it merely loosens the neighbouring parts.
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> "The process is exactly the same in mucous membranes. There is no mucous membrane which will not give rise to puriform elements under certain circumstances. Only a certain difference is always obvious. A mucous membrane is so much the more in a condition to produce pus without ulceration, the more perfectly it possesses pavement epithelium. All mucous membranes with cylinder epithelium are much less disposed to form pus; the matter which is produced is found on accurate examination to be only epithelium, though it may have a thoroughly purulent appearance. The intestinal mucous membrane—that of the small intestines, for example—almost never produces pus without ulceration. The mucous membranes of the uterus, of the tubes, which is often covered with a thick mass of entirely puriform appearance, separate almost always only epithelial elements; while on other mucous membranes—the urethra for example—we observe copious discharges of pus, as in gonorrhœa, without the least ulceration being present; that depends essentially on the presence of many strata of cell-layers, the upper of which form a sort of protection for the lower, so that the growth of these latter is secured for some time. The pus is finally thrown off by continually growing collections of pus, or there occurs simultaneously transudation of fluid which carries off the pus cells from the surface, just as in the secretion of semen the epithelial elements of the seminal canals form the spermatozoa, and coincidently a fluid transudes which carries these off. But the spermatozoa do not arise in the fluid; this is only the vehicle of their continued movement. In this way we frequently see fluid which cannot be looked on as the building-places for cells, exude on the free surfaces of the body. If at the same time there is a growing epithelial formation on the free surface, the elements loosened by the transudation will exhibit only growing epithelium."*

After some remarks on the forms of pus, mucus, and epithelium, and on the necessity of not confounding these three forms when developed, although they proceed from the same foundation cell, so "that pus, mucus, and epithelial cells are pathologically identical parts, which can be substituted for each other, though they cannot perform each other's functions," Professor Virchow goes on to observe, that in deeper parts purulent formations proceed from the connective tissue.

> "This deeper pus-formation occurs regularly in the connective tissue (*Bindegewebe*). In it occurs, first, an enlargement of the cells (*Bindegewebskörperchen*); the nuclei become divided, and for some time grow excessively. On this first stage follow very soon divisions of the elements themselves. In the environs of the irritated parts, where formerly single cells lay, double and manifold cells are later found, out of which arises generally a new formation of an homologous kind (*Bindegewebe*). More in the interior, however, where already

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* Cellular Pathologie, pp. 397–8.

> the elements are largely filled with nuclei, heaps of little cells appear, which at first have the same direction and forms as the former connective-tissue corpuscles. Somewhat later we find here round collections, or diffuse infiltrations, in which the intermediate tissue is very scanty, and is continually more and more destroyed in proportion as the cell-growth extends.
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> "If this process takes place on an undestroyed surface, the yet adhering epithelial layers can be seen to pass over the irritated and somewhat swollen parts. The outermost layer of the intercellular substance is also preserved for a long time, while all deeper parts of the connective tissue are already filled with pus-corpuscles, are infiltrated or abscessed. Lastly, the surface gives way, or without giving way is directly transformed into a white diffluent mass. By and by these forms give the so-named granulations which always arise from a tissue, where in a small quantity of soft intercellular substance, more or less numerous round elements are imbedded, at least, in the special growing stage of granulation. The more the surface is approached, the more do the cells, which in the deeper layers were more uninucleated, show divisions of the nuclei, and on the farthest limits these cells can no longer be distinguished from pus cells. A detachment of the epithelium then tends to occur, and then it may be that the ground-substance liquefies and the single elements become free. If the growth still goes on largely, the mass still continually breaks up, the elements accumulate on the surface, and a destruction goes on which continually invades the deeper tissues, and ever throws more elements on the surface—that is peculiarly an ulcer." (p. 400–1.)

In this description it will be seen that the idea of an exudation is altogether discarded, and that the whole process is one of growth, only the developed cells do not assume the epithelial form, or remain as normal cells of areolar tissue, but pass into that closely allied form of pus cell.

These views are still further developed by Weber, who has described very fully suppuration of the periosteum, of muscle, of nerve, and of skin, and who, in addition, has made various interesting observations which seem to show that epithelial cells are in many cases nothing but modified connective-tissue corpuscles, so that not only are the connective tissue, tendinous tissue, bones, and cartilage considered as of one family, but the epithelial formations must, it would seem likely, be included in the same class. Böttcher* has also carefully examined suppuration of muscle, especially of the heart, and has shown that this depends on changes in the areolar tissue and its corpuscles, which this observer has traced in the tissue of the heart.

We quote part of the description of suppuration of the periosteum, as giving most completely Weber's opinions:

> "I must first observe," writes Weber,† "that the connective-tissue cells of the periosteum form, as V. Wittich first showed by imbibition in tendons, an anastomosing network, which can be made visible by the application of diluted chromic acid, but the recognition of which requires some familiarity with the appearances of such networks. The cells of this network have usually a perfect spindle form, and lie arranged in longitudinal rows. The anastomoses are very delicate, and are best seen by further tearing out, or after the imbibition of colouring substances, especially and beautifully of carmine. After the application of acetic acid the cells appear most visible, and they are isolated by the use of hydrochloric or dilute pyroligneous acid. The cell wall lies indeed so close to the ground-substance that one could speak only of imbedded nuclei, did not the enucleation of the cells prove their existence. If now the periosteum is examined in a suppurating fracture (as is unfortunately often seen, and gives rise frequently to amputation), or in an amputation stump of a man dead soon after amputation, there is seen a membrane swollen with fluid, which can be readily drawn from the bone, and whose silver-glancing fibrous threads clearly appear. Perpendicular section through such a membrane shows only, particularly at the loosened parts, which are apparently moistened with a gelatinous exudation, appearances which are also met with in the healing of fractures without suppuration; the extreme (of these appearances) only occur, however, in the suppurating periosteum (also in caries), and also are only visible when the periosteum hangs in a suppurating wound, or passes into granulations. To learn the genesis of the appearances we must look at the periosteum at some little distance from the suppuration. We see here, in all the above referred to cases, the nuclei of the connective-tissue corpuscles increase by division, with attendant enlargement of the original cellular space; the cells are then sometimes globular, sometimes retain their usual spindle form; finally large spaces are formed which are entirely filled with round nuclei, which contain one, two, three, or five nucleoli, and exactly

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* Virchow's Archiv für Pathol. Anat., Band xiii. p. 227. 1858.

† Virchow's Archiv, Band xv. p. 477. 1859.

> resemble pus bodies. Not infrequently one sees also divisions of the cells themselves; in the place of the spindle-shaped body, we see an indented cell, then two conical cells touching by their bases, then finally three or more, and in them again growing nuclei. The capillaries of the periosteum are thickly covered with similar pus cells, which evidently arise from growth of the so-named nuclei of the capillary walls. . . . .
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> "The pus formation in the periosteum follows, therefore, according to my observations, chiefly an *intra-cellular* course, yet there can decidedly occur division of the connective-tissue corpuscles into cells."*

As in the periosteum so in the suppuration of muscles, the pus cells form in the so-called nuclei, which Böttcher has shown to be nothing but the nuclei of connective-tissue bodies with extremely delicate walls, which interlace among the muscular fibres and run into the tendons, from which they can be coloured by imbibition. According to Weber and Böttcher, these bodies can be seen without preparation in fatty degeneration, in scirrhus, cancroid, and sarcoma; while in fresh and healthy muscle it is extremely difficult to see them without preparation in diluted chromic acid, or after imbibition with carmine. Weber gives a drawing of a suppurating and fatty gastrocnemius, in which the anastomosing areolar tissue and its cellular bodies were beautifully seen.† Fatty degeneration also begins in these bodies.

But, our readers may say, if pus-cells thus arise always from changes in cells, either epithelial or connective-tissue, from divisions or growth and cleavage of nuclei, and if an exudation has nothing to do with them, all our ideas of inflammation must be modified. And so, no doubt, they must be, if these views be correct, as will be evident from the following considerations.

In the old enumeration of the chief inflammatory symptoms, redness, swelling, heat, and pain, the main importance was formerly attached to the heat, from which symptom, indeed, the term inflammation was directly drawn. Afterwards the redness became the cardinal sign, and, especially by the French pathologists, hyperæmia and stasis were looked on as phenomena without which inflammation could not occur; but the observation of inflammation in the cornea and in cartilages, has shown that parts may be acutely inflamed at a distance from bloodvessels, and that hyperæmia is not essential. The doctrine of exudation, as taught by the Vienna school, elevated a third sign, the swelling, into the essential symptom, since the swelling was supposed to be chiefly owing to exudation, and without exudation the Vienna school could not conceive inflammation to exist. And certainly, enlargement of parts must be considered as a necessary condition, only (in Virchow's view) it is not owing to exudation, but (in addition to hyperæmia and transudation of serum in certain cases) to rapid growth of the cells. And as those rapidly growing cells tend to form pus cells, the limit between inflammatory hypertrophy and suppuration is, as Weber remarks, very narrow.

To complete this enumeration, we may observe, that the pain has been considered by the nervo-pathologists as the starting-point of inflammation, but the progress of histology, and the occurrence of inflammatory processes in nerveless parts, seem at present to give little support to those who assume this point of departure for this complex process.

What, then, as far as we can see, is Virchow's idea of inflammation? To answer this we must refer not only to the work before us, but also shortly to a very elaborate and speculative paper on 'Irritation and Irritability,'‡ in which the views of Haller, Bichat, Broussais, Alison, and other physiologists are considered, amended, or adopted.

Irritability is, as Virchow puts it, a property and a criterion of every living cell and cell-derivative, and does not belong merely to nervous or muscular tissues. It consists in the power possessed by every cell to be impelled to certain actions by the influences which reach it from without—i.e., either from other parts or elements of the same organism, or from bodies entirely foreign. The irritating power, that which impels the cells or their derivatives to the manifestation of their actions, may reach the cells through nerves, through vessels, or through adjoining parts. The muscles receive the irritation from the nerves, the cells of many glands receive it from the blood, which brings directly to them exciting substances which pass into their substances. Every irritation produces in the structures acted upon mechanical or chemical change; the former can be gross, or fine—i.e., molecular. This change or irritative act is a counter-working against the irritative cause, a reaction against an action working from without. Now, this power of reaction is possessed only by cells or their derivatives. Every cell is an unit of life, and manifests by its reaction, whatever the nature of that may be, the appearance of life. In fact, from this reaction only can we know that a part has life.

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* Weber: op. cit., p. 477–80.

† Billroth (Beiträge zur pathol. Histologie, p. 24. Berlin, 1858) has thrown doubt on Böttcher's statements and figures, but Weber (op. cit. p. 482) affirms most positively that Böttcher is entirely correct. In speaking of Billroth's work we may strongly recommend it to our readers as containing a mass of important and novel facts in pathological histology, and as giving a good discussion on the new questions of the day. Billroth thus sums up his own opinions on the formation of pus: "The formation of pus occurs almost exclusively in the areolar tissue, as the connective-tissue corpuscles produce more speedily new cells through cleavage, and in that respect are more prone to development than the cells and nuclei which are contained in other tissues." (p. 56.)

‡ Reizung und Reizbarkeit, von R. Virchow: Archiv für pathol. Anat., Band xiv. p. 1. 1858

The actions of cells or their derivatives are divided by Virchow into three classes: the nutritive actions, the functional actions, and the formative actions, though the limits of these cannot always be defined. Formerly the exercise of function of a part was supposed to be so closely connected with its nutrition, that one involved the other; that, for example, a nerve could not act without being changed in structure, and requiring a renewal of nutritive action and repair to restore it to functional activity. But this view must be somewhat modified, as it is certain that a part may again become functionally capable after exhaustion, without any time having elapsed for nutritive restitution, or even when it is separated from the body. A nerve or muscle exhausted by a stimulus can, for instance, regain the power of action by rest, even when separated from the body; and when, therefore, nutritive repair is impossible, and still more convincingly, the well-known fact may be referred to, that after exhausting a muscle or nerve by one stimulus, and thereby apparently damaging its nutrition, it can be again excited to action before nutritive repair can have occurred, by varying the kind or amount of stimulus. There is therefore, to a certain extent, a functional repair different from the nutritive repair; the particles of the tissue dislocated by the functional action, to use Virchow's words,

> "Return to their original condition, truly not without probably a certain chemical alteration, but yet so trifling a one that at least the nutritive metamorphosis is no necessary condition of the renewal. Only if the function is very long continued, if the parts are actually exhausted, is *nutritive restitution* needed."[*]

So also the nutritive and formative actions must be distinguished; for example, a cell may grow, may become larger, or it may give rise to other cells. In the first case, there is simple nutritive hypertrophy, in the latter case formative hypertrophy, or hyperplasia, as Virchow has termed it.[†]

Now, the essence of inflammation, in Virchow's view, is that it is an increased irritation, which causes the cells to manifest their reaction, and to attract and take up more material into their substance. The irritation is the *primum movens*, then follows an attraction of nutritive substance into the cells, then nutritive changes in the cells, nutritive or formative reaction. And the supply of nutritive substance may be drawn either in the ordinary way from the bloodvessels, or from neighbouring parts, which not being irritated, may be robbed of their nutritive supply by the increased attraction which the irritated parts have for the nutrient fluid.

> "The formative activity, as the nutritive," writes Virchow, "begins always with a more or less evident enlargement of the elements, but it is distinguished by a division of the nuclei, which, generally preceded by division of the nucleoli, very quickly occurs. Very soon there is a certain severalness of formation, for in some cases the division of the nuclei is predominant, and continues, while in others, as soon, a division of the cells follows."[‡]

An extract from the "Cellular Pathology" will more completely illustrate Virchow's views:

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[*] Reizung und Reizbarkeit, p. 17.

[†] Handbuch der speciellen Pathologie, Band 1. p. 327.

[‡] Reizung und Reizbarkeit, p. 46.

> "A range of inflammatory processes manifest in their first appearance nothing more than an increased taking in of material into the interior of cells, which process must be regarded as entirely the same as in simple hypertrophy. If, for example, we consider the history of morbus Brightii in its customary course, it is found that the first thing we can generally prove in such kidneys is, that in the interior of the yet intact renal canals, the single epithelium cells, which it is known are already tolerably large, increase in size still more; the renal canals are thus filled with epithelial cells which are not only very large, but are also very cloudy, for in the interior of the cells a great amount of material is accumulated. The entire canal is therefore broader, and appears already to the naked eye as a contorted white opaque part. If we isolate the single cells, which is rather difficult, because the cohesion of the individual cells has already suffered, we find in them a granular mass which apparently contains nothing more than the granules which naturally are present in the interior of the cells, but which are so much thicker the more energetic the process is, so that gradually even the nucleus is thereby obscured. That is the condition of opaque swelling which we find on many irritated parts, as an expression of the irritation which attends many forms of the so-called inflammation. From these processes backwards to the appearances of simple hypertrophy, there are hardly any recognisable boundaries. We cannot beforehand say, when we meet with a part so enlarged and with the said abundant contents, whether it will continue to exist, or be destroyed, and therefore it is extraordinarily difficult, if one knows nothing of the process through which the change in question has been produced, to distinguish the simple hypertrophy from those forms of inflammatory processes which essentially produce an increase of the reception of nutritive material."*

After alluding to the beautiful researches of Redfern on cartilage, and again repeating that the process is the same in non-vascular and nerveless parts as in those supplied with bloodvessels and nerves, Virchow continues:

> "There is here, as you see, an essential difference from those opinions which were generally advanced as the next condition of these swellings. According to the old maxim, ubi stimulus, ibi affluxus, it was formerly thought that the first thing which occurred was the increased flow of the blood (which itself was referred back by the neuro-pathologists to the irritation of sensitive nerves), and then the immediate result of the increased flow was an increased separation of fluid, constituting the exudation which filled the part. In the first timid attempts which I made to alter this interpretation, I used the expression of 'parenchymatous exudation.' I had, in fact, convinced myself that in many parts where a swelling had occurred, there was nothing whatever to see except tissue. In a tissue constituted by cells, I saw in the swollen part nothing but cells; in tissues constituted by cells and intercellular substance, nothing but cells and intercellular substance; the single elements throughout were larger, fuller, filled with a quantity of material with which they ought not to have been filled, but there was no exudation in the way generally imagined—i.e, free, or in the interstices of the tissue. All the substance was contained in the elements themselves. That was what I meant to express by the term parenchymatous exudation, and from which the term parenchymatous inflammation is derived—a term which, indeed, was anciently used, but in an entirely different sense than that I meant, and which has now obtained greater currency than is perhaps necessary."†

Virchow then illustrates this nutritive action by a description of the changes produced in the cornea by irritation, and then proceeds:

> "To the appearances of this nutritive irritation (i.e., the cloudy swelling), the beginning of the formative changes is often very immediately joined. If we follow the highest degrees of irritation which take place in a part, it is seen that the elements, shortly after they have experienced the nutritive enlargement, show further changes, which begin in the interior of the nuclei, usually in such manner that the nucleoli are uncommonly large, in many cases somewhat longer, sometimes rod-like. Then as the next stage we generally see that the nucleolus has a depression, appears of biscuit form, and later two nucleoli are found. This division of the nucleoli points out the impending division of the nucleus itself, and the next stage is then, that about a such divided nucleolus the biscuit-like depression, and later, the actual division of the nucleus occurs, as we have already seen in the colourless blood cells and in pus cells." . . . .
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> "In many cases the changes are limited to this series of transformations, the end of which must be considered to be the division of the nucleus. This can be again repeated so that three, four nuclei, and so forth, arise. So it comes to pass that we sometimes find cells, not

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* Cellular Pathologie, p. 267.

† Ibid., p. 270.

> simply in pathological conditions, but also not seldom in entirely normal development, which possess twenty to thirty and more nuclei. Lately in the marrow of bones, namely, of young children, cells have been observed where the entire structure is stuck full of nuclei, which are often as large as the whole original cell. Such formations occur in many tumours in such quantities that in England a special kind of tumour is distinguished; and on the proposal of Paget, a myeloid tumour has been received into the classification." . . . .
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> "If we now take a step farther in these processes, we arrive at the new formation of the cell itself. After the growth of the nuclei has taken place, the cell as the containing formation may continue; only the rule is then that already after the first division of the nuclei, the cell itself divides, and that after some time cells are found, closely lying by each other, though a more or less straight wall of separation divided, and each possessing a particular nucleus; that is the natural regular mode of the actual increase of the elements."[*]

To recapitulate the process of inflammation according to this doctrine is so far in the main this: every cell is endowed with an independent life, i.e., a power of exhibiting certain reactions when stimulated. If the stimulus or irritation is natural, we have normal function or normal nutrition. But if the stimulus be unnatural or excessive, we have an excess of reaction; a cell, for example, grows too large (increase of nutritive activity), or forms too rapidly other cells or parts or derivatives of cells (increase of formative activity). Hence the limits between health and excessive growth (hypertrophy), or excessive formation (hyperplasia), and then between these conditions and inflammations, are extremely narrow, for hypertrophy and inflammation are merely exaggerations of nutrition. The swelling of an inflamed part is owing in great measure to this growth of cells and also to fulness of blood (hyperæmia) (and to transuded fluid ?), but these latter phenomena are of secondary importance and are not essential to the process. Of an organizable exudation in the sense of the old hypothesis of inflammation there is no evidence.

In some of these views—for example, in the doctrine of the attractive force exerted by the over-irritated cells on the nutrient material—there is an analogy with the views of Müller and of Alison and other celebrated British physiologists on normal nutrition. In fact, in Virchow's descriptions we are often reminded of the ideas which are found scattered in the pages of acute but speculative writers some half century ago.

So far the phenomena of inflammation, in Virchow's view, arise out of the activity of the cells, and lead to increased formation; to use the old phraseology, the productive powers of inflammation are in the foreground. But there are a series of what Virchow calls "passive processes," in which there is no manifestation of special activity by the cells; but on the contrary, an evident failure of nutritive and formative powers: and these passive processes are often the sequences of antecedent so-called inflammatory conditions.

> "I call those changes of the elements, passive disturbances, whereby they either simply lose the faculty of activity, or are destroyed so completely, that finally some loss of substance, some lessening in the sum of bodily constituents, is produced. Both series of passive occurrences taken together, viz., those which are denoted by an essential loss of power, and those which end with a complete destruction of parts, form as a whole the domain of the so-named *degenerations*, although, as we shall more accurately distinguish hereafter, a great part of the so-called active processes must be brought under the head of what is called degeneration. . . . .
>
> "This last category, where the elements during the progress of the process are destroyed, I have proposed to denote by an expression which K. H. Schultz has used generally to denote disease—viz., *Nekrobiosis*. We have to deal here always with a decay, with an annihilation, we might almost say with a necrosis. But the idea of necrosis really has no analogy with these processes: for in necrosis we conceive the mortified part as more or less retaining its shape. But here (*nekrobiosis*), on the contrary, the part vanishes, so that we can no more perceive it in its previous form. We have no necrotic mass at the end of the process, no kind of customary mortification, but a mass in which the form is absolutely unrecognisable. The nekrobiotic processes, which must be perfectly divided from the necrotic, have in general, as their final termination, softening."[†]

When the elements are not completely destroyed, but still remain in a condition more or less capable of action, the parts are generally firmer than usual, so that the term induration might be often properly employed, only there are many conditions in which the induration is not increased. This form of degeneration must be distinguished from the nekrobiotic processes.

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[*] Cellular Pathologie, p. 276.

Ibid., pp. 286-7.

Among the nekrobiotic processes the most important is the fatty degeneration; but other degenerative processes are also included. Now all these processes may follow on that abnormal activity which is the essence of the so-called inflammation; but they may also originate independently. Fatty metamorphosis may be primary or secondary (p. 315). The secondary fat metamorphose, as a rule, on a primary active stage.

> "One whole range of those processes," says Virchow, "which we unconditionally call inflammation, proceeds in such a way, that, as the second or third anatomical stage of the ulceration, a fatty metamorphose occurs. Here the fatty metamorphosis arises not as the immediate result of the irritation of the parts; but when we have an opportunity more accurately to follow the history of the changes, it is seen that almost always another stage precedes—that namely, of the cloudy swelling (*der trüben Schwellung*), in which the part is enlarged, increases in circumference and thickness, because it absorbs into itself a great quantity of material." (p. 315.)

Virchow then enters into a most interesting account of fatty metamorphosis, and of allied changes—as calcification and atheroma; but we must quit that part of the statement.

In perusing this account of inflammation, given, as far as possible, in Virchow's own words, and representing, we believe, pretty accurately, though briefly, his opinions, our readers will no doubt have already made one criticism. "It is all very well," they may say, "to direct our attention to the condition of the minutest anatomical elements—viz., these cells; to describe their growth, and to attribute everything to the changes produced in them by their increased attraction for a richer and more abundant nutritive fluid; but in all this we don't see any explanation of some of the most obvious phenomena of inflammation. Who that has ever seen a case of rapid pleurisy or pericarditis with pints of fluid and masses of false membrane on the surface, or of pneumonia with all the air-cells in a whole lobe blocked up with dense exudation, can rest content with this doctrine of cell-growth? We can conceive this would account for chronic inflammation, but surely not for these violent attacks, where in the course of a few days such enormous effusions occur. How does Virchow explain such cases of rapid inflammation?"

We must admit that Virchow has not dealt very closely with such forms of disease; but we conceive his answer would be that the collection of fluid is to be considered as a transudation; and that cases of inflammation with much transudation must be distinguished from cases without any.

> "Two forms of inflammation can be perfectly divided from each other; *the pure parenchymatous*, where the process runs on in the interior of the tissues, without any evidence of an exuded blood fluid; and the *secretory* (exudative) inflammation, which belongs more to the superficial organs, where an increased exit of fluid from the blood follows, which leads to the surface of the organ the peculiar parenchymatous material. These two forms are principally distinguished through the organs in which they occur; there are certain organs which, under all circumstances, are only attacked by *parenchymatous* inflammation; others in which almost every time a superficial *exudative* inflammation is recognised."*

With respect to the phrase, "that the transuding blood leads to the surface the material proper to the parenchyma," we must observe that Virchow supposes that in transuding the fluid is impressed by the natural action of the part or organ, and takes on more or less the characters natural to the fluids of the part.

In regard especially to fibrinous exudations, Virchow holds opinions so different from those usually entertained, that we are afraid we cannot properly explain them in the space we can assign to this subject. The usual notion is, that in inflammations the fibrine of the blood augments in amount, and by many persons this blood affection is thought to be anterior to any local affection. But Virchow entirely reverses this order, and denies that the fibrine found in any part of the body out of the bloodvessels has transuded from the blood. (p. 146.) On the contrary, fibrine *is supposed to be produced locally in certain organs, and from these organs it passes into the blood through the medium of the lymphatics.* He points out that the cases in which local accumulation of fibrine and an hyperinotic condition of the blood co-exist, are precisely in the inflammations of those organs which are richly supplied with lymphatics—as the lungs and the pleura; while, when the brain, for example, is inflamed, as there are few lymphatics, the fibrine of the blood is not increased.

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* Cellular Pathologie, p. 352.

> "I do not believe," says he, "that we are entitled to conclude that there is a greater tendency to fibrinous transudation when there is an excess of fibrine in the blood; much more, I should expect that in a patient who produces at a certain point very much fibrine-forming substance, much of it would pass from this point into the lymph and then into the blood. We can then consider the exudation in such cases as a surplus of the *in loco* produced fibrine, for the removal of which the lymphatic circulation does not suffice." (p. 149.)

The local production of fibrine is endeavoured to be elucidated by comparing it with, and approaching it to mucus. In certain inflammations, as in croup, the two substances are both in presence, and can be substituted for each other. At a certain point there is clearly mucus, at another fibrine, at a third a membrane which cannot be referred with certainty to one or the other.

> "Of mucus we know, however, that it does not pre-exist in blood as fibrine does. If, therefore, a mucous membrane produces unheard-of great masses of mucus in a short time, these are the products of the membrane itself; the membrane is not transfused with mucus from the blood, but the peculiar mucin mass, the mucus, is a product of the coat, which, through the fluid soaking through (transuding) from the blood, is brought to the surface. In the same way I have also sought, as I formerly explained, to reverse the opinion which used to be held on the origin of fibrine. While, till at present, the fibrine has been considered as the outpouring plasma, I have given the explanation that the fibrine is a local product of those tissues on which and in which it is found, and that it is brought to the surface in the same way as the mucus of the mucous coat. I have already shown you how it is in this way most easily conceived that in the measure, as in a certain tissue, the production of fibrine augments, so also the fibrine of the blood augments, and *that the fibrinous crasis is just as much a product of the local disease as the fibrinous exudation is the product of local tissue metamorphose.* Never has any one been in a position to produce fibrine through variations in the pressure of the blood; as little as mucus could be produced by pressure of blood in the places where mucus is not naturally produced; what soaks through are always the serous fluids alone."*

Thus, then, Virchow would explain the masses of fibrine in the pleura, or the exudation in the air cells in pneumonia. Fibrine is there, but it does not come out of the blood, it has been produced by the action of the membrane (mucous or serous) on the fluid which transuded from the blood; and as soon as formed, it is absorbed by the lymphatics and taken back to the blood, where it causes hyperinosis. Only certain organs can produce mucus, and only certain organs can produce fibrine. When inflammation occurs in an organ which cannot produce fibrine, the hyperinotic condition of the blood is not produced.

Thus Virchow looks upon the fibrine of inflammatory blood, at any rate, as an excremental product brought to the blood by the lymphatics, and in this respect he appears to approach nearly to the opinions so long advocated by Zimmermann.

In this new doctrine of inflammatory processes, it is impossible not to admit there is much to be said in support of some of the facts brought forward by Virchow. The growth of cells only from pre-existing cells, in pathology as in physiology, is an observation which has been confirmed by so many persons, that we feel it scarcely possible to refuse assent to its universality. And we may also admit that this growth occurs frequently in inflammations, though we may doubt whether it is constant and whether some of the so-called nekrobiotic processes, especially loss of cohesion and of formative activity, are not more often the immediate consequences of acute inflammation than increase of nuclei and cells. But it seems to us an hypothesis entirely unsusceptible of present proof, to refer this growth and formative activity to a mysterious attraction on the part of the cells, and we cannot see why, when Virchow admits the occurrence of transudation of fluid in some cases, he should refuse to believe its occurrence in others. Why should fluid transude through a mucous membrane and not also transude into and through the cells of a gland? And transuding through the cells of a solid organ, why may not this be the starting point of the swelling and growth of the cells, instead of some hypothetical condition of excitement and reaction in the cells?

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* Cellular Pathologie, pp. 350–1.

The hypothesis to explain the existence of fibrine on serous and mucous membranes, and the production of hyperinosis of the blood, appears to us to be at present quite insusceptible of demonstration, and we can only suppose that Virchow now brings it forward because the existence of fibrinous effusions, as the immediate and characteristic result of inflammations, imperatively demanded some explanation.* And to admit fibrinous exudation from the blood in some inflammations, and to deny it in other instances, would have been impossible. The thrusting so completely into the background so important phenomena of inflammation as hyperæmia, and the changes in the contractility of the vessels, also seems to us a weak point in Virchow's theory. We may concede that hyperæmia is non-essential, though on this point some doubt exists, but we cannot overlook the fact of its most frequent occurrence. To almost ignore its presence, and to slight its influences on the nutrition of a part, cannot be correct.

On the whole, indeed, we may say, that this new teaching seems to us one-sided; there are doubtless new and important facts in it, facts which increase our knowledge of inflammatory processes, and define more clearly the relation of these processes to normal nutrition, but it generalizes too exclusively from these facts, and is obliged to have recourse to some very bold hypotheses in order to construct a consistent theory. We cannot now, however, say more on the subject, as many other topics press on our attention and our space.

It may be inferred that the vast importance given to the *productive* phenomena of inflammation, would not be without effect on Virchow's views of the origin of NEW GROWTHS. This is indeed the case; and the mode of growth of tumours of almost all kinds is described as being very similar to that of pus. Tumours are asserted not to originate from blastema or exudation, but from continuous cell growth; and in the great majority of instances, the commencement of the tumour is to be found in changes in the corpuscles of the areolar tissue.

> "From the moment," says Virchow, "when I could affirm that there is almost no part of the body which does not possess cell-elements, when I could point out that bone corpuscles are actual cells, that the areolar tissue possesses, in various places, a sometimes greater, sometimes less amount of actual cell-elements, then were the germs given for (the doctrine of) the possible development of new tissues. In fact, the more the number of observers has increased, the more has it been more commonly deduced that the greater number of New Formations which arise in the body, proceed from the areolar tissue and its equivalents. Exceptions to this are the proportionably few pathological new formations which belong, on one side, to the epithelial formations, on the other, to the higher organized tissues, such as the vessels. *We can then, in fact, with little limitation, place the areolar tissue and its equivalents as the common germstock, instead of the former blastema, the later exudation, the original plastic lymph of the older writers.*"†

The mode of formation is described as being either by simple cleavage of nucleoli, nuclei, and cells, as already fully described under the head of pus, or by "endogenous" changes in the interior of pre-existing cells. Virchow, many years ago,‡ described this last mode of increase, and now briefly recapitulates his observations.

> "In a simple cell a space forms like a bladder, which in comparison with the somewhat

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* As Wunderlich observes (Archiv für Phys. Heilk., p. 297. 1859), how is the increase of fibrin in acute rheumatism to be accounted for? there are no masses of fibrin in the joints which can be absorbed by the lymphatics.

† Cellular Pathology, p. 355.

‡ Archiv für pathol. Anat., Band iii.

> cloudy and generally slightly granular space of the cell, has a very clear, transparent, homogeneous appearance. In what way these first kinds of space, which I class under the name of Physalides, arise, is not yet certain. . . . . Subsequently this bladder becomes so great as to almost entirely fill the cell, the old contents of which, with the nucleus, appear merely as a little appendage to the bladder. So far the process is tolerably simple. Only beside these bladders thus growing and filling the cells, we can see other forms, which contain enclosed cell-elements in the interior of the bladders; this is tolerably frequent in cancers, but is seen also in normal parts, for example, in the thymus. This form appears to indicate that in fact, not directly by division of pre-existing cells, but in particular bladder-like spaces, which I have named Brood-spaces (*Brutrāume*), occurring in the interior of cells, new elements of similar kind can develop."*

This mode of increase is stated, however, to be infrequent; the cell-cleavage is, in all forms, the most common manner of growth.

As in inflammation the cells are supposed to be irritated, to attract nutritive material, and to evince an increase of nutritive and formative activity, so in the case of tumours the process is identical. The homogeneous and the benignant, as well as the heterologous and malignant tumours, have a common origin. Heterology consists, in fact, only in a formation occurring in a place, or at a time, or in a degree, which is unnatural; conditions which Virchow denotes by the terms Heterotopie, Heterochronie, and Heterométrie.

At the conclusion of the essay before quoted, Professor Weber refers to this subject in terms which appear to us to express Virchow's opinions well and concisely.

> "The importance of the areolar-tissue corpuscles," writes Weber, "which we saw play so great a part in the development of pus, extends much further, and is evidenced in a crowd of other new formations, particularly in growths. . . . . This is especially apparent in the development of lipoma, in which, though not in all cases with equal clearness, we can follow the gradual development of areolar-tissue cells to fat cells. According to my observations, the fat appears at once in drops, and not in fat molecules, as in the fatty metamorphosis. Also in teleangieklasia, the areolar-tisssue corpuscles determine, at least in part, the formation of new vessels through development and gradual widening of their anastomoses, in the same way as similar processes have been already described by Schwann. In fibroid, the nuclei of the capillaries play a part in the development of the fibrous tissue; and there is chiefly an increase through cleavage which leads to the cell-formation. This occurs again in the sarcoma or fibro-plastic tumours. . . . The osteo-sarcoma develops itself in similar wise, as I have shown in enchondroma, principally through proliferation of the areolar-tissue corpuscles of the periosteum and the medullary spaces; yet there appears to occur here also metamorphosis and increase of the osseous corpuscles. The same holds true of the myeloid tumours, which are only distinguished from the sarcomatous tumours by the occurrence of the great myeloid cells, which also arise from the areolar-tissue cells. Even so is it with scirrhus and encephaloid; at least, I have seen the development of these from the areolar tissue, as well in the neurilemma and sarcolemma, as also from the so-called muscle-nuclei themselves, in several scirrhous and encephaloid tumours. . . . . The most interesting transformation is that which the areolar tissue cells exhibit in the origin of epithelial cancer or cancroid. . . . This can be particularly finely followed in cancroid of the lip; and here the areolar-tissue running among the muscle, gives the best opportunity for observation. I have figured the gradual transformation of the areolar-tissue-corpuscles from cleavage, and also sometimes from endogenous generation, to great heaps of cells with epithelial character, and with an acinose arrangement. From the pressure of these the muscular substance is gradually destroyed, just as in suppuration. In the same way occurs the development of epithelial cancer in the bones; and I have now before me plates, delineated in the year 1855, which show this development out of the areolar tissue of the Hāversian canals. . . .
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> "We see, therefore, that in most new formations the areolar-tissue cells take the part of brood-places. Why, in the one case, the product should be a rapidly evanescent one—as the pus corpuscles; in another, a spindle-formed cell, or a like-figured nucleus—as in the fibroid and sarcoma; in the third, a round many-nucleated cell or large pale clear nuclei—as in scirrhus and encephaloid; or, lastly, in another case, a cell of epithelial characters, with proportionably thick consistent cell wall; *that is a question that we are not at present in a condition to decide.*"†

Billroth, in his last interesting work, has arrived at opinions nearly similar. At the end of his essay on tumours he concludes:

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* Cellular Pathologie, p. 359.

† Archiv für path. Anat., Band xv. p. 525. 1859.

> "That the source of all cell formation excited by pathological processes (whether the cell-formation leads to the formation of solid or fluid tissues), is found principally in the areolar tissue, and indeed in the cells and nuclei of these; and the nuclei and cells of the other tissues partake of this either not at all, or in a slight and secondary measure."*

And in another place, Billroth remarks on the similarity and dissimilarity of chronic inflammation and formation of tumours, in these terms:

> "However close in many relations chronic inflammation and tumour-formation stand, they are clinically easily distinguished; for chronic inflammation at last becomes extinct, the cell-formation ceases, retrocession occurs, and the process can so heal. But in tumour-formation the cell and tissue formation is unlimited, it goes on relatively *ad infinitum;* here and there local retrocessions occur, yet the process never perfectly heals, unless the tumour is thrown off by gangrene."†

It is apparent that these opinions are in fact a wide generalization, approaching to each other a great number of formations, and simplifying exceedingly our mode of regarding them. But what, then, our readers may say, becomes of malignancy and benignancy as characters of tumours? Why, at present Virchow is disposed to look upon malignant tumours as possessing a kind of contagious property; that is to say, in the neighbourhood of a malignant tumour the cells are irritated (perhaps by the passage into them of the fluid formed by the diseased cells, percolating into them, or attracted by them, and then taking on the same formative activity). And indeed Van der Kolk long ago showed for how great a distance round a cancer the germs of the disease were spread, so that tissues apparently healthy to the eye were in reality profoundly diseased. In favour of Virchow's view is the fact that the tumours with most juice are most malignant; and dry, fluid-free tumours are generally harmless. A cancer then grows in this way, and also by conveyance of its irritating fluid or corpuscular germs to a distance by lymphatics and veins (in some cases).

As an almost necessary consequence of these opinions, Virchow entirely disowns the doctrine of the constitutional origin of tumours. He does not believe in a mysterious general cancerous dyscrasia, for example, which manifests itself by the formation first of one and then of another tumour. No, first of all is supposed to occur in all these cases the local affection (i.e., irritation of a cell, increased attraction of healthy plasma, increased growth), and then propagation from infection or from transportation of the fluid and germs, and contamination of the blood by the absorption of products from the local lesion. The dyscrasia, or constitutional disease, is in all cases, according to Virchow, entirely secondary. "Every dyscrasia is dependent on a permanent supply from certain parts of hurtful ingredients." (p. 119.) And this is the case with tubercle‡ as with cancer, with scurvy as with leukæmia.

We have now, as far as possible in Virchow's own words, given an outline of his cellular theory, and if we have been successful in our description, it will be seen that all parts of this theory hang well together. The common origin of every growth, of every cell, is a pre-existing cell, and originating in this common way, we are able to connect physiological and pathological growth, and to more or less clearly explain where and why several processes diverge. As so great importance is given to the cells, and as both vessels and nerves are ranked as quite secondary agents in the phenomena of inflammation of the formation of pus, and of the growth of tumours, we recognise the propriety of the term Cellular Pathology.

If it shall turn out that these views are supported, there is no doubt that Virchow has made a vast step forward, and that a very simple and grand expression of morbid phenomena has been given. But many of the statements are not only novel, but are based on propositions still greatly controverted, as, for example, the existence of areolar-tissue corpuscles. Other statements evidently embrace only part of a subject. If, for example, there are no general dyscratic diseases, in the usual sense of the term, but if there is in every case a local origin, what is the local origin of small-pox, which sometimes kills before a pustule is seen? or of the other diseases of the class which begin with general symptoms, and then at a certain stage show local phenomena in a particular part. Again, in the case of cancer, is there really nothing in all those arguments which only a few years ago were supposed to prove indubitably that every local manifestation in cancer is but the topical sign of a general constitutional state? And are the opinions on phthisis to undergo so radical a change that we are to look upon it entirely as a local disease, until by continued absorption of hurtful substances the blood and other parts are contaminated? Certainly not without grave consideration ought we to abandon views formed after full inquiry and no little careful experience. In medicine, as in all other things, we must give way to facts, but we are not called on at once to admit the truth of opinions which go against our accepted doctrines.

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* Beitrage zur path. Histol., p. 79. 1858. † Billroth, p. 65.

‡ For Virchow's opinions on tubercle, which we have not space to describe here, we may refer to Dr. Jenner's review in this Journal, in Jan. 1858.

Now, Virchow's statements of the local origin of dyscrasia are given so briefly and with so little or no reference to clinical facts, that they can scarcely be looked on as more than hypotheses. That there is truth in the local production and absorption of hurtful substances, from chronic inflammation and from many tumours, we have no doubt. *That* is a doctrine by no means new, but that it expresses all the phenomena of cancer, for example, is most certainly "not proven."

And as in the doctrine of inflammation we seemed to see that with much real truth Virchow has mixed up hypothetical explanation, and to make his view consistent has thrown into the background such important phenomena of inflammation as hyperæmia and nerve-implication, so in his statements on tumour it seems to us that he has been too anxious to break entirely with old doctrines, and to look at everything from the point of view which he has chosen.

We have hitherto selected for comments those parts of this book which seem to us most essential and fundamental in the doctrine of the "cellular pathology." But the work is not limited to these subjects—it glances at the whole of pathology, and discusses with great ingenuity almost all morbid processes. We have not seen in any of Virchow's writings so good a description of Embolie, Thrombus, Pyæmia, Melanæmia, Leukæmia, Rickets, Atheroma, &c. So also the histological account of the tissues is extremely simple and good. We could have wished to have discussed many of these subjects, but this article is already too long, and we can only hope that our readers will study the work for themselves. We can assure them they will have some real enjoyment, and when they disagree, will disagree with respect and admiration for the author.

Review II.

1. *Lettsomian Lectures on Syphilis.* By Victor De Meric, Esq., Surgeon to the Royal Free Hospital, and to the German Hospital, Dalston.—1858. pp. 68.
2. *Lectures on Chancre.* Delivered by M. Ricord, Surgeon to the Hôpital du Midi; published by Fournier, with Notes and Cases; and translated from the French by C. F. Maunder, Demonstrator of Anatomy at Guy's Hospital.—1859. pp. 244.
3. *A Treatise on Syphilis in New-born Children and Infants at the Breast.* By R. Diday, Surgeon to the Hôpital de l'Antiquaille, Lyons; translated by G. Whitley, M.D. (The New Sydenham Society.)—*London*, 1859. pp. 272.

How can we explain the discordance of opinion which exists on the subject of syphilis! Is the disease as seen in the London hospitals different from that which prevails in France? Do modern syphilographers understand in a peculiar sense the terms hardness and softness? Are constitutional symptoms influenced by national habits or temperature? We confess to a feeling of surprise at reading controversial works, and listening to dogmata which unite only in one point of view—namely, in putting the whole subject in lights which it never enjoyed before. But from this war of words a theory has sprung which, if true, simplifies—but, if untrue, tends to confuse; and it is to this subject that we shall in the first place chiefly confine our attention: we mean the teachings of that school of which M. Ricord is the founder and head.

A word upon the origin of the disease:

> "It is perfectly certain," says M. de Meric, "that previous to the general spread of the disease in 1493–95, no ancient or mediæval writer had given a description of the complaint, as seen and depicted by Joseph Grundbeck, Benedictus, and Leonicenus, who wrote in 1493–95; these authors being the first who published an account of the malady." (p. 3.)

No one, we add, is likely to witness the malady as Benedict has depicted it.

> "I knew," wrote this trustworthy and learned man, "some honest and religious nuns, confined in the strictest manner, who unfortunately contracted the venereal disease from the peculiar state of the air, together with that of the putrid humours and the weakness of their habits of body."*

From the profundity of the wisdom of the ancients, it was supposed that infection might be communicated by the use of holy water contaminated by a person suffering from the disease; that a "young lady of the period" might catch the pox by being sprinkled with the "asperges" at the church-door. "But," quietly sneered Fallopius, "the disease must have been contracted *per unum 'asperges' ego scio.*"†

Heretofore we were content with squabbling as to whether the venereal disease first broke out at the siege of Naples, or whether it was imported with other western produce by Christopher Columbus from the New World. The vulgar view of the case, that whenever the laws of nature are offended, as by one female receiving the embraces of a number of men, disease is as sure to ensue as that typhus fever will follow overcrowding, is generally rejected as unscientific; and we plunge accordingly into the obscure legends of the past.

But of all stupendous assertions, unsupported by any argument or facts, is one of Van Helmont, revived by M. Ricord, which throws common mediæval myth into the shade—namely, that syphilis is a modification of glanders, transmitted from the horse to man.

> "Now it was not known," says M. De Meric, "at the period alluded to, that this latter disease was communicable from the horse to man; and it has since been suspected that some of the loose female characters, already labouring under simple ulceration, and hovering round armies, might have been infected by an individual suffering from glanders, or having about him some infectious secretion originating from a diseased horse. A mixture of the poison of glanders with the unhealthy and decomposing secretions of dissolute females, may have given rise to the dreadful malady which has for centuries undermined the health of generation after generation. Proof there is none; but a tolerable share of probability."‡

We have thus, by way of simplifying the question of the origin of the disease, a portal opened to the whole range of veterinary surgery, in addition to that of archæological research; and we see no reason why speculation should not likewise be extended to the probable effects of the introduction of canine and bovine maladies into the human race, and thus avail ourselves of additional disquisitions calculated to afford delectation to those curious in startling assertions.

But let us pass from this unprofitable subject. Cause and effects obey in the history of disease one fixed and immutable law; and we find that when ancient writers confine themselves to describing facts as they saw them, the phenomena which they record differ in no essential manner from those witnessed under similar circumstances in the present day.

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* Benedict V.: Tract. de Morbo Gallico, cap. 8.

† Ibid. cap. 18.

‡ De Meric: Lettsomian Lectures, p. 4.

Now we read a great deal about the enormous host before Naples, assembled by Charles VIII., King of France; of the length of the siege; of the many causes of disease generated in the camp. What are the facts? That monarch marched into Italy in the month of August, 1494; he had to make his way through Etruria and the Papal States; and he marched out again in October, 1495, leaving behind the Duke de Montpensier, with six thousand men. Why, the whole campaign lasted but little more than one year! It was carried on in a cultivated country, and during the greater part of the time the troops must have been in movement, and therefore more healthily employed than in lying listlessly before a beleaguered place. How comes it that if the tremendous malady of which Gilinus and Leonicenus and others wrote, sprang from this "puddle in a storm," similar results have not been witnessed in the vast assemblages which have characterized the warfare of modern times?

Have M. Ricord or M. Victor De Meric ever seen a case of glanders in man? It bears as much resemblance to the venereal disease as it does to the mange in dogs. Let us at once refer the origin of the disease, with Coradinus Gilinus, to the junction of Saturn and Mars, Jan. 16th, 1496; or that of Jupiter and Mars, Nov. 17th, 1494; or revive the theory of the learned Wendelinus Hock de Brackenaw, who refers this calamity to the year of God, 1483, because in the month of October of that era there were four planets in Libra—namely, Jupiter, Mars, the Sun, and Mercury. Such statements are quite as satisfactory, and infinitely more clean, than Van Helmont's vision of intercourse with an infected mare.

M. De Meric commences his work with modesty:

> "I may not," he says, "on the present occasion, follow the ravages of the disease through the length and breadth of the world, trace its effects in different climates, describe its decrease and occasional exacerbations; compare it with the sibbens of Scotland, the yaws of the West Indies, and the black disease of Canada; but I shall just stop to inquire what aspect syphilis presents in this country in our own refined state of society." (p. 5.)

He then proceeds to speak of the rather large proportion of chancres which become phagedænic; of gangrene of the prepuce and glans; of intractable secondary or tertiary ulcers; of carious skulls, which now, as of yore, lead the sufferer to the grave. He speaks, too, of "our fair delinquents, once adorned by freshness and youth, turned into objects of disgust and commiseration."

> "We all know," he adds, "that a man may have had a chancre and a suppurating bubo, and remain for the rest of his life perfectly free from any taint whatever. Such an individual should not be said to have suffered from syphilis; his frame is not infected, he has simply had a venereal ulceration, followed by a glandular complication. The words chancre and syphilis might, then, advantageously be reserved for the more serious complaint; and it would at once be plain that syphilis is to be classed with other blood-diseases, as cancer and scrofula." (p. 6.)

We will not here enter into a disquisition upon the meaning of blood-disease; nor stop to inquire how far cancer, or even scrofula, may be the better understood upon such a basis. Who has detected morbid changes in the blood of those suffering from either malady? And wherefore is it that the life-endowed cells, which end in the development of the tissues, may not have more to do with the development of malignant disease, than the composition of the circulating fluid? The cancer or scrofula which comes from our parents cannot be eliminated by bleeding, nor cured by transfusion; nor can the disease be transmitted by inoculation. We will, then, drop the term blood-disease, as in this instance too vague for useful investigation.

Mr. De Meric looks down upon some practitioners who believe that "the poison of syphilis may be generated spontaneously; that frequent promiscuous intercourse, coupled with uncleanly habits, may cause the development of syphilitic ulcerations." We will ask the author whether it be not true that the poison of typhus is generated spontaneously in crowded habitations? that the poison of typhoid fever comes spontaneously from decomposing fæcal matter? And may not the same be said of every other morbid yet non-tangible influence which acts injuriously on the human frame; we know it not, save by its effects. Just as promiscuous intercourse develops syphilis, so does overcrowding induce typhus, or bad drainage develop typhoid fever; we have no need to descend for explanation to the pathology of quadrupeds.

Now, nobody denies that there is a syphilitic poison. MM. Ricord and De Meric may therefore spare both argument and illustration in proof of what is known. But when we proceed to the next step: "Nor does the analogy between syphilis and other contagious diseases stop here, for it is extremely probable that the organism is insusceptible of two syphilitic infections,"* we must request the reader to suspend his belief. And before entering upon the point, we will inquire into the nature of primary syphilitic ulcers.

M. Ricord asserts that there are two forms of chancre—one the *chancre mou*, the simple or non-infecting chancre; the other the indurated or infecting chancre. He proceeds to explain that the excess of relative frequency of the simple chancre may be explained as follows:—1. The simple chancre is the most abundant source of the chancrous virus; it is the form of ulceration which secretes pus endowed to the highest extent with the property of contagion, and which retains for a long period the faculty of inoculation. 2. It creates no immunity against a renewed contagion of chancre of the same kind—that is to say, against its reproduction upon the same individual. The indurated chancre is only produced once upon the same subject. To the natural inquiry, are these statements true as regards English experience, we answer unhesitatingly in the negative; they are contradicted by daily evidence of hospitals wherein syphilitic disease is witnessed on a large scale.

In the first place, can chancres be recognised in this primary form as infecting and non-infecting? We entertain very strong doubts whether M. Ricord himself would encounter a heavy responsibility in determining this point. He might succeed at a guess; but he could not lay down clear and unmistakeable rules. What are his own words upon the point?

> "You will remark, that in those places where the induration is most exuberant, it also persists longer than in any other region. On the other hand (and I hasten to point out this fact to you now, in order that you may see the contrast), there *are certain anatomical regions where* indurations are ill-defined, in which their presence is with difficulty recognised, and from whence they rapidly disappear."†

How convenient! The infecting chancre must have a hardened base. If the hardness cannot be felt by the uninitiated, it is due to inexperience. If it cannot be felt by the experienced, it has disappeared, having been short-lived!

If constitutional symptoms appear after a proper interval, the indurated base must have been there; perhaps parchment-like. "Induration," says M. Ricord, "so difficult to recognise in deep-seated regions, is but a transitory symptom. When once produced, a few days will suffice to allow it to disappear."‡ We cannot pretend to meet this circular sort of argument, which may be thus expressed:—No variety of sore, except that with the indurated base, will produce secondary or constitutional syphilis. If a patient suffer constitutionally, the primary sore must have been indurated. If the surgeon have no remembrance of "induration," he must have overlooked it. If the sore, to appearance, be soft throughout, there must have been a transitory induration; subsiding in a few hours. We ask what species of argument is this? Is it not akin to the Hunterian doctrine, that all syphilitic sores require the use of mercury? If a suspicious-looking sore get well by simple means, it cannot be syphilitic? The experience of London hospitals does not support such assertions; on the contrary, it gives them daily refutation.

The character and appearance of the primary sore are chiefly modified, as Mr. Lawrence has for many years pointed out, and as has been shown again in a modern work (Coote on the 'Treatment of Syphilis'), by the *tissue* to which the poison is applied. There are distinct and recognisable varieties of sore on the glans penis; the orifice of the prepuce, the integument of the penis; the integument of the body. Induration at the base occurs chiefly at the reflexion of the prepuce on the glans; but may, in rarer instances, be seen in other situations. An indurated chancre is very rare indeed in the female. Sores are sometimes superficial for a few weeks, and then become indurated at the base just as the process of cicatrisation is complete; but the induration is never transitory. Now, M. Ricord, in commenting on the fact that of two primary sores possessing the same faculty of physiological or artificial reproduction, one produced contamination of the organism, the other left the organism unscathed, inferred that the virus was the same, but that some constitutions resisted and others not. But, in 1852, M. Bassereau, one of his pupils, published a work on syphilitic eruptions, wherein he endeavoured to prove that differences as to constitution, mode of life, sex, age, and climate, had nothing to do with the development of constitutional syphilis or its non-appearance. From a large number of cases collected at the Paris Lock Hospital, he took one hundred, who, after chancre, had suffered constitutionally; and an equal number whose chancres were not followed by secondaries, and who had no treatment capable of warding off systemic syphilis, and found in these two series of cases the same proportion of lymphatic temperament, weak frames, bad hygiène. He took into particular account the fact that the same person may, at short intervals, take first a soft, then a hard chancre; and also that patients have been known to present soft chancre successively through a certain number of years, and to contract at last an infecting sore, which contaminated the economy.* Finally, by confronting the person infected with the individual who had been the cause of the contagion, he found that all those patients who presented chancres followed by constitutional symptoms, had taken the disease from persons affected in exactly the same manner. He likewise found that individuals suffering from chancres which had remained local symptoms, and had not affected the economy, had been diseased by persons with whom the chancres had also left the organism untouched.

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* De Meric, p. 7.

† Maunder's translation of Ricord's Lectures, p. 62.

‡ Lectures on Syphilis, by V. De Meric, p. 20.

From these observations M. Bassereau has founded his theory, to which M. Ricord has yielded a willing assent—namely, that the cause of this difference lies in the poison itself.

> "The chancre which taints the frame appeared only at the epidemic of 1495, and from that period were those fearful constitutional symptoms observed which are rife up to the present moment. The soft chancre, M. Bassereau believes, is as old as the world; the hard, with its dismal train, began its reign, and that in the most ruthless way, towards the close of the fifteenth century."†

We admit that temperament exerts a dubious sway over the development of secondary syphilis. But not so sex or climate. Out of 200 males admitted into the venereal wards of St. Bartholomew's Hospital, 92 had constitutional symptoms. Out of 200 females, only 54 were similarly affected. Climate, too, exerts a most marked influence when combined with proper diet and regularity of habits. The convicts at Gibraltar are reported by Mr. Nesbit, one of the Government surgeons, to be rarely sufferers from systemic disease, although most of them have primary syphilis in one form or another at the time of conviction and banishment. But it will be replied, there is no direct evidence that any of these persons were suffering from an indurated chancre, or that exact form of ulcer to which M. Bassereau refers. To this we can make no reply, except that surely among so many who are sufferers, there must have been some the subject of the infecting chancre. But in addition to this, secondary symptoms already existing slowly subside under the influence of proper hygienic rules, and do not recur.

We positively deny the statement that the indurated chancre, as understood in England, is the only infecting sore. It is of course difficult to demonstrate the point, because of the shifting nature of the ground taken by those who support the opposite doctrine, and affirm that the induration may be transitory, or insensible, or involved in some complication. The following case is, however, to the point: In 1843 a young lady left an evening party clandestinely in company with a gentleman and never returned to her family. After a few months she was left to her own resources, when she contracted disease. There was a small sore at the entrance of the vagina, quite superficial, which healed under the care of an experienced surgeon, without any specific treatment. About one month afterwards she suffered from feverish symptoms, and soon after a mottled eruption came out over the head, chest, and extremities (lepra syphilitica), then ensued ulceration of the fauces and iritis. Six months afterwards she aborted. Her subsequent career was such as might have been anticipated; she died four years after of phthisis brought on by intemperance; but she was supported by a gentleman who had been a former friend, and who lived with her up to the date of her death. It is remarkable that he never was infected. The answer of the Ricord school to this case may be anticipated; *the induration was overlooked, in the female such an accident being highly probable.*

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* Op. cit. p. 63.

† Lectures on Syphilis, by V. De Meric, p. 21.

A man, aged thirty-five, was admitted from the out-patient department into St. Bartholomew's Hospital, January 30th, 1846, having contracted venereal disease a month previously. He said that a small sore first showed itself on the left side of the penis, behind the glans, and rapidly increased in size. Upon his admission there was seen a large black circular sloughing sore on the left side of the body of the penis behind the corona glandis, and penetrating the reflected lining of the prepuce; a large spreading phagedænic sore (secondary or inoculated) upon the opposed surface of the prepuce, a large oval phagedænic sore upon the inner surface of the right side of the prepuce, the long diameter about the width of a shilling. The prepuce was swelled, but not in a state of paraphymosis. The prepuce was divided; proper measures were adopted, namely, nutritious diet and soothing applications. On February 3rd the slough had separated from the first sore; but the phagedænic ulcer on the right side of the prepuce was extending. On February 5th, hyd. cum creta was ordered, but given up on the 10th, the sores appearing to become fouler in aspect and deeper. Finally, they healed by simple treatment, and the man was discharged March 11th. Was this an *indurated* sore? If so, our ideas of hardness—of the Hunterian chancre—must undergo a revolution. The ulcer was phagedænic, and the hardened base was absent. Now, March 24th he returned to the hospital with rupial spots over the body, and indurated swellings on the cicatrices of the prepuce, and painful periosteal swelling of the right leg. He left the hospital April 7th: he was re-admitted June 2nd, with a foul ulcer the size of a split-pea at the base of the glans penis behind and to the right of the frenum; and a phagedænic ulcer the size of a half-crown upon the right fore-arm. He says that he had had connexion with a *healthy* female ten days previous, having been in perfect health up to that time. Since that period we have lost sight of him.

Mr. Lawrence, who had the charge of this case, examined the female with whom this man had had connexion; there was no trace of syphilis, and she had never had disease. The case is reported in the 'Medical Gazette.' In 1841 a medical man consulted the same surgeon for a thickened and elevated knot, the size of a sixpence, on the dorsum of the tongue, and superficial ulceration of the mucous membrane of the velum palati and fauces. He had consulted many practitioners, most of whom considered the affection to be due to gastric disturbance. Upon inquiry, it was found that eight months ago this gentleman had suffered from a slight *excoriation* of the prepuce, which had healed without special treatment in about four days. There had been no induration. It would be idle to go on multiplying these cases. They represent the daily experience; they seem, in conjunction with others, to confirm the opinion that the character of the primary sore, all varieties of which proceed from one virus, is chiefly influenced by *structure;* that, as in the case of phagedæna, constitutional peculiarities exert an occasional and considerable influence; and finally, that the manifestations of constitutional syphilis are by no means limited to one kind of chancre.

M. Clerc, a pupil of Ricord, is a supporter of the duality of the syphilitic poison; but he considers that both kinds are posterior to the celebrated epidemic, the soft being a degeneration of the hard. But a fatal objection, says Mr. De Meric, to this view is, that this *chancroid*, when communicated from an already syphilitic individual to an untainted person, will develop upon the latter in the shape not of a soft but of a hard chancre, and infect the economy.

"Struck by the logical manner in which M. Bassereau had conducted his investigations,

> and by the value of his clinical facts, several specialists of France undertook to control the new doctrine by the same system of confrontation which its author had adopted. The latter had succeeded seventy-five times out of seventy-eight confrontations in showing that constitutional syphilis had existed both in the infecting person and the individual infected. M. Clerc, of Paris, was just as fortunate in seven cases. M. Diday, a distinguished specialist, M. Rodet, and M. Rollet, all successively surgeons of the Lock Hospital of Lyons, have instituted similar confrontations, and published numerous cases, all corroborating the doctrine of duality."*

For these observations the police regulations of France offer peculiar facilities, and the inferences and results are calculated to attract a considerable amount of attention. But M. De Meric allows the numerous sources of fallacy:

> "Among the number, I may mention deceit or unwillingness on the part of patients; promiscuous intercourse in a limited interval; mistakes as to whether the professed or the concealed harlot has been the source of the disease; chancres which escape the notice of the patients; a soft chancre, springing in the cicatrix of an old indurated one; a soft chancre observed in a person previously affected with lues, which chancre may implant actual syphilis in an individual who had never been diseased, &c., &c. These are certainly great obstacles in the way of obtaining reliable cases of confrontation." (Op. cit., p. 23.)

The confrontations directed by M. Ricord, and carried out by M. Fournier, may be divided into the following groups:

1. Transmission of simple chancre with its original aspects from one untainted subject to another, also previously untainted.
2. Transmission of simple chancre, with its original aspect, from an untainted subject to one already infected with syphilis.
3. Transmission of soft chancre affecting syphilitic individuals with its original characters, and not producing constitutional symptoms upon the recipient.
4. Transmission of soft chancre affecting syphilitic individuals in the shape of hard chancre, followed by systemic syphilis.
5. Transmission of hard chancre upon previously untainted subjects.
6. Transmission of hard chancres upon individuals already infected with syphilis in the shape of soft chancres, and presenting all the characters of the latter.

No absolutely conclusive results have yet been obtained from this inquiry, which must be continued over a space of many years. We must, however, bear in mind the many sources of fallacy, among which none seems to strike us as more fertile in error than the protean forms which, according to Ricord, Bassereau, De Meric, and others, the infecting or indurated chancre may assume. "Soft chancres," says M. Ricord, "are rarely or never found in the cephalic region." About the head or face the chancres are always of the hard kind. We disagree with this assertion, but hail it as an indication, though but a faint one, of the recognition by M. Ricord of the influence of texture upon the shape of a sore.

The necessity of clearly knowing what M. Ricord understands by the term "infecting chancre" is so obvious that, at the risk of being thought tedious, we will follow him through his own very copious description:

> "With the indurated chancre," he says, "we enter the domain of syphilis. Man alone is subject to it, although it owes its parentage to the horse. It may manifest itself on any part of the body, from head to foot, mucous membrane as well as skin. I have seen it," says M. Ricord, "on the lips, on the tongue, on the conjunctiva, on the pituitary membrane, on the neck of the uterus, on the vagina, at the anus, in the rectum."†

It develops itself in a slow and insidious way; a certain time is necessary in order that the inoculating pus may, so to say, *prepare the ground* for its development. This supposed period is called a period of *inobservation.* The ulceration is essentially indolent; it establishes itself, and spreads without pain. The surface is smoother and less worm-eaten than that of the simple chancre; its edges are smooth and shining, as if varnished, while the centre is of a darker and uniform grey tint. The soft chancre is, as it were, struck out with a punch; the indurated chancre is made, as it were, with a gouge; the base is indurated. Now, the chancres called simple are so varied in character that we think the last test—namely, that of induration—is the only safe point of difference to be relied on. The induration, says Ricord, is exactly and abruptly circumscribed at the circumference of the base of the chancre: as Hunter has observed: it does not affect the adjacent tissues, which retain their habitual suppleness. It originates, *à froid*, without any inflammatory radiation. Page 58 (op. cit.) is devoted to show, what we imagined acknowledged —namely, that the induration is not of inflammatory origin. There are varieties in the form of induration which owe their origin to the *locality* in which the induration is seated. At times it is well defined on the edges of the ulcer, but almost entirely wanting in the central part, thus forming a kind of ring. In other instances, instead of sinking into the tissues, it remains superficial, and resembles a sheet of parchment. "*It is requisite to know how to touch and feel before one can perceive it.*" (Op. cit., p. 59.) It never precedes the ulceration, and generally developes itself at the end of one week after the infecting coitus. It never manifests itself earlier than the third day. It is far less apparent in some situations than in others. In deep-seated situations it is but a transitory symptom; it may disappear in a few days; it often disappears before the work of reparation is finished, before cicatrization is complete.

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* Lettsomian Lectures, by V. De Meric, p. 22.

† Maunder's translation of Ricord's Lectures, p. 54.

Allow us to inquire of what value is induration of such uncertain, transitory and variable character as a diagnostic mark in those observations of confrontation to which we have referred? Mr. De Meric relates a case of transmission of soft chancre where, in his own experience, the lady accused submitted to examination. She had in the cervix the identical kind of chancre which the patient presented in the corona. What proof have we that M. Ricord's three or four days of parchment-like induration had not existed and passed away? We do not say it was so, or even likely to be so; but we say that, upon the definitions of the French school itself, such cases are unsatisfactory. It is very probable that sores on the cervix uteri, when superficial, should resemble superficial sores on the glans penis; the structures are not very dissimilar. The indurated chancre is generally solitary; but M. Ricord speaks of a case in which one patient had no less than nineteen! Let us now inquire what M. Ricord understands by his indurated chancre:

> "There were," says he, "in this patient, Louis M., aged nineteen—1. Chancre parcheminée on the glans; superficial ulceration. 2. Indurated chancre on the corona; superficial ulceration, cartilaginous induration, semi-spherical shape, plunging deeply into the subjacent tissues. 3. Six chancres on the foreskin, two on its mucous, four on its cutaneous surface: all these chancres distinctly presenting the characters of the *parcheminée* induration, easily recognisable. 4. On the integuments covering the penis, six similar chancres parcheminées, form ecthymatous. 5. On the interior surface of the scrotum, and especially on the left side, five chancres with indurated base." (Op. cit., p. 66.)

Here we have a solution of many apparent discrepancies. This case reconciles many opposite opinions, for it proves one important fact—that on this side of the Channel we read precisely the same facts in a different way. In this case the patient had, according to the English school—1. Superficial ulceration of the glans; 2. An indurated chancre behind the glans; 3. Some non-indurated chancres, probably inoculated, on the foreskin; 4. Some non-indurated chancres on the integument of the penis; 5. Similar sores on the scrotum. Possibly from inattention this patient had allowed the infecting discharge to spread and inoculate adjacent parts. The sores presented all the usual varieties of primary syphilis in the male; and, on the acknowledgment that they were infecting chancres, M. Ricord once again puts the whole question in its original light—namely, that one poison may produce all the varieties of primary syphilitic ulceration, modified by texture, and that all such sores are capable of infecting the system. But if such be the views of M. Ricord, De Meric, and others, of what value can be the formal accounts of "confrontation?" It must rest upon this single fact: Is there any sore following impure intercourse, however superficial, which may not be followed by secondary symptoms? To this we reply that we know of none such: the most superficial abrasion, the minutest pustule, *may* give rise to the systemic infection; and although we cannot lay down the rule by which one person suffers and others escape, yet by impressing caution we save our patients from the misery and humiliation which may result from their acting with overweening confidence. The induration recognised and described by Hunter is a fact; it can be felt; and the button-like mass, when situated in the prepuce, can be seen "rolling over," incompressibly hard, as the covering of the glans is withdrawn. It is slow to form and slow to disappear; and although the chances of systemic infection are greater when the subcutaneous texture is thus diseased, yet cases are by no means uncommon in which a patient, properly treated, escapes constitutional symptoms. The transitory variety of the indurated chancre, the parchment-like base, either annular or continuous, appear to us as myths, suggested by an ingenious mind to meet opposing difficulties to a favourite theory.

The infecting chancre produces, according to M. Ricord, "a bubo of peculiar nature." In general it coincides with the induration of the chancre or follows it immediately.* It is also produced during the course of the first or second week, rarely manifesting itself later than this period. The glans have the appearance of a series of small tumours, of an ovoid shape, extremely hard, independent of each other, and moveable in the parts which surround them. The hardness is as the indurated base of the infecting chancre. There is no tendency to suppuration. The adenitis peculiar to the simple chancre has no fixed period for its development, and suppurates freely.

Here we again suppose that we have some arguments tangible to deal with; but no—the loophole is again opened. "The bubo resulting from the infecting chancre does not suppurate; but it may be *complicated with glandulur suppurations*, deriving their origin from a cause extraneous to syphilis; there may be double contagions." A patient, according to M. Ricord, may have contracted new chancres on old indurations; or an infecting chancre may become contaminated, at a variable period of its existence, by the pus of a simple chancre, and the ulceration assuming then the characters of the simple chancre, exercises on the glands the influence peculiar to this variety. If we do not bear in mind the possibility of this double contagion, we are liable to fall into an inevitable confusion, attributing to the indurated chancre what is produced by the simple chancre, and *vice versâ.*

> "It is necessary to bear in mind that the symptomatic bubo of the infecting chancre never suppurates by itself, and without the intervention of an extraneous exciting cause. Do you require a convincing proof? Make a trial of the pus which it furnishes in the rare instances in which it arrives at suppuration, and you will never obtain by artificial inoculation the characteristic pustule of chancre." (Op. cit. p. 84.)

Let us apply these rules, with their exceptions, to practice. Let us imagine that a young girl on the streets contracts an ulcer of the external organs, followed by a suppurating bubo. The immediate presumption is, that the ulcer is simple and non-infecting. But, suppose that the ulcer has an indurated base, we may either infer that the induration is inflammatory, or that the inoculation of a simple chancre has supervened on an indurated and infecting chancre; or that the infecting chancre has been followed by a bubo, which has excited a common inflammatory attack among the inguinal absorbent glands. Can inoculation, an experiment by no means free from risk, determine these points? We deny it. The evidence would still be inconclusive if the observations were conducted on persons of both sexes placed under the strictest supervision and watched for any length of time. We repeat, however, that there is scarcely an English surgeon who could not bring forward cases to prove that a soft chancre, followed by a suppurating bubo, may be complicated by constitutional syphilis, and we have reason to believe that the same views are generally gaining ground in many of the large schools of surgery in Germany.

As regards constitutional syphilis, we do not in experience find that regular succession of symptoms of which M. Ricord speaks. The disease does not in all cases pass through the stages of granular enlargement, eruptions of the integument, ulcerations of the mucous membrane. In very many instances the "tertiary" symptoms, as they are called, that is to say, the affections of the bones, ligaments, and periosteum, are contemporary with the primary symptoms and precede those of the integument. But generally speaking, the eruptive diseases manifest themselves soon after the infecting sore. M. De Meric denies that there is usually any premonitory fever; let him watch closely, and he will find that many a patient suffers more or less severely for several days before the appearance of the cutaneous disease. We have known the outbreak of syphilitic lichen mistaken for an attack of the small-pox, and have witnessed syphilitic lepra ushered in with symptoms not unlike those of rubeola; loss of appetite, and a feeling of sickness and depression, are by no means uncommon even among those whose attack is of gentler character. But still there are cases, especially among the poor, where the marks of syphilitic lepra have been allowed for many weeks to pass unnoticed. Indeed, M. De Meric has himself met with exceptions:

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* Maunder's translation of Ricord's Lectures, p. 81.

> "One patient of mature age, who suffered from gangrene about the glans by excess of inflammation, and had been much weakened by this process, felt languid and listless a few days before the appearance of papules on the skin. Two other patients, one with a rash and the other with papules, were attacked a few days before the eruption with very severe pain between the angle of the scapula and the spine. One of them suffered so much that I ordered mustard poultices to the spot, and the other's respiration was so seriously interfered with by the pain, that I was misled, and ordered leeches, fearing pleurisy."*

From observations on the natural history of the disease, the same author repeats the general opinion, "that we may, regardless of the kind of eruptions, reckon a mean of six weeks, dating from the primary sore, where no treatment has been resorted to." (Op. cit. p. 31.) But to this rate there are many exceptions. We have noticed that the slighter forms of primary syphilitic ulceration, *when followed by secondary symptoms*, usually precede the eruption or the sore throat but a very short period. Also, that an attack of syphilitic lepra or lichen, or syphilitic iritis, may supervene while the patient is still under the influence of mercury, the primary ulcer being unhealed. In other cases the interval is longer. John W——, aged twenty, was admitted into St. Bartholomew's Hospital in June, 1844, with a syphilitic sore in the body of the penis; he was treated in the usual way, and dismissed in about a month. In the month of November following, six months afterwards, he was again under treatment for papular eruption over the whole body. He had had no other primary affection between while.

Mr. De Meric refers the varieties of syphilitic eruptions to four heads—erythema, papules, vesicles, pustules. Other authors speak of the scaly, papular, pustular, and tubercular eruptions. It matters not which form of classification is adopted; but we cannot agree with the author that the term "scaly" is too vague; it indicates a condition of the integument far more persistent and better marked than the papular, vesicular, or pustular rash, which run so readily one into the other. But it is quite true that some cutaneous affections have a tendency to pass into ulceration, while others do not, although no useful division can be founded on the fact, for generally speaking it is the condition of the patient's health which exerts the unfavourable influence upon the usual course of the disease.

We think that in the works before us there is an evident want of information on the subject of constitutional syphilis. We do not meet with graphic descriptions of those severe ulcerative affections which are so baffling to the surgeon, so erratic and uncertain in their course as regards the patient. The teaching of the School of the Midi appears to us too precise; the venereal disease is divided into stages which do not exist in nature, and the only conclusion to which we can arrive is either that the symptoms and history of the disease are very different on the two sides of the Channel, or that the same series of facts have been very differently interpreted. But when it is borne in mind that the venereal patients of a large hospital are from all countries, that many have been infected by foreign prostitutes, and yet that the phenomena of the disease are the same, we think that the latter explanation is the more probable; and we unhesitatingly assert, that the teachings of the English school appear to be far more faithful to nature, and likely to be verified by future experience, than the ingenious hypotheses which have been so multiplied in the schools of Paris.

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* Lettsomian Lectures, by V. De Meric, p. 30.

We next proceed to the important subject of infantile syphilis—

> "An infant," says M. Diday, "may contract syphilis; firstly, during intra-uterine life, through the formative or nutritive elements derived from its parents; secondly, during or after birth by the absorption of the virus from some source or other. Hence we have two classes of phenomena, which we shall study successively under the designations of congenital and acquired syphilis." (p. 14.)

I. *Influence of the father.*—The father alone being syphilitic, can he communicate the disease to the child? M. Diday answers in the affirmative, adding, however, that it is difficult to obtain proof, because the father is very rarely affected with the disease without communicating it to the mother before or during pregnancy; and when the child is born with symptoms of syphilis we do not know to which of the parents they are due.

> "Numerous and precise facts furnish a positive demonstration of the theory. Professor Cederschjold has frequently seen children affected some weeks after birth with copper-coloured spots on the forehead, ulcers about the arms. He adds, 'the mothers were healthy, and there was no reason to suppose that they had been infected.' "*

It is asserted, too, that a man who has had syphilis, but who presents *no symptoms* of the disease at the moment, may beget a syphilitic child, the mother remaining to all appearance healthy. But we must pause in hastily drawing conclusions, for cases occur in which a woman retains the power of infecting the child under circumstances which might mislead the surgeon.

A healthy-looking woman, aged thirty, presented herself at St. Bartholomew's Hospital, with an infant of a few weeks old covered with a syphilitic rash. She said that twelve or fourteen years ago she had been on the town, but that to the best of her knowledge she never had had disease. She was married to a respectable man, a widower, the father of several healthy children. Every child to which she gave birth became, however, the subject of syphilis and died, and she entreated the surgeon, in unfeigned grief, to eradicate the disease by any means in his power. Now, it would be possible to quote this as an instance of infection from the father, had he ever suffered from venereal sore; but such was not the case; he was the parent of healthy children by the first marriage; the more probable explanation is, that at some period of the woman's life of prostitution the system had been generally infected, but without any obvious outward manifestation.

A man who is affected with syphilis and has connexion with a pregnant woman may, according to the author, communicate the disease directly to the fœtus without infecting the mother. But then, what is meant by "not infecting the mother?" Is it not true that there are many cases in which a mother brings forth, after connexion with an infected person, an unhealthy child at each subsequent pregnancy, although the parentage may be different? and should we not regard this in itself as an indication of general syphilitic taint. Cases have presented themselves before us, to show that during the period of utero-gestation secondary symptoms, which are latent at other periods, often burst into full activity. It is difficult, therefore, to say, how far the mother is free from taint, although there may be no present manifestation.

II. *The influence of the mother.*—It is unnecessary to speak of the influence of the mother infected before the moment of conception; but we shall proceed to inquire up to what period of pregnancy can syphilis, then first contracted by the mother, be communicated to the fœtus? In other words, is there a period of pregnancy after which syphilis contracted by the mother can no longer be transmitted to the fœtus? "Until the sixth month," said M. Ricord to his class, in 1847, "the mother may transmit constitutional syphilis acquired during gestation; but if the infection of the mother take place during the last three months, *it is not certain* that transmission is possible." M. Diday says, "Madame B——, a patient of my own, suffered from syphilitic contagion at the commencement of the seventh month. The child, born at the full time, died at the end of five months." (p. 31.) But new and varied observations are required for the definite solution of these questions.

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* Tidschrift for Lakare, Band vii. No. 10. 1840.

III. *Combined influence of both parents.*—M. Diday thinks the conclusion, "that in such a case the fœtus has no chance of escaping," has been made too hastily, and that so serious a sentence demands more conclusive evidence. "At the worst," he says, "the infection of both parents can only expose it to two noxious influences instead of one. But if the disease be slight in each of the parents, may it not be the case that this double chance is less serious than the single one, with which only one of the parents, being severely affected, would have threatened its future health?" (p. 32.)

Acquired Syphilis.—I. Infection during labour. II. Infection by lactation. III. Infection from accidental causes.

Of the first, we agree with both Diday and Ricord, "it is without doubt rare, but not impossible."

Of infection by lactation some assert, with Ricord, that the nurse was the subject of a primary sore, and communicated one to the child. Others, adhering to the older opinions, believe that the disease is communicated in a secondary form. Each of these explanations has its difficulties. Diday believes that congenital syphilis may be accidentally engrafted upon an adult—as by an infected child upon a nurse with an excoriated nipple; and that the sore then formed may transmit the disease in its secondary form to a healthy child.

As regards the influence of the milk, M. Diday, after collecting different opinions, leaves the matter in abeyance, experience not having given sufficient support to its suggestions.

Accidental causes of infantile syphilis are mentioned and supported by cases. But hospital experience would show that such cases are very rare.

In the description of the disease, M. Diday draws especial attention to morbid changes as they occur in the viscera, particularly the lungs and liver. Indurations are met with in the substance of the lung; when cut into, they are found to be composed internally of a compact yellowish tissue, in the centre of which is a cavity containing a sero-purulent fluid. The microscope reveals in it the most marked characters of pus. (p. 88.)

In the liver, the distinct appearance of its two substances has completely vanished. On a uniform yellowish ground, a more or less close layer of small, white, opaque grains is seen, having the appearance of grains of semola, with some delicate arborescences formed of empty bloodvessels. On pressure no blood is forced out. (p. 92.)

Further observations are required before the syphilitic character of these morbid appearances can be accepted.

> "The transmission of the poison by the fœtus to the mother, denied formerly, has now become admitted," says M. Diday, "if I may believe the development which this principle has received from various authors, in the first rank among whom justice requires that I should mention Mr. Jonathan Hutchinson and Mr. Victor De Merio." (p. vi.)

We are glad to read in M. Diday's work a refutation of that great error which Ricord has promulgated, namely, "that if a new-born child transmit syphilis to its nurse, it is because it had itself a primary chancre;" and as a counterpart, "that if only hereditary syphilis, constitutional lesions exist, it will not transmit any disease to its nurse." He brings forward a mass of interesting information; and in speaking of the glandular affections which not uncommonly present themselves in the axillæ of the nurse, remarks, "It is only one of the applications of this great result of observation, viz., that the poison of syphilis, when it involves the organism of an individual, always determines a process of reaction in the first gland which it meets with in its course, manifested externally by the enlargement of that gland." (Op. cit. p. 18.)

From experiments conducted by Mr. Savory it would appear that the mother may be poisoned by means of the blood circulating in the yet unborn fœtus. He administered chloroform to a pregnant bitch so as to produce complete insensibility, and then opening the uterus, he drew out one of a litter of pups, taking care not to injure the umbilical cord. After separating the pup carefully from all surrounding parts except the umbilical cord, he immersed it in water of the temperature of 100° Fahr., the normal degree of heat in the pregnant uterus. Then upon injecting a solution of strychnine into the pup, he noticed the phenomena of poisoning in the mother, the other pups remaining free from any ill effects. Now, as Mr. Savory very justly remarks, all that this experiment proves is the *possibility* of the mother being contaminated by poison emanating from the unborn fœtus; it serves to show that the conclusion of Mr. Hutchinson and others —namely, that a healthy female may become affected with constitutional syphilis, without even having a primary sore, by means of a fœtus infected with syphilis through the exclusive agency of the father—is not a doctrine physiologically impossible. But it leaves untouched and unsolved the problem whether the poison can be conveyed to the fœtal germ and there deposited, to become so blended with the formative cells as to affect intra-uterine life, and to contaminate the maternal circulation, without in the first place contaminating the mother by means of the ovary. And when we recollect that a woman, apparently healthy, who has once given birth to a syphilitic child, will, in many instances, give birth to other children similarly affected, though the parentage be different, we must confess that the reasoning does not appear to us to be quite conclusive.

A modern author* draws the following conclusions from very considerable experience in a public lying-in hospital, on the subject of syphilitic infection as between parent and offspring.

1. If the syphilitic poison is once in the body, and carried into the circulation, it may remain an indefinite time in the organism, and, under favourable circumstances, even when no external sign betrays its presence, may again make its appearance, and develop itself in various secondary forms, known as lues venerea.

2. The lues, latent, or visible, can be conveyed from person to person, and the poison taken up thus second-hand can be transmitted to the third or fourth time, &c.

3. Infection is generally effected by the genital organs. Nevertheless, syphilis can be introduced as a blood-disease by any other channel, through which certain secretions, or perhaps the blood of an infected person, can be conveyed into the circulation of another; so by means of the mouth, the nipple, a denuded spot of skin, vaccination, or the uterine blood destined to the nourishment of the fœtus.

4. A woman infected in such a manner bears in her, although there may be nothing externally to observe, nine times out of ten, specific marks of disease in the organs of generation. The author constantly found the uterus diseased; and there are few surgeons who have not remarked upon the vaginal discharge which persists even after delivery in women who have borne a syphilitic infant.

We must be permitted to doubt whether experience justifies us in asserting much more than Mr. Whitehead has here laid down. But credit is undoubtedly due to those who still persevere in their endeavours to unravel the difficulties in which this interesting subject is involved.

There remains for us only the pleasing duty of recommending these works to the attention of the profession. Mr. De Meric's lectures are well and concisely written, and explain the views of which he is the supporter so clearly that much fuller information is obtainable from them than from works of a more imposing appearance. We strongly recommend the pamphlet. The work of M. Diday is of great merit; it contains all that has been written on infantile syphilis; and he puts the whole subject in a well arranged form for further investigation, as well as present use. Mr. Maunder has translated Ricord's lectures faithfully; but the author is somewhat wordy and diffuse; still, those desirous of learning M. Ricord's opinions may here find them well reported. We do not agree with him; indeed we think that the subject of syphilis has suffered under the handling which it has received from the very questionable experiments of inoculation. But the cause is one which boasts of ardent supporters either way; and we doubt not that the controversy will prove advantageous to the cause of truth.

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* **James Whitehead: On the Transmission from Parent to Offspring of some Forms of Disease, and of Morbid Taints and Tendencies. 1851.**

REVIEW III.

1. *Handbuch der Historisch-Geographischen Pathologie.* Bearbeitet von Dr. AUGUST HIRSCH, in Danzig. Erste Abtheilung: Acute Infectionskrankheiten.—*Erlangen*, 1859. 8vo, pp. 300.

Manual of the Geographical History of Disease. By Dr. AUGUST HIRSCH, of Dantzic. First Part: Acute Infectious Diseases.—*Erlangen*, 1859. 8vo, pp. 300.

2. *On the Influence of Variations of Electric Tension as the Remote Cause of Epidemic and other Diseases.* By WILLIAM CRAIG, Consulting Surgeon to the Ayr Fever Hospital.—*London*, 1859. 8vo, pp. 436.
3. *Report on the Nervous System in Febrile Diseases and the Classification of Fevers by the Nervous System.* By HENRY FRASER CAMPBELL, A.M., M.D., Professor of Anatomy in the Medical College of Georgia, U. S.—*Philadelphia*, 1858. pp. 172.

THE increased attention which of late years has been bestowed upon everything which bears upon the causes of epidemic and contagious diseases, the circumstances under which they originate, and the means by which they may be prevented, demand from us a notice of the works now before us. The first on the list is truly a great undertaking, and displays an extraordinary amount of labour and research. The present volume constitutes the first of three parts into which the work is to be divided; the other two, which have not yet appeared, being destined to treat of chronic constitutional diseases and diseases of the individual organs. This first part is dedicated to the Epidemiological Society of London, in acknowledgment of their labours for the promotion of public hygiene; and if this society wrought no other good than the stimulation to labour of such men as Hirsch, its existence would be amply justified.

The diseases treated of are the following, in the order here given: 1, Malarious Fevers; 2, Yellow Fever; 3, Cholera; 4, Typhus Fevers; 5, Furuncular Plague; 6, Small-pox; 7, Measles; 8, Scarlet Fever; 9, Erysipelas; 10, Sweating Sickness; 11, Dengue; and 12, Influenza. In studying the geographical distribution of each of these diseases, the author has referred to almost every memoir and document bearing upon the subject. Copious references are given to the works of every author quoted, and this circumstance of itself greatly enhances the value of the work. We propose to make a few brief observations under the head of each of the above diseases.

I. *Malarious Fevers.*—The relative prevalence of these affections in every quarter of the globe is considered, as also their varying prevalence at different periods of time. The author then goes on to show that no race or nationality enjoys an immunity from malarious fevers, but that the natives of malarious countries are the subjects of these diseases less frequently and in a milder degree than strangers. The influence of season of the year, atmospheric moisture and temperature, the winds, the elevation of the country, the geological and mineralogical characters of the soil, and terrestrial moisture upon the production of malarious fevers, are severally considered. The article on these diseases concludes with several arguments in opposition to the view that malaria are the product of decomposition of vegetable matter. These are classified under three heads:

1. Several instances are mentioned of moist swamps, in countries presenting all the climatic and terrestrial characters of other regions where malarious fevers abound, and yet which are exempt, or almost so, from these diseases. Such are several localities in Peru, the Pampas of Rio de la Plata, many places in the delta of the Mississippi, and Negapatam in the Presidency of Madras.
2. In countries where malarious fevers are endemic, their prevalence varies greatly in different years without any variation in the causes which favour the evaporation from marshes; and their prevalence has also been observed to differ greatly in adjoining districts in the same year, without any difference in the causes assigned.
3. There are instances of malarious fevers breaking out in countries where formerly

they had been unknown, without any apparent change in the physical characters of these countries.

The author does not deny that external agencies may exercise a strong influence over the production of malarious fevers, but he thinks many observations are still wanting before the true essence of the disease is discovered. It is not a little curious to notice that he does not make use of the argument against the production of malaria from vegetable decomposition, which in this country is most frequently resorted to—viz., that malarious fevers may be met with quite independent of either moisture or vegetation. It is much to be doubted if either this or the author's arguments will stand the test of a severe critical examination; but space will not allow us here to enter into the question.

II. *Yellow Fever* prevails in three different regions of the globe: America, Spain, and the West Coast of Africa. Elaborate tables are given by the author, showing the dates and localities of all the great American epidemics. As regards the susceptibility to yellow fever among non-acclimatized individuals, it is found that this is greater in proportion to the distance from the equator of the place of their nativity and previous residence, and less in proportion to their length of sojourn in the yellow fever zone. No length of residence, however, will confer complete immunity; but it seems an established fact that negroes, and even Europeans who have the slightest admixture of black blood, are much less prone to be affected than whites. The influence of season of the year, atmospheric temperature, moisture, and electricity, and the physical characters of the soil upon the production of yellow fever, are dwelt upon at considerable length, and then follow some observations on the relations which are supposed to exist between yellow fever and the bilious remittent malarious fevers. The arguments which have been urged in support of the view that these two are merely modifications of one disease, and gradually pass into one another, are combated; and the conclusion is arrived at that the two affections are as distinct as yellow fever is from typhus.

III. *Indian Cholera.*—Under the head of this disease we have an elaborate history of its origin and progress, and the effects of the various atmospheric and terrestrial agencies upon its prevalence are carefully considered. Numerous authorities are quoted to prove the intimate connexion between moisture and the spread of cholera—as shown by the fact, that in towns it has very generally been observed to be most prevalent in those localities which border upon the sea-shore, or on the banks of rivers and canals. This is a circumstance which is also strongly insisted upon by Mr. Craig, in the second of the works upon our list, as confirmatory of his views, to be presently alluded to.

IV. *Typhus Fevers* are treated of under five different heads—Simple Typhus, Typhoid Fever, Sinking Typhus, Recurrent or Relapsing Typhus, and Bilious Typhoid. These several affections, however, the author regards as varieties or modifications of one diseased process rather than as distinct species.

By *Simple Typhus* is meant the true typhus, as characterized by the presence of the measly eruption. In tracing the history and geographical distribution of this disease, the author has omitted to mention that the first epidemics, upon the accounts of which we can place any reliance, occurred in Italy at the commencement of the sixteenth century, and were most accurately described by that celebrated physician, Fracastorius of Verona. The author also has been a little too hasty in stating that true typhus is met with in the East Indies. The statement rests solely on the observation of two cases at Simlah by Dr. Allan Webb, the author of the 'Pathologica Indica;' but in these cases we have no evidence that the eruption noticed was anything more than petechiæ, which are far from uncommon in the ordinary low remittent fevers of the tropics, which fevers, by the way, sometimes assume a general character very similar to that of typhus. It is true that we frequently hear of "typhus" occurring in India; but the term has too often been applied without any reference to its real nosological meaning. Dr. Morehead, in his recent work 'On the Diseases of India,' maintains that typhus is unknown in that country; and this, too, has been the result of our own observation, and of a correspondence with other medical officers in the Indian service, who are acquainted with the distinctions between the different species of continued fevers, as seen in this country. Indeed, as far as our knowledge at present extends, true typhus in Europe appears to be confined within the limits of 40° and 60° north latitude; and in the New World between those of 32° and 50° north latitude; and to exist in no place where the mean annual temperature rises above 62° Fah., or falls below 40°.

With regard to the so-called typhoid fever—that which is accompanied by disease of Peyer's glands—its distribution is not so limited, or rather, it is less limited in the direction of the equator, though probably more limited towards the polar regions. There is every reason to believe, that cases of this disease are far from uncommon on the continent of India; but the author is scarcely justified in putting down as examples of it all the cases which have been described by authors under the appellation of typhoid fever. We could point to instances quoted by Hirsch (some of which indeed came under our own observation), in which we know that there was no evidence to prove that the fever alluded to was the *fièvre typhoide* of Louis. The only cases as yet on which implicit reliance can be placed are those which have been recorded by Scriven[*] and Ewart;[†] but they are quite sufficient to decide the question.

Typhus syncopalis, or sinking typhus, we only know as a variety of the true typhus, characterized by a tendency to rapid death by syncope, and originating from a concentration of the causes which give rise to the ordinary forms of the disease. Such were the cases observed during the famous siege of Saragossa, in 1809, and elsewhere during the wars of the first Napoleon; although in the work before us the only cases referred to appear to be American.

An accurate account is given of the various epidemics of the relapsing fever, first described by Rutty, of Dublin, in 1739. This is the true famine fever of Ireland—the *hungerpest* of German writers.

We scarcely know what the author means by Bilious Typhoid Fever, or *Typhus icterodes*. Jaundice, it is true, is a very frequent symptom of the relapsing fever; but most of the instances referred to appear to have been cases of either yellow fever or of malarious remittent fever. Here again we think the author has allowed himself to be confused by the varying titles applied by different writers to the same affection.

After treating separately of the geographical distribution of each of these fevers, the author proceeds to consider the influence of season of the year, temperature, and other atmospheric and terrestrial agencies, upon them all collectively. In thus doing we think that he has erred; and if he had proceeded differently, he might have arrived at another conclusion, than that they are all merely varieties of the same disease. For example, we would merely refer to season of the year. This has no influence over the prevalence of true typhus; but the so-called typhoid fever (or, as it has been recently designated, pythogenic fever) is almost invariably most prevalent in autumn, so much so, that in many places it has received the name of autumnal or fall fever.

V. *Furuncular Plague.*—A distinction is drawn between the Oriental and the Indian plague—the latter only differing in its frequent complication with pleuro-pneumonia. In the summer of 1836 this Indian plague spread great devastation through many parts of the Bombay Presidency. An historical account is given of the epidemics of both varieties, with observations on the apparent influence of various external causes on their prevalence.

VI., VII., and VIII.—*Small-pox, Measles, and Scarlet Fever*, are all treated of in a manner similar to the diseases already noticed. The author has not been able to discover any authenticated accounts of the occurrence of scarlet fever in India. Dr. Morehead tells us that in Bengal and the North-Western provinces a fever has prevailed epidemically on several occasions since 1824, remittent in character, and accompanied by a scarlet eruption, and in some instances, by inflammation of the mucous membrane of the mouth and pharynx. None of the medical men, however, who have observed it have considered it as identical with European scarlet fever. Numerous observations are also collected by the author, to prove that in countries where scarlet fever is met with, it is as prevalent and as fatal in the families of the higher ranks living in large houses in open, airy situations, as in those of the lower classes crowded together in small, badly ventilated dwellings in narrow streets and lanes. This is a fact of extreme interest, and the truth of which is pretty generally recognised. Indeed, not a few consider that the disease is more fatal among the upper ranks than in the lower.

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[*] Scriven: British and Foreign Medico-Chirurgical Review, Oct. 1857; and Indian Annals of Medical Science, Part vii. 1857.

[†] Ewart: Indian Annals of Medical Science, Part vii. p. 65.

Hirsch has also ascertained that out of 100 epidemics of scarlatina, 30 commenced in autumn, 24 in summer, 25 in winter, and 21 in spring. Now, in London, and also in several of the other large towns in England, it is found that the total mortality from the disease reaches its climax in October and the beginning of November, but it remains to be shown whether the disease is then more prevalent or more fatal.

IX. *Erysipelas* is treated under the heads of Simple Erysipelas, Hospital Erysipelas, Erysipelas Neonatorum, and Typhoid or Gangrenous Erysipelas. The last of these is shown to be of peculiar interest, from the fact that during the last ten years it has prevailed to a great extent in an epidemic form in many parts of the continent of North America. A table has been constructed pointing out the precise date and locality of each of these epidemics, and containing references to the various memoirs in which they have been described. Like hospital erysipelas, it is eminently contagious.

X. *Sweating Sickness* is next taken up, and some interesting observations are made as to the relation between this disease and Indian cholera, which are well deserving of attention, but for which we must refer to the original work. Next we have,

XI. *Dengue*, or, as it has been also called, Breakbone or Neuralgic Fever, a disease which appears to be peculiar to North America and the West Indies, and which is characterized by acute febrile symptoms accompanied by most severe arthritic pains lasting for a few days, and sometimes followed by a relapse, but by no means fatal. The true nature of this disease requires further investigation. In this country it may be said to be unknown, although it is not unlike in many of its principal characters to what is familiar to us under the name of relapsing fever. As it is scarcely even mentioned in European medical literature, we would refer for descriptions of it to Hirsch's work, as also to that of Dr. Campbell. Lastly,

XII. *Influenza* comes under notice. The article on this disease commences with a most elaborate chronological table of all the epidemics which have occurred from the year 1510 downwards, and concludes with an equally elaborate bibliography of the literature bearing upon the subject. There are many observations given to prove that epidemics of the disease have appeared under all conditions of season, and consequently that its origin and propagation are quite independent of all known atmospheric influences.

The work of Hirsch is truly a most valuable contribution to medical literature, and must henceforth be regarded as the great authority upon everything having reference to the geographical distribution of disease. We shall hail with much pleasure the appearance of the two remaining volumes.

We regret that we cannot speak in terms of equal praise of the two other works now before us. That of Mr. Craig has been written with the object of proving that the great cause of epidemic and many other diseases is to be looked for in the variations of electric tension, and that such causes as contagion and *miasmata* exist only in the minds of medical theorists. The work contains few or no results of the author's personal observation, but the observations of many others have been carefully collected to bear out his reasoning. In many instances these observations would quite as readily admit of another explanation than that which is here given to them, while in some parts of the work there are assumptions which, to say the least, are most gratuitous. For example, we still require something more than the mere assertions of the author to convince us that nervous force and the electricity evolved during galvanic action are one and the same. The whole argument of the work is founded upon these principles:

> "That electricity and nervous force are identical, that the electricity evolved from the air in the lungs during respiration, and that separated from the ingesta during assimilation, is that which supplies the vital electricity to the nervous system, and that any cause which hinders the supply, or suddenly and to a great extent withdraws it after being supplied, will injuriously affect the system." (p. 48.)

This withdrawal of vital electricity is thought first to affect the nervous system, and through this, the blood and solid tissues. Epidemic and endemic diseases, we are told, are most prevalent in damp places, because moisture is a good conductor of electricity. But not only is it asserted that nervous force and electricity are identical, it is necessary for the author's argument to maintain that caloric and electricity are also identical—"merely different manifestations of the same element." (pp. 193 and 221.) Now, it would be out of place here to enter into such an intricate question as the abstract nature of heat and electricity, but we do insist that in the present state of science the author is not justified in making the assertion that "caloric and electricity are identical," without a single fresh fact or observation in support of the assertion, and still less to base upon such a groundless foundation a theory of the origin of disease.

Again, if all diseases mentioned by the author originate from an abstraction of vital electricity, what, we would ask, constitutes the difference between the causes of each? —what is it that determines whether the disease is to be plague, cholera, ague, or yellow fever? An attempt is certainly made at one place to account for the difference between plague and yellow fever. We give the author's own words, and we leave our readers to draw their own conclusions as to the sufficiency of the explanation:

> "In those regions where yellow fever is endemical, it is the effectual wetting of the dry soil and the hasty drying under a burning sun, that produce the phenomena of this disease. In those regions, on the other hand, which are frequently visited by plague, the wetting is as sudden, and in some places more effectual, but the drying is less rapid but more continuous. In the former-mentioned disease, the abstraction of vital electricity is larger, the shock to the system greater and more sudden, and the course of the disease shorter. In the latter disease, the abstraction of vital electricity is not so large in quantity, but is more continuous, and the course of the disease is more protracted. It requires a different amount of agency to produce respectively plague and yellow fever. The land in the unreclaimed regions of the back settlements of North America, with a stiff and unabsorbing condition of its surface, will present a wet expanse to a burning sun, and produce quick evaporation, readily placing the locality in a low state of electric tension, and this negative surface will speedily abstract from the animals in contact with it. On the mud-banks of the Nile, and the other swampy regions of the Levant, there is at certain seasons of the year a slower but more constant evaporation—a more gradual abstraction of vital electricity." (pp. 269–70.)

The plague we have long considered as closely allied in its etiology to the typhus of this country. Indeed, we occasionally see cases of typhus complicated with glandular swellings and abscesses, and so resembling the Eastern plague that a celebrated Egyptian physician some years ago, on seeing them in the London Fever Hospital, declared that in Egypt they would be set down as examples of the latter disease.

The author is rather sparing in his remarks upon the origin of the epidemic diseases of our own country, which, by the way, would seem to be the only ones he has had an opportunity of personally observing. There is one statement, however, under this head, which we cannot allow to pass unnoticed. The epidemic of fever which occurred at Croydon in 1852, is attributed to the abstraction of vital electricity produced by the evaporation consequent on the overflowing of the Bourne spring. Now, this epidemic was an example of the so-called typhoid fever, which clearly owed its origin to the putrid emanations from the drains at that time undergoing repair. We have the most ample evidence upon these points on the authority of some of the ablest physicians and engineers of the day; and we hardly think the author could have committed such an error if he had consulted for information the official documents upon the subject published by the Board of Health, in place of the account contained in a Scotch newspaper.*

Again, with regard to cholera, we are not prepared to attest the statement that there is "a disturbance of the central nervous system, as displayed in deranged sensorial manifestations." The sympathetic system, it is true, may be primarily influenced; but the central nervous organ and the special senses in most cases remain perfectly unaffected until the last. The author also states, that the inhabitants of houses with wooden and carpeted floors are more exempt from cholera than those of houses with stone floors, and this he attributes to the non-conducting powers of carpet and wood. We doubt if the fact which is thus explained does not in the first place require to be established.

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* The Scottish Guardian.

Mr. Craig's work exhibits considerable originality, but is apparently little more than the elaboration of an hypothesis in his own study, the author having had but few opportunities of personally observing the subjects of which he treats. The preface of the book commences by stating, that it ought to be the object of every writer steadily to aim at finding out the truth, and when this has been accomplished, to proclaim it to the world. We very much doubt if the author has established the correctness of his views, and in our opinion their publication in the present form is premature.

The third work on our list is the reprint of a paper which originally appeared in the 'Transactions of the American Medical Association.' The author does not profess to investigate the exciting causes of fevers, but only to search out the laws of their morbid phenomena. Recent researches, we may observe, have shown that the subject of their etiology is scarcely so unpromising as we would conclude from reading Dr. Campbell's statement, that the causes of fevers are "entirely and confessedly beyond our reach and comprehension."

Dr. Campbell's object is to demonstrate, that the nervous system is primarily and principally affected in febrile diseases, or in short, that fevers are essentially diseases of the nervous system. Fevers he considers as divisible into two great classes, the *paroxysmal* and the *continuous*. The former he believes to result from some morbid action of the cerebro-spinal nerves, while in the latter the ganglionic system is chiefly affected. Indeed, the whole argument of the work may be summed up in the following fundamental proposition.

> "As in the nervous system we recognise two grand departments, viz.: 1st. The cerebro-spinal system, all the normal actions of which are subject to cessation and interruption; and 2ndly. The ganglionic system, all the normal actions of which are of a continuous and uninterrupted character, so in the manifestations of febrile diseases do we distinctly recognise two grand distinguishing characteristics, respectively typifying the normal actions of these two systems of nerves. Thus, a character of *paroxysm* obtains in certain cases, while a character of *continuousness* as plainly marks the others." (p. 11.)

Cerebro-spinal and *ganglionic neuroses* consequently constitute the basis of the author's classification. In the former class he includes neuralgia, tetanus, epilepsy, hooping-cough, croup, spasmodic asthma, intermittent fevers, dengue fever, and yellow fever; and under the latter, diarrhœa, cholera, typhoid and typhus fevers, and the various exanthemata.

We make no comment upon the author's views, which, of course, are entirely hypothetical. His work, however, will be found to contain an interesting account of an epidemic of diphtheria, which occurred at Augusta in the autumn of 1848 (p. 76), as also accounts of several outbreaks of the so-called *dengue* or *breakbone fever* (p. 135). The latter deserve especial attention from the fact that the disease is scarcely known to the profession in this country. Diphtheria, as it appeared at Augusta, Dr. Campbell believed to exhibit paroxysmal characters, and to be greatly benefited by antiperiodic treatment, such as the administration of quinine.

Review IV.

1. *The Healing Art the Right Hand of the Church.* By Therapeutes.—*Edinburgh*, 1859. pp. 279.
2. *God in Disease.* By James Foulis Duncan, M.D., Fellow of the Royal College of Physicians in Ireland; Physician to Sir P. Dun's and Simpson's Hospitals, &c. Second Edition.—*London*, 1857.
3. *The Soul and Future Life.* By Thomas Cromwell, M.D.—*London*, 1859. pp. 307.
4. *Man and his Dwelling Place.*—*London*, 1859. pp. 416.

It is far more strictly true of scientific subjects than of literary, that they are united by a common bond, and maintained in a certain and close relation to one another. As our own science has been subdivided, and its branches multiplied, its relations have become extended, and its points of contact with other sciences much more close and numerous. Microscopic Anatomy brings us in contact with the whole field of Natural History. Chemistry, refined and subtle to a degree our fathers never conceived possible, and the various medical inquiries connected with sanitary subjects, establish relations with the sciences and the practical arts, that are most nearly concerned both with the destructive energies of war and the progressive improvements of peace. Physiological Psychology cannot ignore the old questions of metaphysics, nor the moral questions now so keenly agitated that refer to the social improvement of our country and the gradual elevation of our race. And lastly, every hour of our life, every speculation on disease, and every effort at its removal, brings us face to face with that grand unsolvable problem, into which all minor difficulties ultimately run up, which throws its inscrutable and gloomy shadow over this beautiful world—the existence of physical and moral evil, under the administration of a Deity infinite in goodness and in power. We trust, therefore, we shall not be considered going out of our way, or beyond our proper limits, if we offer a few remarks on the connexion of our art with those benevolent efforts now made to diffuse the light of science and religion—the two co-ordinate motive powers in the improvement of our species, among the less favoured tribes of our race, and among the outcasts of our own civilization; adverting, at the same time, to the influence which the current philosophical opinions of our time, as exhibited in such works as those whose titles we have placed at the head of this article, must exert both on these efforts and on their connexion with medical science.

In M. Guizot's 'Lectures on Civilization' the author remarks that whenever a man obtains possession of a new and important truth, he is constrained by an impulse more or less irresistible, according to the character of his mind, to become a missionary in its propagation. Now this, and much more than this, results from the moral structure of the human mind; not only are we constrained to diffuse the opinions we hold, but we are equally constrained and equally bound to give practical effect to them. Not only is there a feeling of benevolence in human nature, a separate and independent sentiment, which no amount of ingenuity can explain away or resolve into elements of a purely selfish description; but this sentiment is supported, and sanctioned, and rendered imperative, by the decisions of that faculty, call it conscience, or the moral sense, or what you will, which at once claims, and is felt to claim rightfully, control, superintendence, and direction over the rest. It matters little what account we give of conscience analytically, whether we regard it as a simple and original faculty, or capable of being resolved into yet simpler elements; even on this latter view, the moment these elements are presented to each other, they combine and form one homogeneous power, which claims authority, and has its claims at once conceded. For the truth of this statement we appeal to every man's individual consciousness, and to the nature of the human mind itself, which is evidently a system, as much as any piece of human mechanism, adapted to subserve a particular purpose; and consequently the purposes which its various propensities, affections, and passions are capable of serving, measure and define the extent of our obligations; regard being always had to the supreme authority of the faculty before referred to, in which human nature may be said to culminate. The obligations to benevolent exertion, then, do not *arise* from positive injunction, but are strictly natural, originating from the nature of man and from the relations in which he is placed, combined with the powers and opportunities which the progressive development of society confers on individuals, bodies of men, or separate nations.

Civilized men have not been slow to take advantage of their great superiority in the arts of war, and the progress of events appears to indicate that the whole of the earth's surface will ultimately fall under the dominion of the energetic and progressive nations of the West, our own restless and acquisitive Anglo-Saxon race appropriating the larger share of it, either by direct conquest, or by the progress of commercial aggrandizement. But the moral obligations resulting from superior power, education, and knowledge, either towards barbarous races of men or defective civilizations, or even towards the

Helot tribes of our own kindred and nation, have been slowly acknowledged, and are even still imperfectly and feebly acted on.

A better day, we believe, however, has risen. While the researches of science, particularly the sciences of vitality, with which we have to do, are carried on with an ardour, and prosecuted with a success, which half a century ago could not have been conceived; extended views of political economy are clearly and irresistibly establishing the expediency, or rather the necessity, of applying these to the welfare and improvement both of the inferior tribes with whom in the first instance, perhaps, our cupidity or our ambition brought us into contact, and of those of our own kindred and tongue whose material and moral wellbeing we have too long neglected. The progress of research, either in the field of physical investigation, or of political organization, shows, not as zealots allege, the opposition between science and religion—nor, as sceptics maintain, the inutility of moral efforts—but brings into harmony the progressive advancement of the intellect with the moral instincts of man; and by giving right direction to the simple but pregnant injunction of the Bible, that we should do good unto all men as we have opportunity, affords it also a secure and valid basis of authority.

From what we have said, it appears that the benevolent exertions which characterize modern times have a deep basis in natural obligation even anterior to any supernatural revelation; we most willingly admit, however, that they derive their impetus mainly, if not solely, from the express injunctions and, if possible, still more from the beneficent spirit of Christianity, which, whatever views we hold of its peculiar supplemental statements and doctrines, all must admit, though it is a view far too little insisted on or expounded, to be in the first instance a republication of natural theology, including under that term both the existence and attributes of the Supreme Being, and the moral obligations of man resulting from his relations to God and the nature with which He has endowed him. These are everywhere asserted or assumed in the sacred Scriptures, so that the positive evidence in their favour comes in aid of the principles of an enlightened theism—its mild and genial spirit gives warmth to the speculations of philosophy, and its sanctions of tremendous power give an efficacy to the deductions of reason, which nothing else can impart.

Both as philosophical and religious men, then, we are bound to take cognizance of, and to aid all those movements which have for their object to raise the character of our own population, to promote or secure the development of our vast dependencies, or even to diffuse in foreign and barbarous lands the light and the blessings of our own civilization, clearly destined as it is to overspread the whole surface of the globe. We have adverted to the immeasurable superiority of civilized men in the arts of war, and doubtless the blessings of peace do in time and to a certain extent follow the devastating steps of conquest, and perhaps it is not possible that the religion of peace should in all cases make progress without being preceded by the destroying angel. Still, all should and must rejoice in any schemes that bring civilized man in contact with his savage brother, not from the lust of gain, and not for oppression and the love of power, but for purposes of amity and goodwill. In the lower strata, too, of our own civilization, the impression is by no means worn out that the people are only the subjects of power, if not its victims, having no share either in its guidance or in the blessings which it brings along with it. Everything, therefore, that brings the superior, and, still more, the scientific classes of society in contact with the inferior, in the way of felt and palpable benefit conferred upon the latter, is of immense advantage both in elevating their capacity and status, and in giving stability to our existing institutions. Now there is probably no body of educated men who can contribute more effectually to the promotion of these ends than the medical profession. It may be at once admitted that recent medical inquiries have not added so much to our power of curing disease, as to our knowledge of its nature and our means of distinguishing one from another; still there is no doubt whatever that we are advancing even in this, while the vast strides that have been made and that are still in progress in extending our knowledge of the causes of diseases, and consequently of obviating their occurrence and mitigating their virulence, are daily increasing our influence in modifying and accelerating the progress of society, especially in forwarding those benevolent exertions on which, in so large a measure, this progress is dependent.

The stern records of the Registrar-General, indicating the fearful amount of preventible mortality that prevails in the richest and most highly civilized country in the world, as well as the appalling ravages of infectious disease among less civilized communities, are unanswerable proofs of the prodigious importance of medical agency in all schemes for the advancement of society, and for the improvement of human beings in whatever state they may exist. And though we may deplore, as we have just said, our very limited power over many forms of disease incident to our own country, and to our own stage of civilization, we may at least rejoice and hope in the increasing knowledge of natural processes which promises ultimately to put it in our power, if not to annihilate, at least greatly to diminish the operation of many of those fearful scourges that decimate our own population, and sweep with a far more destructive energy over less favoured lands. And let it never be forgotten, if such in any measure be our power, our power exactly defines and measures our obligation.

We have dwelt so long on the natural obligations to benevolent efforts, and on the power and influence of science to promote them, because we are deeply persuaded of the injurious effects that have resulted both to science and to religion from their dissociation; or, perhaps, we might more correctly say, from the presumed opposition existing between them, an error and an absurdity, for the prevalence of which, though it may not be possible altogether to acquit men of science, we do believe the religious party, in England at least, to be mainly responsible, and from which, by a kind of retributive justice, they have principally suffered both in the turn of their mind and in the conduct of many of their undertakings. The bitter jealousy with which a popular theology, founded on a thorough perversion of insulated passages of Scripture, has heretofore regarded scientific speculation, must be familiar to every one at all acquainted with a certain description of religious literature, if literature it may be called. It ought surely to be enough to appease this feeling to remind those who entertain it of the analogy that subsists between nature and revelation, long ago pointed out by Bishop Butler, from which it might fairly be anticipated that new views might be developed, and old notions either extended or corrected as learning and information increased; and it might teach caution to remember that there is probably not now in Europe a single educated man who receives the first verses of Genesis *in the same sense* in which they were all but universally accepted not fifty years ago, and in which, by many parties, it was made a testing article to adopt them. The damage resulting from such extreme and, frequently, intolerant assertions of opinion is, we believe, very great, and we would earnestly recommend the consideration of the remark of an enlightened American writer, "that there is a true system of theology as in other sciences, and the world is still struggling to ascertain what it is; progress is made, and patient thought is rewarded in this as in other sciences." We believe the bonds that unite religion to science to be indissoluble, and the whole course of events goes to prove it. However securely Christianity may seem to have been planted in any community, if that community has retrograded, or has ceased to advance in intelligence, religion has invariably become feeble and corrupt, if it has not been altogether extinguished. On the other hand, in spite of periods of difficulty, and doubt, and trial, her influence has always been strongest, and her progress most satisfactory, in nations remarkable for intellectual development and culture: and it is from them that she goes forth to other lands, or descends to the darker regions of social existence with the fairest hopes of success.

Her progress henceforward is, we are persuaded, necessarily interwoven with that of science, and with none so intimately as with the sciences connected with our own profession, not only as branches of evidence by which the truth of her statements may be tested, but as one of the most powerful means, if not the most powerful and essential of all means for the diffusion of her influences.

This view is advocated in the first work whose title is placed at the head of this article, 'The Healing Art the Right Hand of the Church.' The author regards medical science and the practice of medicine as an integral part of church organization, and seems to believe that the want of power in the Church to control, or even to contend with the social evils of our own state, or to spread Christianity in heathen lands, proceeds from their unhallowed disruption. How he proposes to reunite them we do not learn; for though we have read the book attentively, there is an absence of all positive statement, and a vague mistiness of view that prevents us, we confess candidly, from gathering with any exactness what the author precisely intends. We venture to express a devout and humble hope, that he does not seek to accomplish his object by the multiplication of clerical doctors, a genus of which the numbers are already more than sufficiently abundant. The work contains a considerable amount of curious learning, and recalls to our attention a number of interesting facts related in Scripture; but there is a want of definiteness and completeness about it, which prevents it from making any strong impression, though it brings out very clearly one thing, that the instances of the cure of disease, recorded in Holy Writ, ought to be regarded not only as evidences of the truth of Revelation as works of supernatural power, but that they formed part and parcel of the blessings which the mission of the Son of God was intended to confer on mankind. Thus connecting Christianity with the immediate warm and living interests of humanity, and removing it from the regions of dry and cold abstraction.

The second book on our list, 'God in Disease,' is clearly the work of a well-instructed physician, and of a devout and pious man, who seeks to illustrate the religious and moral ends which sickness and pain may answer both to those who suffer and to those who witness them. It is well written, and abounds in illustrations sufficiently exact to suit a work intended for popular reading. To many readers it will afford information of an interesting character. But towards meeting the great difficulty, *why* so much misery, pain, and bitter sorrow should exist at all, it does not advance a single step. The utmost that books of this kind can do, is to show that if there be any hint of a future solution of this fearful question, or any intimations in nature or in the Bible, as there are intimations numerous and express, of the co-existence of perfect benevolence on the part of the Creator, with all the misery and evil which it is the lot of humanity to endure; a careful study of the facts may show us that these intimations are probable, credible, or not incredible, as the evidence may warrant us in affirming.

Both these books are in themselves more strictly theological than properly brings them under the notice of a medical review; and into their theological, and still less into their ecclesiastical contents, we have no intention of entering; but the former, more especially, deals with something like the train of thought we have followed in the preceding pages, and enters pretty fully into the subject of medical missions, which has attracted a large share of attention from the best minds both in the profession and out of it, and naturally associates itself with medical agency in the prosecution of benevolent schemes in our own country, and among our own population. We think no one can doubt that there is a peculiar propriety in the employment of medical agency both in missionary labour abroad, and in what may with perfect truth be called missionary labour at home. Sickness, pain, and accidental injuries, are the common lot of man, but chiefly are they the sad inheritance of the poor, and so far are uncivilized men from being exempt from their influence, that the concurrent testimony of all trustworthy travellers, and the well established narratives of the total, or all but total, extinction of whole tribes of Red Indians by small-pox, for example, prove them to have less power of resistance than the inhabitants of civilized countries.*

The man, then, who can modify in any appreciable degree the power of disease, or whose scientific knowledge enables him to diminish the frequency of its visitations, or who can bring the efficacy of a highly developed surgical skill, about the beneficial effects of which the most sceptical can entertain no doubt, to bear upon the sufferings and the fears of men, is clearly the person who is best fitted for the work of a missionary. Moreover, it is in the prevention of disease, especially of epidemic disease, that medical science has both achieved its principal success, and holds out the largest promise. To the knowledge of those prodigious, but steadily recurring chemical processes which maintain the balance of organic nature, and maintain in constant operation what has been finely called the vast miracle that still goes on in silence around us, we have added a large amount of knowledge of, what may be called, disturbing forces, generally resulting, at least in their injurious influence, from the ignorance or mismanagement of men, and therefore to a great extent removable by better information and increased forecast. We have not yet been able to analyze the subtle poisons of malarious exhalations or of contagion, but we have done something, and may legitimately hope to do still more to remove what knowledge we possess from the region of mere empirical facts, and impart to it a scientific, and therefore an available character, that is, we can with more confidence, for example, in the case even of the most malignant epidemics, predict their probable occurrence, and either avoid or provide against their advent. But in all cases where we have to do with voluntary agents, if we would do any good we must secure their own willing and intelligent co-operation. The great secret of doing good to any man is to teach and assist him to do good to himself. This is a principle which we believe lies at the root of all charity, and from want of attention to it many a well intended scheme of benevolence has been shipwrecked; and from this results the necessity of the medical missionary, who primarily addresses himself to the physical requirements of men, being enabled, either himself or by enlightened coadjutors, to stimulate the intellect and to purify and elevate the moral feelings of those with whom he comes in contact.

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* 'The Bombay Times' of April 26th, which brings the account of the death of that remarkable man, Sir Jamsetjee Jeejeebhoy, equally distinguished for his vast wealth and his beneficence, contains some interesting details respecting the Grant Medical College on the occasion of the annual exhibition for the distribution of prizes and diplomas to the successful candidates—all natives. Amongst the prizes was a gold medal, of the value of 15*l.*, awarded for the first time, and now for proficiency in Practical Medical Jurisprudence,—for which, and for some other prizes, the College is indebted to the munificence of the Parsee baronet. The institution is on a small scale, and has been in existence only a few years; it has for its alumni under fifty students, and of these only nine have obtained degrees this year. But it is with a view to the future that we consider it important—as a seed of a higher civilization—as the means of introducing medical science and the exact sciences into India, and thereby conducing to expel the delusions under which the native mind labours through the influence of an absurd and licentious idolatry and the trammels of caste. The medical teacher and the medical practitioner we hold to be one of the ablest of missionaries, especially if he works in the true spirit of Christianity.

It is, as the author of 'The Healing Art' truly says, because Christianity and its Divine founder addressed themselves to the whole nature of man that Christianity possesses so much power. In our own country nothing can be more striking than the difficulty of obtaining the concurrence of the people themselves in sanitary arrangements, for example, and the short-lived character of all improvements that have been forced on by the influence or power of single individuals—that have, in short, been developed prematurely and by pressure *ab extra.* It is in fact of essential importance that division of labour should take place in all great movements; but it is at least of equal importance that the whole nature of man be had respect to in all that you attempt to accomplish; his convictions as well as his respect for authority, and even his self-love as well as his moral instincts. Hence the propriety of employing as agents men who both can and will do something more than merely explain a dogmatic creed—who will show to uncultivated or feeble understandings the reasonableness of positive precepts, the direct and even immediate advantages resulting from their observance, and, in fact, who will strive, by the introduction of all the useful arts of life, to train men to the use of their rational faculties, and not to remain the mere creatures of impulse or prejudice, to become capable, in fine, of looking both before and after.

If we examine even very cursorily the volumes of the Society for the Promotion of Social Science, the address of Lord Shaftesbury, the paper of Dr. Alison, and the contributions of various other eminent names, both professional and extra-professional, we cannot help being struck in the first place by the vast importance of sanitary arrangements in regard to moral development, and consequently the fitness and power of medical men to forward them. The dependence of the moral feelings and of intellectual development on physical well-being is made more and more clear every day. The imperative necessity for the free access of light, and a plentiful supply of fresh air and pure water, for the preservation of physical health, and for the prevention both of acute epidemics and of the tendency to chronic disease, needs merely be adverted to; but the deteriorating effect of the want of these accessories on the healthy energy of the brain, the great organ of the mind, is brought out in various papers of these Transactions so strongly, that they might well form the text of a whole article. At all events they establish unquestionably the position that, in our own country and under our own eye, there is ample scope for what may well be called missionary exertions, and no more promising agency for these purposes than that of medical men. How that agency is to amalgamate with existing organizations, or how it may be regarded by them, following the example of the author of 'The Healing Art,' we do not inquire; but one thing we are quite sure of, that now-a-days, and in the present spirit that animates the public mind, all ecclesiastical organizations must submit to be tested not by appeals to antiquity, not even by a strained interpretation of a few Scripture texts, but by their evident tendency to conform to the dictates of common sense, and to promote the well-being oi mankind. No ecclesiastical organization whatever *is* religion, but only a means of propagating it, or of forwarding some of its objects, and they must all depend on their ability to show that they are capable of fulfilling the purposes for which they in reality exist.

If we turn for a moment to heathen countries, while we wish to do credit and justice to the excellence of the motives that have led men to engage in missionary labours, we have some fear that a dispassionate observer would be compelled to admit that the success attending them has not been quite commensurate to the vast means expended, and has certainly fallen very far short of the sanguine anticipations of their founders. In China, for example, where the Roman Catholics claim converts to the number of two hundred thousand, and where an extremely well-informed friend of ours admits that they cannot be less than half that number, the same competent authority does not believe the Protestant missionaries can claim above four or five hundred. The reasons of this prodigious difference of success, for which the different duration of the missions is quite inadequate to account, are probably various, and lie out of our way to discuss; but one we have no doubt of is the skill with which the Jesuits adapted the scientific attainments of their agents to the situations they were meant to fill, and on the other hand the unscientific character of Protestant missionaries, arising from the unfortunate enmity that, till recent times, existed in the religious community to learning and scientific attainments of any kind. Now, the establishment of a Medical Missionary Society is at all events a step in the right direction in this respect. It is an acknowledgment of the co-operative power of science in the work of civilizing and Christianizing mankind, and a proof that the experience of such men as Livingstone, and the statements of candid travellers and investigators, such as Barth, have produced some good effect, in compelling the religious world to the conviction, that a mere exhibition of Christianity as a dogmatic system is not all that is necessary to secure its reception either among barbarous tribes, or among Orientals where civilization is at the best stationary. But another advantage of this association of medicine with benevolent enterprise is that it introduces an element of impartial attention to evidence, and of enlightened and dispassionate investigation into a region where such habits of thought are much wanted. We do not believe that there is any body of men more zealous and independent in their search after truth than our own profession. The utter want of anything like a corporate or collective interest, secures the utmost latitude and freedom of opinion; things greatly to be desiderated in the religious world, where the views held are compressed into dogmatic formulæ, and even identified with a stereotyped mode of expression, and where nobody can help being struck with the facility with which what have been aptly called the romantic fictions of the prophetic press are received; and, on the other hand, the tenacity with which forms of expression and ancient views are clung to and maintained. Among a body of this kind, we hold that the opinions and searching inquiries of the medical profession are of inestimable value to guide and to discipline the ardent zeal and precipitation, which, however much we may admire the purity of motive from which they spring, may be dangerous in the conduct of practical affairs.

Along with the exertions of medical men in the field of benevolent enterprise it may not be amiss before we close to advert to the employment of female agency, a subject which comes prominently out in the sociological volumes, where papers of real merit are contributed by several ladies. Here again the acuteness of the Romish Church, and the skill with which she adapts some parts of her system to every phase of human nature, has anticipated Protestant benevolence, and ought, if we do not allow our prejudices to stand in the way of our judgment, to afford suggestions both to guide and to warn us. The great struggle in the Crimea, and the deep excitement caused by the "horrible and heartrending" misfortunes that befel the sick of our army, first brought prominently before the public the beneficent and soothing influence of woman, under circumstances where her agency had been little thought of before. The passionate feelings of that agitating time were not favourable for a calm consideration of the mode in which female agency on such occasions should be organized and conducted; still the disinterested zeal and devotion of the English ladies are above all praise, and the hearts of their countrymen responded with ardent gratitude to their noble exertions. But the efficiency of the Sisters of Charity is admitted by every medical man with whom we have conversed to have been fully equal to theirs, and their zeal and patience not to be exceeded, while, from their peculiar discipline, their ministrations were unattended by certain disadvantages which not unnaturally accompanied the interference of those occupying the position and cherishing the habits and ideas of Englishwomen of rank. We cannot allow ourselves to believe that a system like that of Rome, which proscribes the efforts of the human intellect, and lays a blighting hand on the best affections of the human heart, should succeed better than our own in developing means of usefulness and benevolence; but it is not by ignoring any real success that the Romish Church may achieve, still less by any invidious attempt to detract from the merits of females who for many a long year have practised the patient zeal and real philanthropy which so justly make Englishmen proud of the lady nurses of the Crimea, that we must seek to uphold the credit of our own views.

How far it may be possible to engraft on the system of Protestantism the benefits without the defects of Romish institutions, is a subject well worth inquiry, but upon which we cannot enter here; but we think it right in making the preceding remarks on one of the most beneficial innovations on the ordinary and accepted forms of benevolent administration—to suggest its analogy to other movements that are going on among and around us, and while we consider them, to remind our readers that we should not refuse to take advantage of any information that may be gained by the experience even of hostile religionists. Nor can we shut our eyes to the fact that on the other side of the Atlantic, in the land of eager rush, and haste, and tumult, experiments have begun to be made which must at least compel attention to the question, how far a complete scientific training for the practice of medicine may or ought to be given to women. All this will open the wider question, how much of our views about the education of women has a foundation in nature, or is necessary to preserve the delicacy and purity of feeling, on which the beneficial influence of women rests, and for the absence of which no amount of intellectual culture, and no apparent material advantages, however great, could compensate, or how far they are founded on notions purely conventional. To sum up and conclude this part of our subject, we consider the establishment of a Medical Missionary Society valuable in itself—certainly of much greater value as indicating an advance of thought, and as likely to force an advance of liberal views on the part of religious people regarding science and its place as a power co-extensive with religion in the progress of society. In dealing with barbarous tribes, individuals, or bodies of men, have been constrained by the nature of the case to seek influence by teaching and by learning themselves the useful arts, but in this case there is a distinct recognition of the necessity for a full and complete scientific training; there is an admission of the co-ordinate value of knowledge as a means of introducing and establishing religion, which is equally applicable to the outcasts of our own civilization as to the rudest savages in the world. And there is the further admission that religion addresses itself to the life which now is as well as to the life which is to come, that she takes cognizance of the whole man, moral, intellectual, and physical, and does not limit the range of her ideas, nor restrict her exertions to a future life alone, which from the very nature of a being like man, composed of a nervous system and sentient organs, can only be duly appreciated when these are sound and healthy—*mens sanà in corpore sano*. In these indications we see ground for hope that a more perfect amalgamation of science with religion may take place than has before existed, that the lofty scorn of the one may be abated, and the bitter jealousy of the other appeased, and that working together as fellow-labourers, men of science and ministers of religion may jointly labour to mitigate the sorrow and the suffering that abound, to inaugurate a healthier and better state of society, and to prepare our race for a higher and nobler existence beyond the grave.

The two books that appear last on our list are not directly connected with the subject we have been discussing, but when it is considered that the character of English faith is likely to be stamped on the opinions of the world, and that it is utterly impossible to prevent the religious opinions of the civilized part of mankind from being powerfully influenced by the views they entertain of a great many subjects not actually religious, but of kin and cognate to natural theology more especially; there is no impropriety in taking a glance at the views advocated in these books, as specimens of the tone of philosophical thought in our country at the present day. All attempts to repress speculative philosophy by treating Christianity as a foregone conclusion, have signally and most fortunately failed; nor is there the slightest reason why a single department of nature or a single object of inquiry, if it legitimately fall within the limits which our Creator has imposed upon the human faculties, should be prohibited. Every scientific inquiry that comes in contact either with the deductions of natural theology, or the intimations of Christianity, is at least a means *quoad tantum* of testing their accuracy, and we unhesitatingly affirm that from every such ordeal both have emerged stronger and more secure.

The work of Mr. Cromwell—the 'Soul and the Future'—is an avowed pleading for views purely materialistic. He regards matter as exhibiting two forms of existence—inorganic and organized; in the latter, from the lowest form at least of vegetable or animal structure life is found, which gradually increases in the energy of its manifestations, till it reaches, what he calls, the evolution of "mentality," which again enlarges the sphere of its operations in exact proportion to the complexity rather than the size of the brain, of which alone it is the product, and finally culminates in man. Mr. Cromwell is a believer in a future life after a period of decay and sleep, the thinking power being at the resurrection restored by the same high fiat which originally conferred it on the brain, though what the Deity is, if, as he comes very near saying, there be nothing in the universe but matter, he nowhere explains, and we confess to a little difficulty in conceiving. We certainly do not find very much that is absolutely new in Mr. Cromwell's statements; they are chiefly a reproduction, explicit, honest, and straightforward, of older writers, especially of the views of Dr. Priestly and Mr. Lawrence, respecting the progressive growth and gradual decay of the human faculties, along with the development and decay of the physical frame; nor do we consider him at all successful in his attempts to derive support to the cause he adopts from recent anatomy. The author certainly overstates his case when he affirms that all vital phenomena may be explained by the ordinary forces that operate on inorganic matter, and that the notion of a principle of life controlling, modifying, and even occasionally reversing ordinary chemical actions, is *altogether* obsolete. Nor does microscopic anatomy help us, in the slightest degree, to explain or to conceive how the brain elaborates thought. The brain, we are willing to admit, may and probably does consist of a congeries of diverse organs adapted to exhibit or produce different states of the mind; further, so far as we know, its intimate structure consists of granules, cells, and tubules, analogous to the constitution of other organs; but having admitted this, we do not see how Mr. Cromwell's views are in the least helped by the admission. The brain may be just as well the instrument of an immaterial entity as the actual thinking principle itself; and the latter hypothesis is encumbered and opposed by all those difficulties which have caused it to be held untenable by certainly a vast majority of the greatest and most acute thinkers that we recollect. If matter be all that we are capable of conceiving, or all that we actually know, and thought be a product of the brain (and no mode of expression does anything more than attempt to disguise what the materialists must admit, that thought is a product of brain), which after all is nothing but matter, how is it possible to escape the conclusion that thought itself is material; and let Mr. Cromwell, or any one else, try to apply material laws to the wondrous evolution and endless complexities of thought—to the brilliant imaginings of the poet—to the profound speculations of the sage—to the visions of hope, or to the gloom of despair—to the restlessness of ambition—to the placidity of content—in a word, to the endless complications of feelings, and passions, and hopes, and desires, and fears, that agitate the life of man, and never cease till we have "shuffled off this mortal coil," we believe and feel sure that the thing is utterly impossible, and therefore willingly hold to the old and all but universal belief that our mortal tenements of clay are the habitations of something higher and better than themselves.

But even supposing that the vague language which materialists employ could conceal from us the fact, that on their hypothesis it seems inevitable that every thought must be a material product of some kind or other, it must be held that if a particular arrangement of the particles of matter gives origin to thought, or "mentality" in Mr. Cromwell's language, it is a process different from, and contrary to, any analogy or fact of which we have knowledge throughout the rest of nature. The supporters of such a doctrine must maintain that this arrangement both forms a new substance—an organized body—and something quite different from, and additional to, either the particles taken separately, or the organized body itself—the sum of them combined together.* Further, while it does not seem possible to conceive any arrangement of material particles by which mental processes are evolved, only grant the existence of these mental processes, which it is impossible to deny, because the fact is forced on us by every act of consciousness, and we find it possible, at least, to devise hypotheses which will account for all the phenomena we call material. And this naturally conducts us to the last book on our list, 'Man and his Dwelling Place,' where something like this is vainly attempted.

'Man and his Dwelling Place' is an extremely thoughtful, ingenious, and amiable book. It appears to have been written with the view of reconciling certain theological opinions, and obviating certain theological difficulties, by a view of the nature of the material world in which we live; which, after all, and in spite of all the author's ingenuity and learning, which are evidently very considerable, slips through our fingers, and baffles all our attempts at comprehension. Matter, according to our author, is not inert, but spiritually active; the perceived inertness is merely phenomenal, and due to the defectiveness or deadness of man. Language like this may admit of three interpretations; it may either involve the old pantheistic notion of the *Anima Mundi*, which the evident piety of the author forbids us to believe it can mean; or it may mean litttle more than language in ordinary use among certain religionists implies—the evanescent character and utter unsatisfactoriness of all terrestrial things, and that their power over us (the power of the phenomena, in our author's language) results only from the spiritual deadness of man—a sense in which the author seems occasionally to employ it; or it may mean a *tertium quid*, but what that is, we confess that we have laboured honestly, but all in vain, to make out.

> "Man feels that which is apart from him to be inert, not because it *is* as he feels it, but because of his own condition; if his feeling were true, he would feel himself in presence only of existence that is spiritual, it is through a want in him that his feeling is caused to be untrue."†
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> "**Nature is not truly, and in itself, such as it is to man's feeling; that which man feels to be differs from that which is apart from him by defect.**"

Such passages as these occur everywhere in this book, they are multiplied, extended, and ramified over all the subjects that can well be considered as related to philosophy and science; but the continually recurring idea is, that all our notions are wrong, because we mistake the phenomenal for the real—that which *appears* for that which *is*, and thus by reason of defect or deadness in ourselves we see only a physical or inert world, or nature or universe around us, where, if we were alive, as God and the Bible intend to make us, we should behold the whole of Nature to be full of true spiritual activity. It must be quite evident that there is a great deal of verbal quibbling in all this. We do not mean this in any sense disparaging to our author, who is evidently a man of perfectly upright purpose, as well as of great compass both of mind and information. But everybody knows, who is at all conversant with such subjects, how difficult it is to find language sufficiently exact to express the subtle ideas which such writers originate and seek to diffuse. And, on the other hand, how easily a man may mislead or bewilder both his readers and himself, by language used in a sense different from its common acceptation, and which may be employed to denote sometimes one thing, sometimes another. It is impossible to go over a book whose scope is so wide, and whose subjects are so varied. We have chapters on the most important topics of science, of religion, of philosophy, and of ethics; besides five Dialogues, in which the leading ideas are reproduced, and extended, and placed in different points of view. The whole is written in a grave, earnest spirit, and with a deep and melancholy sympathy with humanity, especially in relation to the insoluble problems by which it is beset, and which, in spite of the author's ardent anticipations, we fear he leaves unsolved and unsolvable still. His tone has a great charm for minds of a certain character; but instead of attemping to follow him, we will, as a contrast to our last writer, Mr. Cromwell, very shortly notice his chapter on Idealism.

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* Vide Lord Brougham's Dissertations on Natural Theology.
† Dialogue IV. p. 409.

If we turn from 'Man and his Dwelling Place' with its tone of grave, almost sad, eloquence, to the cool, calm, subtle reasoning of the metaphysical supporters of Idealism, or to its refutation by its opponents, from good, old, sagacious Dr. Reid downwards, we cannot fail to be struck with the prodigious difference. Our author evidently looks with kindness on any speculation that appears to invalidate the trustworthiness of the human faculties, because this seems to favour the reception of his favourite dogma; indeed, he seems only, or chiefly at least, to regard idealism as a necessary step in the progress of the human mind, to the evolution of the great truth—the spiritual activity of the universe.

But, let us ask him, if the reasoning of the Idealists be adopted, where are we to end? will there not be many beliefs subverted, and many views overthrown, which he and we alike would devoutly wish to maintain? Nay, is there any resting-place for the sole of our foot short of a universal and dreary Pyrrhonism? The author's favourite illustration, which is, that the moon appears to us a luminous disc in the bright nocturnal sky, and that the brilliant stars, which are "the poetry of heaven," appear like shining points glittering in brightness above us; yet, that the one is an opaque mass, and the others are the centres of vast systems, can stand him in no stead either in the support of Idealism, or of his own still more incomprehensible views, unless he can show where the alleged defect in man lies, wherein it consists, and how it operates. Astronomers have shown how, from the laws of light and the influence of distance, the aspect of the heavens must be what it is, and can be nothing else. Let him give us clear definitions of what he means, statements of unquestionable fact, and intelligible processes of reasoning similar to what astronomers have furnished us with in their case, and then we may feel that he has made such progress as to require consideration. But for anything of this kind we have searched the book, attractive and interesting in tone and spirit as we willingly admit it to be, but we have searched in vain.

The hypothesis of the non-existence of matter rests upon consequences logically deduced from certain assumed views of the mode in which external objects are presented to the mind. And its overthrow was effected by showing that these views had no foundation in fact, but were themselves entirely gratuitous and hypothetical, by giving literally a purely inductive account of the faculty of perception. No philosopher of the Scotch or Common Sense School, as it has been called, ever thought of offering any logical proof of the existence of matter, but maintains its existence to be an inevitable and irresistible conviction of our nature, anterior to any reasoning whatever. It is quite beside the mark, therefore, to complain as our author does, that the Idealistic logic has not been successfully assailed. The objections lie not to the logic but to the postulates on which it professes to rest; and the affirmation of those who, keeping to their natural sense of things, maintain the existence of a material universe, is that their belief is a necessary and fundamental intuition of our nature—that it is universal—that it can neither be assailed nor defended by principles more evident than itself—that even those who profess to doubt or deny it share the common belief, or at least are constrained to speak and act as if they shared it. These, we think, are the tests of an intuitive principle of belief laid down by Father Buffier, an old, but very acute writer. At all events there seems no one principle of our nature to which they apply more perfectly than the one we have been thus shortly discussing.

But so eager is our author to obtain the alliance of any view or principle from which he thinks he can derive aid, that not only does he look with favour upon Idealism, but he has a good word to say for Positivism, which, with considerable ingenuity, he strives to make out to be a kind of extreme Idealism, affirming strenuously that Positivism, which professes to deal only with relations, ignoring the absolute, the real essence of things, has not any tendency to Atheism, and maintaining it to be the very reverse of Materialism. The chapter on Positivism, though short, is even more obscure than most of the book, and towards its close, the author almost seems to repent of his opening eulogiums, and points out in strong language how the fruits of this system, fair and promising though they be, like those of Pandemonium in Milton's glorious poem, turn to ashes to our taste. The same fatal objection lies against Positivism as against Idealism —the ineradicable conviction that our powers and faculties are truthful and trustworthy; if we deny this either in terms or in any way, however circuitous, there is no rest for the sole of our foot. The moment you have proved, if it were possible, the untruthfulness of our intuitive convictions respecting Causation, you have likewise annihilated the basis of the very principles on which your philosophy rests.

The attempt that is frequently, we might say constantly, made to extend the notion of mere antecedence and sequence, as being all that we perceive in Physical Causation, to a full account of the whole doctrine of cause and effect, appears to us to be an attempt to take possession of the whole battle-field on which the principles of Natural Theology must be maintained, or impugned, during a time of truce. Nobody doubts that, so far as physical phenomena are concerned, antecedence and sequence are all that we perceive; the power of the antecedent to produce the sequence is not seen or known previous to experience; but to deny the idea of power, or to affirm that mere priority of occurrence is all that mankind mean by cause, not only subverts the foundations of religion, but is a statement utterly at variance with those fundamental principles of belief which are utterly ineradicable from the human mind, and any tampering with which ultimately lays the axe to the root of even those principles which Positivism accepts, and on which her conclusions rest. We do not think any supposed compatibility of Positivism with the author's favourite speculation will induce us to look with favour on its principles, or to regard them as necessary parts of the training we are to undergo for the reception of his favourite speculation. In Sir William Hamilton's very able but fragmentary and difficult Essay on the Unconditioned, some steps are taken, and some principles are evolved, which might help us a little to a view of the real limits of our inquiries imposed upon us by our nature in speculative philosophy, especially where it is conterminous with natural theology; but the style of that very acute and able thinker is so arid, and does so bristle with technicalities and logical forms, that his views, where they are perfectly sound and true, will be long in being fully mixed with the mighty stream of popular opinion, and influencing it as their value and importance entitle them to do; besides, with reverence we must say, that, deep as our obligations are to Sir William Hamilton, for restoring a sound philosophy to our confidence, and for expanding it, even he himself has scarcely escaped what we must venture to call the contamination of German mystification and vagueness.

This is not the place to discuss such matters, but we cannot forbear alluding, in our conclusion of this article, to the very great importance of attending to and settling, so far as can be done, the limits of religious and philosophical speculations as a means of restraining minds often of a high order within just bounds. When these are passed, when we abandon, what Bishop Butler calls, our natural sense of things, when we quit those fundamental laws of belief which lie at the bottom of all our knowledge, convictions, and faith, when we leave these, especially when we proceed on the assumption that the mind of man does not truly respond to the facts of nature, we get on insecure and dangerous ground; and however beautiful, and however devout in appearance, the speculations that may follow, the instability of the foundation on which they rest exposes them to be subverted and replaced by speculations as baseless as themselves, and as plausible, though possibly of a character utterly opposite.

REVIEW V.

1. *A Treatise on Human Physiology; designed for the Use of Students and Practitioners of Medicine.* By JOHN C. DALTON, JUN., M.D.—*Philadelphia*, 1859. pp. 608.
2. *Outlines of Physiology.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine, and Senior Professor of Clinical Medicine, in the University of Edinburgh.—1858. pp. 247.

WE recently* reviewed two foreign works on physiology; one a "Treatise," in French, by Monsieur Longet; the other a "Textbook," in German, by Dr. Funke. Each of these works having given us a tolerably clear idea of the present state of physiological science in the countries to which their authors respectively belong, we wish now to introduce to the notice of our readers two other works on the same subject, which, although written in our mother tongue, are nevertheless the representatives of two different nations. The first of these works is a large volume of some six hundred pages, by Dr. Dalton, the Professor of Physiology in the College of Physicians and Surgeons, New York. The last is a small book by Professor Bennett, of Edinburgh. As these two works bear as little internal as external resemblance, we shall not attempt to review them conjointly, but will give a separate outline of each.

Until within a very recent date, American works on Physiology were almost entirely unknown in Europe—a circumstance solely due to the fact of their being little else than crude compilations of European works. Within the last few years, however, a great change has taken place for the better; and our friends on the other side of the Atlantic can now boast of possessing manuals equalled by few, and excelled by none, of our own. In Dr. Dalton's treatise we are glad to find a valuable addition to physiological literature. It is well illustrated by woodcuts; and although strictly speaking a work on human physiology, the author has very judiciously given illustrations of the vital phenomena, as learned by experiment and observation on the lower animals. With pleasure we have observed throughout the volume proof of the author not being a mere compiler of the ideas of others, but an active labourer in the field of science. It was certainly, however, not by reading the first chapters that we came to this conclusion; for in them we found little else than a recapitulation of Robin and Verdeil's views on the constituents of the human frame. We regretted to find also that Dr. Dalton had omitted, until he reached his thirty-fifth page, to acknowledge the source of the views he had adopted. After devoting nearly four chapters to the consideration of the three groups of proximate principles which compose our bodies, our author passes on to the subject of food; and while on the influence of diet, he cites the interesting experiment made by D. W. A. Hammond, one of the Assistant-Surgeons in the United States Army.

Dr. Hammond, a most zealous searcher after truth, has several times subjected himself to experiment; and on the occasion of that above alluded to, he restricted himself during ten days to a diet composed exclusively of boiled starch and water. After the third day his general health began to deteriorate, and before the termination of the experiment, it became very much disturbed. The symptoms of disorder manifested themselves by debility, headache, pyrosis, and palpitation of the heart; and even after the starch diet was abandoned, it required several days to restore to him the normal standard of health. Here is a fact worthy the consideration of our hospital and dispensary physicians, who are constantly called upon to treat by medicine the very same class of symptoms in their half-famished patients, when the soup kitchen, instead of the doctor, should have been applied to.

While perusing the chapter upon the function of the spleen, we came upon a statement which rather surprised us; and to prevent our misinterpreting it, we quote our author's own words:

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* British and Foreign Medico-Chirurgical Review. April, 1859.

> "Another symptom which usually follows removal of the spleen, is an unnatural ferocity of disposition. The animal will frequently attack others of its own or a different species, without any apparent cause, and without any regard to the difference of size, strength, &c. This symptom is sometimes equally excessive with that of an unnatural appetite; while in other instances it shows itself only in occasional outbursts of irritability and violence." (p. 176.)

We have had so many opportunities afforded us of observing animals after removal of the spleen, without having ever noticed any unusual ferocity manifested by them, that we should like very much to know how the above idea originated. That the temper of an animal should depend upon the presence or absence of its spleen is to us an enigma. The change of temper of the animals alluded to by Dr. Dalton may have perhaps been due to some other cause than the mere extirpation of the organ in question. We at least have never observed it. At this very moment we have in our possession an animal from which the spleen was removed nearly two years ago; and although it has been constantly under our observation during that time, we have not seen it manifest any peculiar ferocity of disposition, either towards those of its own or of a different species. Dr. Dalton further states, that dogs, after their spleens have been extirpated, will eat the flesh of their fellows. Such may be the case with American dogs; but it certainly does not usually occur with our English ones, for we have occasionally, for reasons of economy, tried to make them eat the flesh of their fallen friends without success.

We must now pass on to the chapter on Secretion, which is replete with interesting matter. Our author tells us that, besides the process known by the name of "assimilation," there is another, somewhat similar to it, which takes place in the different glandular organs. To this the name of "secretion" has been applied. The object of the latter process is to supply certain fluids that are necessary to the performance of the various physical and chemical actions occurring in the animal organism. These secreted fluids vary in consistence, density, colour, quantity, and reaction—some of them being thin and watery, like the tears and the perspiration; others viscid and glutinous, like mucus and the pancreatic fluid. They may be alkaline, like the saliva; acid, like the gastric juice; or neutral, like the bile. Each secretion contains water and the inorganic salts of the blood, in varying proportions, and is distinguished by the presence of some peculiar animal matter which does not exist in the blood, but which has been produced by the special secreting action of the cells of the glandular organ. A true secretion is only produced in its own particular gland, and cannot be formed elsewhere, since the glandular cells of that organ are the only ones capable of producing its characteristic ingredient. Thus, pepsin can only be formed by the gastric glands, pancreatine by the pancreas, tauro-cholate of soda by the liver.

Again, our author remarks, that one secreting gland can never perform vicariously the office of another. The instances which have been from time to time reported of such unnatural action are not (he says), properly speaking, instances of "vicarious secretion," but only cases in which certain substances, already existing in the blood, have made their appearance in secretions to which they do not naturally belong. Thus, cholesterine, which is produced in the brain, and taken up from it by the blood, usually passes off with the bile; but it may also, under certain conditions, appear in the fluid of a hydrocele or ovarian cyst. Sugar, again, is produced in the liver, and taken up by the blood; and when it accumulates in large quantity in the circulating fluid, may pass off with the urine. The colouring matter of the bile, in cases of biliary obstruction, may be reabsorbed, and afterwards make its appearance in the serous fluids or the perspiration. In these instances, however, the unnatural ingredient is not actually produced by the kidneys in the one case, or the perspiratory glands in the other; but is supplied to them, already formed by the blood. Cases of "vicarious menstruation," Dr. Dalton says, are simply capillary hæmorrhages taking place from various mucous membranes, in consequence of the disturbance of the circulation in amenorrhœa. A true secretion is always confined to the gland in which it naturally originates.

Glandular organs have not even an equal power in secreting foreign soluble substances, which may have found their way into the blood. Thus it has been shown that ferrocyanide of potassium, when injected into the jugular vein, although it passes off with facility by the urine, does not appear in the salivary secretion. It has been further shown, that a solution of this salt may be injected into the very duct of the parotid gland, absorbed by the blood, and discharged with the urine, without ever having appeared in the saliva even of the gland into which it was injected. Curiously enough, if, on the other hand, the iodide be substituted for the ferrocyanide of potassium, we then find that the salivary glands have an equal facility with the kidneys in secreting it. The process of secretion is seen, therefore, to depend upon the peculiar anatomical and chemical constitution of the glandular tissue; and it is unreasonable to suppose that we can force upon the cells of one organ the office of those of another.

No one can reflect without astonishment on the immensity of the secreting surface which our bodies present. Take, for example, the sweat glands, the whole number of which in an adult of average size is supposed to be 2,300,000, and the length of each of these tubes one-fifteenth of an inch. The entire extent, therefore, of this glandular tubing, is not less than 153,000 inches, or about two and a half miles. When we think of this we can easily understand how labourers employed in gas works lose 3½lbs. weight by cutaneous and pulmonary exhalation in less than an hour;* and how great a disturbance may be produced in the system by the sudden arrest of this secretion.

While on the subject of secretion, our author has made a few interesting, although not original, remarks upon creatine and creatinine (p. 287), which we may briefly lay before our readers. Creatine is a neutral crystallizable substance, supposed to be formed in the muscles, from the juice of which it may be obtained in the proportion of 0·70 parts per 1000. It is found in the blood and urine. In the latter of the human subject it exists in the proportion of about 1·25 parts per 1000. Strong alkalies, with the aid of heat, have the power of converting creatine into urea and sarcosine. Strong acids, on the other hand, change it into creatinine, a substance very nearly allied to it, and also occurring in the muscles, blood, and urine. Creatine being found in greatest quantity in the muscles, and creatinine in the urine; the latter is supposed to result from the decomposition of the former.

Having said this much regarding the treatise of Dr. Dalton, we shall leave it for the present, and turn our attention to Dr. Bennett's book.

The 'Outlines of Physiology' form a small volume, possessing more of the character of a synopsis than of a treatise on physiology. One of our contemporaries, indeed, tells us that 'The Outlines' were originally written as the article Physiology in the new edition of the 'Encyclopædia Britannica,' and that "they are now published separately in a small volume, to serve as a text-book for the author's lectures in the University."

The work is not without a certain degree of merit; but we are unable to regard it as an improvement on any of the author's previous labours; we should have expected something better from the pen of Dr. Bennett, and we regret to find that there are various parts with which our critical duties compel us to express dissent. The high opinion which we have repeatedly expressed regarding our author's labours in the field of medical science, renders it the more imperative upon us not to pass *sub silentio* his shortcomings in the present volume, especially as it is intended for the student.

The first point in the 'Outlines' with which we are inclined to find fault, is the author's mode of classifying the tissues. In "Part First," p. 14, the elementary textures of the animal body are described under the four following heads:—1st, Molecular Tissues; 2nd, Cell Tissues; 3rd, Fibre Tissues; and 4th, Tube Tissues. To divide histological elements into molecules, cells, fibres, and tubes, would be natural enough; but to attempt to divide tissues in a similar way appears to us open to many objections. Are all textures not ultimately made up of molecules? Dr. Bennett himself answers the question:

> "Organic formative fluids deposit molecules, which arrange themselves, subject to vital laws, into nuclei, cell-walls, and higher textures. These once produced, subsequently decay in an inverse order, breaking down into individual fragments, and ultimately into minute molecules. . . . . Hence the first and last element is, as regards form, molecular." (p. 17.)

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* Dr. Southwood Smith: Philosophy of Health, chap. 13.

Under the second head, Cell Tissues, we find lymph and nerve cells, pigment and fat cells placed together. The third, Fibre Tissues, sub-divides into, 1st, Molecular Fibres =blood-clot; 2nd, White Areolar Fibres=tendon; 3rd, Elastic Fibres=ligamentum nuchæ; 4th, Epidermic Fibres=hoofs and horns; 5th, Non-voluntary Contractile Fibres=involuntary muscle; 6th, Voluntary Contractile Fibres=voluntary muscle.

His division of tube-tissues we also dislike, for although they have one character in common, yet they differ so widely in the others, that we think it would have been more advisable to have selected some other mode of arrangement. The division, as it at present stands, forces one to group together the air-conducting trachea with the liquid-transmitting aorta, the soft and sensitive neurilemma with the hard and insensible ivory.

We shall briefly point out some of the statements with which we disagree. Firstly, while speaking of the chyle and lymph cells, our author tells us that, "These are blood corpuscles in an early stage of development, being for the most part free nuclei, only a few having cell-walls around them." (p. 18.)

On reading this paragraph, any one little versed in the doctrines of physiology, would naturally suppose that the mode of development of blood-corpuscles was perfectly understood; and that the transformation of chyle and lymph cells into them was demonstrable to one or other of the senses. Such, however, is not the case. Between chyle and lymph there is generally supposed to be a wide gulf. The former being regarded as the nutritive fluid furnished by the digestive process to nourish the frame; the latter the effete matters, given up by the tissues in a fluid state, after having performed their allotted labours in the animal economy. That chyle cells are *immediately* transformed into blood corpuscles, and that lymph cells are free nuclei in the course of development into blood corpuscles, is therefore a theory. We know as yet so little regarding the manner in which red corpuscles are formed, that we may be wrong in our criticism. We shall therefore not dwell upon a purely hypothetical point, but pass on to the consideration of some of the statements of facts contained in the book, which of course ought only to admit of interpretation.

At page 19 it is stated, that the contents of the white cells of the chyle, when brought in contact with the oxygen of the air, while passing through the lungs, become red. We should like to know on what authority this statement is made. For we are unacquainted with any facts that can be brought forward in favour of it; while, on the other hand, we know for a certainty that chyle cells may be exposed to a stream of air containing the usual per-centage of oxygen without their contents becoming red.

At page 29, white fibrous tissue is said to offer "an elastic medium and support to the frame generally." We understand very well what is here meant by an elastic medium; but we cannot help saying that we think the term is one liable to confuse, if not altogether to mislead, the student as to the true characters of white fibrous tissue; tendon and ligament being generally looked upon as typical specimens of white fibrous tissue, and the student being usually taught to regard them as non-elastic and inextensible structures.

We also object to another statement made a little lower down in the same page. It is—"the most important property of the fibrous tissues, however, is that of contractility." Now, we think that the student will have some difficulty in comprehending this statement, if he remembers that he was told, not many pages before, that hoofs and horns are "fibrous tissues;" previous experience having probably taught him that their chief property is not that of "contractility."

We shall now leave the histological portion of the volume, and turn to that devoted to physiological chemistry.

At page 36, we find it stated that "sodium in the condition of soda, or common salt, gives alkalinity to the humours, and fluidity to the blood." We have here some difficulty in comprehending the meaning of our author. The first part of his sentence leads one to suppose that soda and common salt are identical; while the second part of it conveys the idea that common salt is the substance which gives alkalinity to the humours of the body. Either our knowledge of physiological chemistry is very imperfect, or our author has ill explained his meaning. Because, firstly, we have been taught to regard soda and common salt as entirely different substances, the one being the oxide, the other the chloride of the metal sodium; secondly, as common salt or the chloride of sodium is a neutral body, it seems to us that it cannot possibly have the power of giving alkalinity to the humours.

At page 38, we are told that sulphur passes from one region to another in a similar manner, from the sea, which contains sulphur in large quantities, to the atmosphere. That sulphur passes from the sea to the atmosphere is a fact entirely new to us, we may therefore be excused asking the question—"In what form, and by what physical law, does this transposition take place?" Surely the author cannot mean that the transition of sulphur from the water surrounding our globe to the atmosphere, takes place in the form of sulphuretted hydrogen, for it is well known that the sea as a whole yields not a particle of that gas.

We shall now turn to the chapters on Organic Chemistry, and point out those parts in it with which we do not entirely agree. The first statement which we are not prepared to endorse is, that "fibrin forms nearly the whole substance of the muscles." If by the word fibrin our author means that substance which coagulates in drawn blood, we think it would have been perhaps better had he somewhat qualified the above statement, for although muscles are composed of a substance possessing many of the characters of fibrin, the two are nevertheless far from being identical.

On examining the classification of the different kinds of food adopted by our author, we cannot say that it appeared to us to be a good one. For in his classification he has entirely omitted to mention the saccharine group, merely including the albuminous, fatty, pigmentary, and mineral. We have read over his remarks on the various foods with great care, nevertheless we are at a loss to account for the above omission, unless we suppose that he has replaced the saccharine by the pigmentary group of foods.

At page 40, we are informed that the mere union of a little oil and albumen is all that is required for the formation of tissue—

> "The development of a young animal from an egg (our author tells us), is a good illustration of this fact. It contains only albumen, and a yellow fat, with some traces of iron. Yet we see in the process of incubation, during which no foreign matter except atmospheric air can be introduced, that feathers, claws, blood corpuscles, fibrin, cellular tissue, and vessels are produced."

We do not know how to regard this statement, for our author cannot possibly mean to tell us that the earthy salts which compose the bones of the chick are developed out of a mixture of albumen and fat, with some traces of iron. As our space is nearly exhausted, and we have not yet analysed more than a quarter of the little book, we must confine the remarks which we would otherwise make in regard to the next division, to raising an objection to the statement made at the bottom of page 53 regarding the action of the atmosphere. It is there remarked that: "If cold and condensed, there is more oxygen, which will unite with the tissues during respiration and produce more waste, while greater evaporation will take place from the surface."

It appears to us that instead of the word "greater," the word "lesser" ought to have been used; for we had the idea that the evaporation diminished in proportion as the surrounding air became colder and more condensed.

In the physiological part of the volume, at p. 66, in speaking of the blood-glands, the author says that, "in infancy and early childhood the thymus and supra-renal capsules are large and active; they then decline, and almost disappear in man." We are surprised at this statement, for it is now well known that although the thymus diminishes in proportionate size and activity as age advances, such is not the case with the supra-renal capsules. The latter organs do not even so much as become proportionally more atrophied in old age than many of the other persistent internal organs.

At p. 74 we again meet with another statement which we cannot let pass without a word of remark. The cranium is there compared to a pneumatic trough. "Hence the notion, that by general or local bleeding you can draw blood from the brain, is erroneous, although by weakening the action of the heart it is of course possible to diminish the pressure it exercises on the cerebral vessels." We are perfectly aware that in the

Edinburgh school the author's predecessors taught, like teachers elsewhere, this doctrine; but we were not aware until we read the above passage that a doctrine which had been proved to be incorrect still found supporters north of the Tweed. Experimental physiology has clearly pointed out to us that the cranium cannot be compared to a pneumatic trough, and that the amount of blood circulating in its vessels is liable to be influenced by the same agents that act upon the circulation of the other organs of the body.

Two pages further on, when on the subject of respiration, our author informs us that oxygen is absorbed and carbonic acid exhaled by the lungs in accordance with Graham's law of the diffusion of gases. The author seems altogether to have forgotten that Graham's law of the diffusion of gases only operates when the membranes through which the gases pass are perfectly dry. When they are moist, as we find them in the air vesicles of the lungs, an entirely different law comes into play, namely, the law of absorption.

At page 83 the following sentence occurs :—" The blood corpuscles of which we have previously spoken float in a stream of coloured transparent fluid (the liquor sanguinis), which, when it ceases to circulate in the vessels, has the property of coagulating." While a little farther down in the same page, we are told that " the clot of the blood, therefore, is composed of the fibrin and corpuscles, while the serum is set free." And again : " In addition to the fibrin, the liquor sanguinis holds in solution albumen, fat, &c." How are we to reconcile these statements? At one place we are told that the liquor sanguinis has the power of coagulating, at another that it does not coagulate, but that one of its constituents possesses the property of doing so. Such statements as these must be very confusing to the student. We shall content ourselves with one more quotation. At page 90, while speaking of the formation and destruction of sugar in the animal body, the author tells us that the sugar " is decomposed by the oxygen of the air in the lungs, and there disappears." This is a statement which rather surprises us, considering the source from which it comes. For within the last two years, experiments have been published, both in England and abroad, which clearly demonstrate that the sugar does not become decomposed in the lungs, but that it disappears in the capillaries of the general circulation.

Although we have spoken disapprovingly of numerous passages contained in Dr. Bennett's Outlines, we do not wish to disparage the entire work. Had it been written by a man of an inferior stamp it might have passed without much observation, but as more might have been expected of an original thinker and searcher like Dr. Bennett, we should have been guilty of a dereliction of duty to our readers had we not pointed out what we consider to be its blemishes.

REVIEW VI.

1. *Clinical Illustrations of the Pathology and Treatment of Delirium Tremens.* By THOMAS LAYCOCK, M.D., &c. ('Edinburgh Medical Journal,' No. 40, Oct. 1858, p. 289.)
2. *The Pathology of Delirium Tremens, and its Treatment without Stimulants or Opiates.* By ALEXANDER PEDDIE, M.D., F.R.C.P., &c.—*Edinburgh*, 1854. pp. 51.
3. *Statistics of Delirium Tremens.* By DRS. MACPHERSON and ATKINSON. ('The Indian Annals of Medical Science,' Nos. 5 and 6, pp. 1, 658. Oct., 1855; April, 1856.—*Calcutta.*)
4. *Observations on the Use of the Shower Bath in Delirium Tremens and other similar cases of Cerebral Excitement.* By ROBERT LAW, M.D., &c. ('The Dublin Quarterly Journal of Medical Science,' No. 49, p. 33. Feb. 1858.—*Dublin.*)
5. *Remarks on the Treatment and Pathology of Delirium Tremens.* By CHARLES MOREHEAD, M.D., &c. ('Transactions of the Medical and Physical Society of Bombay,' Nos. 6 and 9, 1843, 1849. pp. 139, 123. 'Clinical Researches on Disease in India.' Vol. ii. p. 530.—*London*, 1856.)

6. *Observations on the Treatment of Delirium Tremens without Opium.* By T. Cahill, M.D. ('The Dublin Journal of Medical Science,' vol. xv. p. 396, 1839.
7. *Cases of Delirium Tremens, with Commentary.* By John Hughes Bennett, M.D., &c. Clinical Lectures on the Principles and Practice of Medicine. pp. 409. Second Edition.—*Edinburgh*, 1858.
8. *Meningitis Phantasmatophora; Brain Fever of Drunkards.* ('Elements of the Practice of Physic.' By David Craigie, M.D., &c. Vol. ii. p. 50.—*Edinburgh*, 1840.)
9. *Remarks on the History and Treatment of Delirium Tremens.* By John Ware, M.D.—*Boston*, 1831.
10. *Vergiftung durch Alkohol und Alkoholische Getränke* (*Alkoholismus. Morbi ex nimio usu et abusu Alkoholicorum*). Von Dr. C. Ph. Falck, zu Marburg. ('Handbuch der Speciellen Pathologie und Therapie.' Zweiter Band. Erste Abtheilung s. 293. Intoxicationen, Zoonosen, &c. Redigirt von Rud. Virchow.—*Berlin*, 1855.)

Poisoning by Alcohol and Alcoholic Drinks. By Dr. C. Ph. Falck. (Handbook of Special Pathology and of Therapeutics. Edited by Rud. Virchow. Vol. ii. p. 293. —*Berlin*, 1855.)

A well known writer, not less remarkable for his paradoxes than for his taste and eloquence, observes,[*] that

> "He never met with a question yet of any importance which did not need for the right solution of it at least one positive and one negative answer, like an equation of the second degree. Mostly matters of any consequence are three-sided or four-sided, or polygonal, and the trotting round a polygon is severe work for people any way stiff in their opinions. For myself, I am never satisfied that I have handled a subject properly until I have contradicted myself three times."

Without any desire to keep Mr. Ruskin company in the last mentioned laudable endeavour, we purpose testing ourselves by one of his trials—i.e., by reconnoitring some of the sides of the polygon within which the subject of *delirium tremens* lies entrenched. Some portions of the fortification we have long considered dreadfully weak, and we have at length made up our mind to play upon them a little of our artillery. We take the privilege of selecting, of course, our own points of attack, declining to invest the whole circuit of circumvallation. We therefore state, *in limine*, that with the psychical, maniacal, forensic, and social relations of delirium tremens we shall have nothing to do, but will confine ourselves to some portions of its more material pathology, and the question of its treatment. We are satisfied that upon these matters our views have been—as the milliners say—cut on the bias; and the tendency of the prevailing doctrines concerning the heterodoxy of depletion and the orthodoxy of brandy and water in the treatment of disease generally, is to uphold the fashionable falsities we would here expose. In doing so, it may be possible that we speak unpleasantly dogmatic; but it should be remembered that to blend the history of the present with the politeness of drawing-rooms, is not easy—praise may be thrown back as impertinence—blame will be as an insult, revenged.

Since the time when the opinions of Abercrombie, Bright, Guldberg, Frank, Speranza, Andreæ, Craigie, and others, who regarded delirium tremens as symptomatic of some modification or variety of meningitis, went out of favour, the more generally received view of its nature has been such as is expressed by Dr. Watson, when he replies to his pupils: "You ask me what is the essential nature of the disease, and I can only state in reply, that it consists in *nervous irritation* . . . . nervous *exhaustion* goes along with and augments the nervous irritability."[†] This view of it being the result of irritation and exhaustion of nervous power from excessive stimulation, has been associated with the belief that an identically similar state, as the *delirium tremens à potu*, may be seen to follow the abuse of opium, tobacco, and from extreme mental and emotional excitement. The opinion has also prevailed that it is the sudden withdrawal of the accustomed stimulus that generally constitutes the immediate exciting cause of the outbreak of the affection. Thus has resulted what seemed a legitimate corollary—viz., that the proper treatment for the malady consisted, on the one hand, in the continued administration of alcoholic stimulants, and on the other, "the great remedy is *sleep;* that.... opium must be given in full doses, and it must be fearlessly repeated if its desired effect do not follow."* Now, speaking generally, we regard these several doctrines as essentially erroneous, and that both theoretic teaching and practical experience compel us to view the pathology and therapeia of delirium tremens in a very different light than the before-mentioned teaching implies. It might be fairly asked of us to explain how it happens, then, that for nearly half a century such doctrines have been so very widely taught and accepted, both in this country and elsewhere? We would reply, in the first place, the exact nature of the affection has not been generally recognised; secondly, the course and progress, or the "natural history," of it has not been attended to; thirdly, that from the student of medicine drawing his hospital experience far more abundantly and continuously from the surgical than from the medical wards, he has entered upon "practice" with a one-sided knowledge of the malady—viz., as occurring after *shock* to the nervous system of the intemperate man who has suffered injury, accident, or operation. Such student will necessarily be inclined to continue *jurare in verbo magistri*, and with which he will find the prevailing dogma of "general stimulation" admirably to square. With respect to the latter statement, it may be observed that since it is in the surgical wards that (as we with all deference think) the *excessive* use of alcoholic excitants is chiefly seen, the student is led to consider gin and bottled stout to be almost panaceas, to which may not unfrequently be added, with unsparing hand, that "queen of all medicines," as Hofmann calls it—opium. The differences between certain diseases of the same names, occurring idiopathically and in patients after accident or operation, followed by suppuration, sloughing, &c., are just as little recognised by the student in the cases of erysipelas, pneumonia, &c., as they are in delirium tremens uncomplicated with and complicated by "shock." The result is that the therapeia of the one side is brought to bear likewise upon the other. No doubt but that the highly stimulating and supporting method of the surgeon is often admirably fitted to his peculiar cases, but equally plain is it to our minds that it requires sometimes not a little "toning down" in its application to the idiopathic forms of disease under the physician's care. If in times gone by the lancet was the cure for everything, in modern days we are not less exaggeratingly trustful in wine, brandy, and beer. We can "blow hot and cold" now just as readily as was done then. It has been stated,† under circumstances of some pretension, that we have taken "narrow views of disease, and that we are diverted from the course which a sounder pathology would dictate by small indications which ought to merge into general principles—that we complicate our ordinances with a multiplicity of detail."‡ When we narrowly inquire in what "broad views of disease" (p. 41) consist, we find, *quoad* treatment —e.g., that ladies who have no appetite for breakfast, should take "a glass of rum and milk as a substitute" (p. 47); that maidens who rise too early from their beds, can have "healthy sleep prolonged, to a term regulated by the requisitions of the system," by "steel, bark, and mulled port wine at bed-time" (p. 52); whilst a solicitor or seamstress, who sleeps too much, can at once be relieved by taking "a glass of rum and milk at seven A.M., and adding two glasses of port wine to the usual quantity at dinner" (p. 49), or by drinking a tumbler of gin and water as an economical agent "at the mid-day meal" (p. 50). We discover, also, that whilst some of us weaker brethren "complicate our ordinances with a multiplicity of detail," these "broader views" of Young Medicine offer a happy refuge to their neophytes in the following very simple system of therapeutics. Alluding to the case of a young lady under his care, Mr. Skey observes:

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[*] Cambridge School of Art: Mr. Ruskin's Inaugural Address, delivered at Cambridge, Oct. 29th, 1858.

[†] Lectures on the Principles and Practice of Physic, vol. i. pp. 400, 401. Third edition.

> "While the weather was yet hot I ordered her to take a cold shower bath every day. I gave her steel in the form of Schwalbach water, twice a-day, to the amount of two-thirds of a quart bottle. Twice a-day two drachms of compound tincture of bark. Thrice in the day

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* Watson, op. cit.

† On the Prevalent Treatment of Disease. Two Lectures delivered in the Theatre of the Royal College of Surgeons, in July, 1853, by Frederick C. Skey, F.R.S., Professor of Anatomy and Surgery to the College, pp. 68. London, 1853.

‡ Op. cit., p. 40.

> a full dessert-spoonful of cod liver-oil. Every morning a liniment of cantharides and turpentine to the upper part of the chest, alternately in front and on the back. She occupied a room without a curtain, and often slept with the window slightly open. She ate meat three times in each day, and she drank three full glasses of sherry during the remaining meals. . . . . . I ordered her to be encased in flannel down to the wrists and ankles as the weather became colder; and on the accession of the slightest sensation of chill to take hot wine or brandy and water." (p. 65.)

And this is the simple (?) system of therapeutics we are offered, by which

> "That *odium chirurgicum,* or rather *odium medicum,* the lancet and the scarificator, with all their concomitants of purgatives, laxatives, and diaphoretics, which tend to rob the body of its richest juices constituting the essence of its life itself, may be largely restricted in their operation." (!) (p. 67.)

The simplicity of the method! Alas for the force of habit and association. How simple to every one appears the system of truth on which his whole mental education has been constructed!

> "There is not a man on the face of this earth for whom a narrow education has marked out the whole cycle of his ideas, who does not think his system of truth the most divinely simple of all, and consequently look upon every other as encumbered with darkness, sinuosity, and confusion."[*]

Further, we cannot shut our eyes to the fact, that there is abundant evidence in the practice of some well-known physicians of the day, of their stretching the principles of the stimulant school of therapeutics beyond what must be considered their legitimate extent. We hear of acute pericarditis complicating rheumatism in its early stage, as a matter of course or of necessity being treated with ammonia, and a pint of brandy during the day; of typhoid fever from the onset, and under all circumstances, requiring a tablespoonful of brandy every hour; of its being suicide to take aught but gin and porter in pneumonia; and of "mulled port" being brought to bear upon the paroxysms of acute gout! But surely we shall not be thought desirous of reverting to the olden times, when bleeding and purgation were considered proper *omnibus semper et ubique*, if we insist upon the truth, as applied generally, of the following remarks of Dr. Wilks—though made by him only in reference to fever:

> "Patients die with stimulants, and without them, as well as recover on both plans. The fact is manifest, that these remedies can in some cases be dispensed with; and that in others, although freely given, do not save the patient's life. . . . . I believe that a large number of patients have been saved by their timely administration; . . . . but at the same time, I do strongly oppose the opinion that wine or brandy can be looked upon in the light of specific remedies, or as antidotes to the disease. . . . . Over and over again have I been surprised at the favourable termination of many bad cases. . . . . Where little or no stimulant was made use of, patients to whom I should have ordered a large quantity of wine, had I had the opportunity, nevertheless recovered on the very small pittance they were able themselves to procure."[*]

We would likewise offer for consideration to the extreme disciples of the "new method," who we fear must often run the dreadful chance of allowing the thread of life to be cut short in a state of drunkenness, the beautiful reply of a patient alluded to by Dr. Theophilus Thompson;[†] "a venerable lady, more than ninety years of age, whom I once attended, and who in answer to my arguments for the desirableness of endeavouring to maintain the circulation with brandy, made answer, 'Let me go home sober.'"

From the preceding observations it can readily be seen how it has happened that the prevalent doctrines connected with delirium tremens have continued to experience such great favour. No doubt some practitioners have rebelled against them, as reference to the works given at the head of this article will fully show; but though the names have neither been few nor insignificant which have been attached to teachings of a very opposite nature to those ordinarily received, we can scarcely regard these teachings as yet occupying any firmer position than as amongst the

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[*] Morell, J. D.

[†] Guy's Hospital Reports, p. 135. 1855.

[‡] Clinical Lectures on Pulmonary Consumption, p. 204. London, 1854.

> "Truths of science waiting to be caught,
> That float about the threshold of an age."

The first point in connexion with delirium tremens, and in reference to which a change of opinion is warranted by theory and necessitated by experience, is as to the *nature* of the disease. Without denying that the nervous system in the intemperate man is suffering irritation and exhaustion, and that his *delirium cum tremore* may be associated with one or other, or both, of these states, as its immediate antecedents, yet we maintain that we gain no knowledge thus of the mode in which these states are brought about; and we do not include those generalizations on their causation, which the progress of modern science permits us to advance in rendering an account of the *poisonous* action of alcohol. So far as we are aware, Levèille was the first to make a right step in this way.[*] He maintained that the disease consists in exaltation of the vital powers of the brain, excited by alcoholic molecules absorbed from the surface of the stomach and bowels, and carried into the current of the circulation. This notion of brain-substance being directly and toxically acted upon by alcoholic particles was slighted in our own country. It was called by Dr. Carter[†] "a somewhat whimsical theory, ... to overthrow which would perhaps cost little labour;" and Dr. Blake objected to it because he "never saw an individual attacked by delirium tremens so long as he continued to indulge in his accustomed alcoholic potations," and which supervened only when (from some cause or other) the latter were suddenly desisted from. But Tiedemann Gmelin (1820) and Magendie (1823) had already detected alcohol, by its odour, in the blood; and Dr. Cooke had affirmed, upon the authority of Sir A. Carlisle, that the fluid in the ventricles of the brain had been perceived in a particular instance to have the smell, the taste, and the inflammability of gin. The truth of these statements has been denied; but we shall see that later investigations readily vouch for their veracity. The opinion of Levèille, then, really involved nothing less than what we hold to be the truth—viz., that alcohol is absorbed directly into the circulation, and is capable of acting as a direct poison upon the nervous tissue, through which the infected blood circulates. That alcohol does become absorbed into the blood, like other poisons, we hold to have been proved by casual observation and by direct experiment. It has been found in the blood, in the urine, in the bile, in the fluid of the serous membranes, in the brain-matter, and in the liver. It is readily detectable by its odour in the breath; and, says Dr. Craigie,

> "The clothes and every part of the persons of these habitual but moderate drinkers, exhale a distinct alcoholic and saccharine odour, more or less strong, and which is only acquired by every part of the body being long thoroughly imbued with the spirituous aroma. . . . . Persons of this kind are easily distinguished by a good observer, especially if he be a water drinker—a habit which has a singular degree of power in preserving the senses, but especially that of smell, in a high degree of energy." (p. 71.)

The investigations of Dr. Percy, in particular, directly support the above statements;[‡] as also the belief of the great rapidity with which alcohol will pass into the current of the circulation. Strong alcohol was injected into the stomachs of dogs; the animals would sometimes fall insensible to the ground *immediately* upon the completion of the injection, their respiratory and cardiac movements ceasing within two minutes. On examination after death, the stomach was found nearly empty, whilst the blood was highly charged with alcohol. Dr. Cooke's statement has been confirmed by Dr. Ogston,[‖] who tells us, that in one case he found about four ounces of fluid in the ventricles of the brain, having all the physical qualities of alcohol; whilst Dr. Macpherson observes (p. 5), "alcohol is found in the brain, a fact I have witnessed when rum was very apparent in the lateral ventricles." We have ourselves been witness to the fact that a piece of cotton dipped into the fluid of the cerebral ventricles of a man who died in a state of deep intoxication, burnt for a moment with a lambent flame on a light being applied to it.

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[*] Mémoires de l'Académie Royale de Médecine, tome i. p. 181. Paris, 1828.

[†] Cyclopædia of Practical Medicine, vol. i. p. 518. London, 1833.

[‡] An Experimental Inquiry concerning the Presence of Alcohol in the Ventricles of the Brain, together with Experiments illustrative of the Physiological Action of Alcohol. By John Percy, M.D. London, 1839.

[‖] Edinburgh Medical and Surgical Journal, vol. xl.

Dr. Christison has questioned the correctness of Cooke's statement as to "inflammability," on the ground that gin of sufficient strength to take fire could not enter the bloodvessels without coagulating the blood; but Dr. Pereira found, by actual trial, "that a small quantity of undiluted commercial gin might be added to white of egg without causing either coagulation or the slightest opacity."* Alcohol being absorbed into the blood, a double series of malific results ensue. Upon the one hand, a train of chemico-physiologic consequences, highly detrimental to the general nutritive process, are induced, and which finally culminate in the production of the "drunkard's dyscrasy." On the other hand, a specific toxic action is exerted upon the brain—i.e., the cerebral substance is *poisonously* affected, and which probably constitutes one of the most necessary antecedents in the causation of delirium tremens. The late elaborate investigations of Mitscherlich, Bouchardat, Klencke, Sandras, Böcker, Scharlan, Vierordt, Duchek, Schulz, and Moleschott, must themselves be consulted if a detailed knowledge of the chemico-physiologic results of the absorption of alcohol into the blood be desired. We are forced to compress the deductions attained into as few words as possible, and as follows.

Alcohol being received into the stomach, is directly absorbed by the vessels of that viscus into the blood, without undergoing any change or decomposition. Part of it is eliminated as alcohol by the lungs, by the liver, and by the kidneys. Another portion is decomposed, its hydrogen entering voraciously, as it were, into combination with oxygen to form water, which with acetic acid, having been produced, is followed by the formation of carbonic acid and water. The result of this perversion of the oxygen is, that not only is the absolute quantity, but the relative proportion, of carbonic acid exhaled at the lungs diminished, and less urea excreted by the kidneys than is proper, but the blood remains charged with unmetamorphosed and unburnt-up matter, or containing thirty per cent. more of carbon beyond an amount which is healthy. The quantity of fatty matter thus made to accumulate in the blood is sometimes so great as, according to credible writers, to give this fluid an oily or milky appearance; and Dr. Huss states that the oily and fatty particles may be seen by the naked eye, both in the arterial and venous system. Dr. Macpherson alludes to the case of a sergeant in Fort William, at Calcutta, who died after a recent debauch, and in whom a peculiar oily state of the circulating fluid was observed by Dr. Adams. Lecanu is said to have found as much as eleven per cent. of fat in the blood. The amount of material which should be naturally metamorphosed and removed from the blood, but which is not so in the intemperate man, is still further added to by the primary excitant action of the alcohol increasing the frequency of respiration, the movements of the heart, and most of the functions of the body, though it may be only for a limited time. Thus more "wear and tear" ensues, more effete matter awaits change and removal, and yet, though more oxygen goes into the system, less carbonic acid comes out of it, the voracious alcohol seizing hold of all the former it can grasp. The result is, that a peculiar dyscrasia is produced, having close relations in its overt forms to the whole tribe of "fatty degenerations," and which is often attended by "tabes." So long as the alcohol remains in the blood as alcohol, a certain amount of toxic or poisonous effect is produced upon the nervous system through which the blood circulates. In ordinary cases a temporary excited state is all that is observable; on an excess, "intoxication" follows; and in the habitual dram-drinker, in whom a constant supply of the alcoholic fluid is kept up, so that the blood always contains a more or less quantity, the nervine toxic symptoms of chronic alcoholism show themselves, with the superaddition of episodes or acute paroxysms—i.e., outbreaks of delirium tremens, &c. It is in this latter respect that the peculiar toxic effects of alcohol upon the nervous system are so commonly and prominently seen. Dr. Percy suggested (op. cit.) that some particular affinity exists between the brain-substance and "spirit," more especially as after analysing a much larger quantity of blood than could possibly exist in the cranium, he could generally obtain much more alcohol from the brain than from this quantity of blood. That such an elective affinity by brain-matter for alcohol in the blood truly exists, was afterwards maintained by Dr. Carpenter, in his well-known treatise 'On the Use and Abuse of Alcoholic Liquors in Health and Disease.'

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* The Elements of Materia Medica and Therapeutics. By Jonathan Pereira. Fourth Edition, vol. ii. part 2, p. 448.

> "The alcohol being thus specially drawn out of the circulating current by the nervous matter, and incorporated with its substance in such a manner as even to change (when in sufficient amount) its physical as well as its chemical properties. It is important, also, to observe that this affinity is obviously such as will occasion the continual presence of alcohol in the blood, even in very minute proportion, to modify the nutrition of the nervous substance more than that of any other tissue, for the alcohol will seek out (as it were) the nervous matter, and will fasten itself upon it; just as we see that other poisons, whose results become more obvious to our senses (although the poisons themselves may exist in such minute amount as not to be detectible by the most refined analysis), will localize themselves in particular organs, or even in particular spots of the same organ." (Op. cit., p. 20.)

Such an affinity between alcohol and nervous matter being admitted, it is, however, doubtful whether always "the selective power of alcohol appears to lead it in the first instance to attack the cerebrum," as Dr. Carpenter supposes. It is certain that—

> "In some persons the influence on the motor system is much more manifest than on the sensorial, for . . . . while the individual sits still he but slightly betrays his devotion to the glass, and it is only when he attempts locomotion that it is discovered he is too drunk to walk."

According to Dr. Wilson,* there is one class of cases in which there are physiological grounds for believing that it is the cerebellum which is chiefly influenced, whilst in another class it is evident that the hemispheric ganglia are primarily affected. It has likewise been maintained, that "there are various reasons for supposing that alcohol acts immediately and directly upon the sympathetic or organic system, even when our perceptions do not distinguish any cerebral or sensorial change."† In the opinion of one well qualified to speak, viz., Dr. Huss of Stockholm, much yet remains to be cleared up, "whether it really is the alcohol itself in the blood, or the changes in its component parts," which are thus toxic to the nervous matter. Dr. Morehead thinks it may be "alcohol accumulated slowly in the blood, incorporated, if we may so speak, with the nervous matter of the brain;" whilst Dr. Todd suggests‡ that the "poisonous material is a compound, partly of alcohol itself, partly of some material derived from a depraved, destructive, secondary assimilation of the brain itself." Upon the question of specific poisoning, Dr. Peddie's remarks are worthy of quotation—

> ". . . . If there is one disease more than another arising from habitual excessive alcoholic drinks, in which a *peculiar toxicological effect* is manifested, it is delirium tremens; . . . . it is a form of alcoholic poisoning, or an alcoholism. . . . . In every instance of delirium tremens the stimulus or alcoholic principle, a powerful narcotico-acrid agent, in whatever way atomically combined or chemically changed after its introduction into the system, acts slowly on the nervous pulp through the medium of the circulation, poisons its substance, and sets up at last what may be termed an alcoholic erythism, or if I may be allowed the expression, an alcoholism; . . . . like plumbism, mercurialism, ergotism, or narcotism, alcoholism is manifestly specific in its nature. Lead, mercury, and other agents may affect individuals in different degrees; . . . the effect is brought about after the manner of a cumulative poison, the action of which is on the nervous centres." (pp. 9, 10, 12, 35.)

The general non-recognition of the specific toxic relations of delirium tremens, and its being looked upon as a form of simple nervous irritation and exhaustion, have led to the opinion that a disease exactly identical with this affection of the drunkard is occasionally to be met with, occurring after the abuse of opium, tobacco, from mental worry and excitement, and even in the course of acute rheumatism, erysipelas, &c. In the words of Mr. Philips,* "similar symptoms to those of drunkards' delirium may be observed in persons whose habits have been strictly temperate. . . . . An exactly similar train of symptoms may be observed in persons who have not indulged in the use of intoxicating liquors at all." Dr. Carter states,† that he has known delirium tremens to have been produced by the long-continued and free use of opium, and that "this is sufficient to overthrow the theory of M. Levèille, which would make the absorption of alcoholic molecules the exciting cause of the disease." Dr. Copland‡ asserts that it may "be occasioned by the drugged beverages prepared in Eastern countries, particularly in the East Indies, when too freely indulged in, and by the excessive use of opium;" whilst in the 'Medical Times and Gazette,' of August, 1853, will be found a case recorded as "Delirium Tremens produced by Abstinence from Tobacco" after addiction to its use. This example we shall present to our readers, along with Dr. Peddie's sharp and correct *critique* of the wonderful instance. The italics are introduced to draw attention to some points of importance bearing reference to what we have already said and shall hereafter say. The case is related as follows—

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* The Pathology of Drunkenness: a View of the Operation of Ardent Spirits in the Production of Disease. 1855.
† See vol. vii. of this Review.
‡ Lumleian Lectures for 1850. Reported in the Medical Times, May, 1850.

> "Delirium tremens, and its twin sister, traumatic delirium, are now so well understood to be dependent on *asthenic irritability of the nervous system*, that but one opinion prevails as to the principles which should regulate their treatment. *Sudden disuse of accustomed stimulants* is always to be deprecated in the event of a patient of known intemperate habits coming under surgical treatment, especially on account of an accident; care should always be taken that he is not deprived of his wonted allowance of alcohol. There is, however, another very potent drug in but too common use among the lower orders, the probable effects of suddenly relinquishing which have, we suspect, been too little considered, and respecting which the notes of a case lately under the care of Mr. Curling appear to afford a valuable hint to the practical surgeon. A withered old woman, a *gin drinker*, and a habitual smoker, was admitted on account of a *severe burn*. *Stimulants were from the first freely allowed her, and opiates administered, but in spite of them she continued extremely restless, wandering at times, and quite unable to sleep.* Her manner and aspect, indeed, much resembled those of *delirium tremens*. At this juncture, *several days after admission*, Mr. Curling ordered that she should be permitted to smoke. The salutary influence of the permission was at once apparent, the woman became quiet and tranquil, and on the next night slept fairly. All tendency to delirium disappeared, and she afterwards progressed steadily to recovery." (Op. cit.)

Now, retorts Dr. Peddie,

> "This was nothing but a mild case of delirium tremens from habitual gin-drinking, precipitated by the severe burn, and aided by the stimulants and opiates so freely given, and in consequence of these combined circumstances—not in spite of them—the restlessness continued. The absence of tobacco had nothing to do with this state of matters, but the disease originated from its ordinary cause, and was running its ordinary course of a few days. Convalescence was in all probability begun when the tobacco was allowed; but if not, no doubt its effect would be good, for it would act, not as a stimulant or as a narcotic, but as a sedative, soothing and depressing the cerebral excitement, and sleep would follow as a natural consequence." (p. 25.)

According to Mr. Solly,§ Mr. Whitfield (the resident medical officer of St. Thomas's) "has seen three cases of delirium tremens induced by tobacco smoke alone. In none of these cases had the patients indulged in drinking intoxicating liquors, so that there was no doubt of the single cause of the disease."

It has even been said that the poisons of lead, of malaria, long fasting, and exposure to cold and wet‖, will produce an identical malady; and a writer of the day treats us with the following *lucus a non lucendo*—

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* Ranking's Abstract, &c., vol. v. p. 18.

† Cyclo. Pract. Med., vol. i. p. 514.

‡ Dictionary of Pract. Med., vol. vi. p. 498.

§ Lancet, May 7, 1859.

‖ Lancet, Feb. 14, 1857.

"The most common cause of delirium tremens is, as has been stated, intemperance; .... intemperance in the use of alcoholic drinks is not, however, the alone cause of delirium tremens; indeed it may be questioned whether it is ever in the strictest sense of the word a cause at all."*

That in many cases like the above a *delirium cum tremore* has occurred, of course we do not deny, but that such delirium is identical with that of alcoholism, we beg to disbelieve; we will hear, however, Dr. Peddie—

"As regards the other causes, independently of alcoholic liquors, said to produce delirium tremens, the kind of delirium differs in each case or partakes more of the characters of insanity; and there is also a corresponding diversity in the nature of the wakefulness, the muscular tremors, and other symptoms, all of which circumstances, if space permitted, could be explained on very different grounds. . . . . I can suppose the continued use of inordinate quantities of laudanum might occasion delirium tremens, as has been reported, from the amount of alcohol necessarily consumed, which of itself would be sufficient to occasion it. . . . . In regard to any influence which the disuse of tobacco may have in occasioning this malady, I would say that it is quite out of the question, and that any attack occurring in the case of the recent smoker, must have been owing to the conjoined habit of drinking." (pp. 24, 25.)

Dr. Macpherson's commentary upon the question is also worthy of notice—

"Authors talk of delirium tremens coming on from moral causes, from excessive or restrained secretions, from abuse of laudanum and of tobacco; but of these causes producing delirium tremens I have no knowledge, although excess in the use of tobacco among Europeans in India is common, and abuse of opium by natives still more so. . . . . The disease most resembling delirium tremens is the delirium *é quinâ*, or the *ivresse quinique* of the French; so close do their symptoms run that there was a difference of opinion among the medical officers of the General Hospital, whether certain cases were delirium *é quinâ* or *à potu*, but in one fatal case the deafness and amaurosis were distinctly pathognomic of the former." (p. 6.)

The similarity of many of the phenomena of poisoning by *datura* with those characteristic of delirium tremens, has arrested the attention of Dr. Morehead. But judging from the cases received from time to time into the Jamsetjee Jeejeebhoy Hospital, there exist these differences—viz., the delirium is more muttering, not so busy as that of delirium tremens, and if the same class of deranged nervous actions which characterize the third stage of the latter are attained from *datura*, they are very generally recovered from, not by a return from coma to a state of health; but the coma ceases, and then succeed the delirium and the other phenomena which attend those slighter cases which have never passed into coma.

Dr. Ware early alluded (op. cit.) to certain forms of delirium occurring in the course of some acute and chronic diseases—

"Affecting at once the mind and the body, and approaching very nearly in their aspect at particular times to delirium tremens; still they are to be distinguished by the want of regularity in the whole course, by their not constituting a proper paroxysm, and by their having no definite termination in sleep."

But perhaps the more important point here is, as to the exact nature of the vigilant *delirium cum tremore*, so frequently occurring after injuries, and sometimes after operations. It is from such examples met with in the surgical wards of an hospital that the student receives his impressions of the nature and treatment of the delirium of alcoholic toxæmia; applying all that he has there learnt to the pathology of the idiopathic forms of this affection which he afterwards meets with. Between the years 1819 and 1827, Dupuytren, Albers, Witthausen, Barkhausen, and Coates described a species of delirium which they called "symptomatic," "nervous," and "traumatic," and mentioned it as occurring in about one twenty-sixth part of those who had met with fractures or other injuries, particularly of the lower extremities. During the session of 1833, Dupuytren again drew attention to this form of malady in his 'Leçons Orales,' and the clinical lecture illustrative of his views may be found translated in the 'Lancet' for March 15th, 1834. Of this "nervous delirium, which often complicates wounds and operations," the great French surgeon says, "that though its causes are obscure, its progress uncertain, and the symptoms through which it declares itself most alarming, it is still rarely fatal when properly and early treated." If the patient die, however, "on examining the body we do not find, either in the brain or spinal marrow, or even in any other organ, any material changes sufficient to explain the disorders which have taken place on the death of the patient." Now, in these cases, in modern times, most persons recognise only the following circumstances:—A hard drinker meets with a serious accident, he is forced to lie up, becomes deprived of his ordinary stimulants, and consequently (as it is believed) has an attack of delirium tremens. But there have not been wanting those who have taken a somewhat different view of the matter; they have looked upon the fracture or other injury as setting up fever, the delirium of which becomes tremulous from its being modified by the alcoholization of the system in which it occurs (Craigie.) To use Dr. Carter's words—

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* Barlow's Manual of Practical Medicine, pp. 540-41. London, 1856.

> "In such cases the system receives a sudden and violent shock. An individual, previously perhaps in the enjoyment of robust health, accustomed to strong exercise, engaged in a laborious occupation requiring much support, is at once confined to his bed; his habits are in a moment broken off, and no wonder that in a few days he falls into a state greatly resembling that of a patient in typhus—a state which absolutely requires the stimuli to which he used to resort previously to the reception of the injury."*

But in delirium tremens unconnected with other diseases or with injury, we recognise no such immediate and overwhelming shock, and it is in this *shock* that a most important factor of the sum is seen, not only as the immediate or exciting cause of the delirium, which we have not any reason for believing would have occurred without it, but as frequently necessitating a modification of treatment which may be but little suited to that form of the affection which is of the idiopathic kind.

> "The sudden shock to the system in the one case, and the altered balance of the circulation and disorder of nutrition in the other, bring, I conceive, the individual at once into the condition of susceptibility to this disease, which would not otherwise perhaps have been so early accomplished. . . . . It is in this irritable state of the habitual drunkard's constitution, although he may not be on the verge of delirium tremens, that alcohol, by its presence in the blood—in whatever way combined—and by its interference with the nutrition of the brain and nervous system, will superinduce on the receipt of an injury—say a gun-shot wound, a severe burn, or a fracture—a febrile attack attended by delirium presenting somewhat of the appearance of that disease, but which in reality has more of a typhoid character."†

We do not agree either in the opinion that this form of delirium is nothing but that of simple alcoholism, nor in that which does not see anything at all *specific* in it. We regard it as delirium tremens hastened or brought on before its time by *shock* in known drinkers, its course, progress, and treatment being modified by the perturbation the system suffers from under the shock in question. Had not the patient met with the injury, he would not have manifested the paroxysm until more fully alcoholized, when the physician instead of the surgeon would have had him under care. But "shock" happens, the crisis is hastened, and the surgeon gets two diseases to deal with instead of one. Next in importance to the recognition of delirium tremens as a form of specific toxæmia from alcoholism—unproducible by aught else—is the knowledge of the "natural history" and *course* of the disease. The particular opinions arrived at in reference to the latter, influence in a great degree the nature of therapeutic interference. If, to use the words of Dr. Laycock, "the delirium belongs to the class of self-limiting disorders;" or, as says Dr. Bennett, "in the vast majority of cases the poison becomes eliminated from the system in a certain time," much of the argument in favour of the supposed value of a popular mode of treatment is rendered uselessly vague. Though it had been stated by Dr. Blake that the "mental irritation" requires a given time to subside, Dr. Ware first dogmatically taught that delirium tremens is a paroxysm of poisoning by alcohol, *which in the majority of cases lasts only a given time, and terminates favourably in a critical sleep.* He says:

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* Cyclopædia of Practical Medicine, vol. i. p. 518.

† Peddie, op. cit., p. 18.

> "The natural tendency of the paroxysm is to terminate in a spontaneous and salutary sleep at the end of a certain period—viz., sixty to seventy-two hours; and even in the reports of cases which have been submitted to the public as evidences of the efficacy of various modes of practice, sleep has not actually taken place sooner than it would have done in the natural course of the disease. . . . . . The termination of a paroxysm of delirium tremens is always, as has been already mentioned, in *profound* sleep. . . . . . Sleep, however, is not always to be regarded as indicating the speedy termination of the paroxysm, since it is not uncommon for patients to sleep a little—from a few minutes to an hour, for instance—on each day of the delirium." (pp. 33–45.)

Drs. Peddie, Guldberg, Huss, and Morehead, teach an analogous doctrine, the former remarking that the paroxysm occurs

> "With remarkable uniformity, independently of age and constitution; usually runs its course, if uncomplicated and properly treated, on the second or third day, though sometimes earlier, and it seldom extends beyond the fifth day. It then terminates in a profound natural sleep, which may continue for many hours, and from which, if it even lasts for six hours, the patient awakes quite coherent, although languid and weak, but from which state, considering the severity of the symptoms, he is restored with singular rapidity to physical strength and mental soundness." (p. 6.)

Dr. Morehead writes:

> "It is this feature—I mean the circumstance of the second stage running a second course—which it seems to me has not received its full consideration in relation to treatment. For, if acknowledged, it may be safely affirmed that the indication of cure is not by full doses of narcotics to force a state of sleep, but to conduct the patient through the period of delirium, &c." (Vol. ii., p. 587.)

We entirely agree in this doctrine, that the paroxysm will, in uncomplicated cases, work itself off in a definite time, and that profound sleep occurs as the natural, the favourable crisis, or rather as the termination of the disorder. Sleep occurs as the *result* of the paroxysm having run its course, and of the nervous system having lapsed into an improved condition, and must not be regarded as the *cause* of these favourable conditions. We shall revert to this point when bringing to bear the law of the "course of the disease" upon the consideration of treatment, now simply observing that the writers we have quoted and referred to have arrived at their conclusions from the observation of cases where care had been taken that no plan should be adopted that might vitiate the result.

Of all the errors in popular acceptation connected with the malady we are engaged with, none is greater than that which affirms the exciting cause of the paroxysm to be a sudden stopping or withdrawal of the accustomed quantity of stimulants. Such is taught, however, there can be no doubt, by some high authorities of the day, both orally and in their systematic writings, and the tenet has become "an institution" to most house-surgeons and hospital sisters. Even in private practice the interrogative reply will be made to some heterodox practitioner like ourselves: "Is there no danger in stopping the stimulants all at once?" Yet we hesitate not to say that there is but little truth in the prevailing creed. Dr. Ware long since opposed it when he maintained that in a large proportion of cases withdrawal of the accustomed stimulants had nothing to do with the accessions of the paroxysm, and that "the disease occurs also in individuals whose habit of drinking has never been suspended at all, but has been continued up to the very commencement of the delirium." (p. 7.) Dr. Craigie, without positively denying that the disease may come on from the cause stated, goes on to observe:

> "I can only say that I never witnessed an instance of this mode of development, and after perusing all the published cases extant, I cannot perceive that any of them, excepting the one recorded by Dr. Armstrong in the ninth volume of the 'Edinburgh Medical and Surgical Journal' (p. 146), afford satisfactory evidence that the disease is induced in consequence of the sudden abstraction of the use of spirituous liquors, and even that case, I think, may be explained without having recourse to the supposition now mentioned. I have, on the contrary, never observed that the sudden and complete abstraction of these liquors aggravated the symptoms of the disease. I find, further, that neither Berndt, Tœpken, Hufeland, Andreæ, Goeden, Sieburgundi, nor many other foreign physicians by whom the disease has been observed, admit that it is produced in this manner, and in all the cases recorded by them the symptoms were developed after a continuance, more or less protracted, of stimulation by spirituous liquors." (p. 57.)

In the twenty-two cases of delirium tremens admitted under Dr. Laycock into the Infirmary last summer, the patients were all alcoholized when admitted, with only one exception, and, says Dr. Laycock: "I cannot recollect, in fact, any case in which I could attribute the delirium to a withdrawal of the liquor." (p. 300.) Drs. Macpherson and Peddie are on the same side; according to the former, "in most of the cases in the General Hospital the patients had been drinking up to the time of their admission" (p. 5); whilst the evidence produced by the latter is highly important, and in itself quite conclusive to our minds. In order to obtain trustworthy data to decide upon the effect of the sudden withdrawal of all stimulants from civil and criminal prisoners known or presumed to be of intemperate habits, and the immediate substitution of prison fare, Dr. Peddie threw himself upon the experience of Drs. Simson, Gibson, and Scott, and of Mr. Page, the medical officers of the large prison establishments of Edinburgh, Glasgow, Carlisle, and Dumfries.

> "As regards the prison of Carlisle, it appears that although the annual number of commitments during the last fifteen years has been about six hundred, and that although three-fourths of these are considered to have been in some way or another the consequences of drunkenness, Mr. Page states emphatically he has never yet seen any ill result from the sudden abstraction of stimulants from habitual drunkards who had been drinking to excess up to the time of being placed on prison fare. Mr. Page had also, during nine years' experience in connexion with the Carlisle County Pauper Lunatic Asylum, observed the same impunity with which all stimulants could be at once withdrawn.
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> "Of the gaol of Dumfries, it is stated by Dr. Scott that during the last fifteen years the number of civil and criminal prisoners has amounted to 5539; that of this number he supposes about two-thirds were committed for crimes resulting from intemperate habits; that he believes a very large number to have been habitual drunkards, and that although all these of course were deprived of their usual libations, and at once put on prison allowance, only five cases of delirium tremens are found on the register of disease, and that all of these patients but one were admitted to the prison with the disease on them. . . . . Dr. Scott also notes, as an important fact, that during the time the railways were being constructed in the county of Durham, a very large number of 'navvies' were committed to prison who had led a very dissipated life for many months, and although deprived of liquor from the moment of apprehension, not a single case of delirium tremens occurred. Then, as regards the prison of Glasgow, in which the annual commitments amount to upwards of 4000, the experience of the year 1850 is adduced by Dr. Gibson as affording an approximation to the facts wished to be elicited. A calculation made in the year showed that while 4122 were imprisoned, the number of assaults, with few exceptions, committed under the influence of liquor, and the 'drunk and disorderly,' amounted to 1519, and of this number only three cases of delirium tremens occurred—a very small proportion indeed, especially when it is considered that the debtors, who are almost all habitual drunkards, and drinking up to the moment of incarceration, are not included in this list; many hundreds more, therefore, may be considered to have belonged to the drunken population of the gaol. The average of the last ten years, however, is greater (5·7), there having been fifty-seven cases altogether during that period; but after all, this is a very small proportion to the number of dissipated and drunken characters gathered together there, and at once broken off from intemperate habits. . . . . The evidence communicated by Dr. Simson, the medical officer to the Prison Board of this city (Edinburgh), is sufficiently satisfactory, for while the number of civil and criminal prisoners committed during the last year was 5864, only four cases of delirium tremens occurred within the last eighteen months. The average number of cases during former years Dr. Scott states as from

two to three per annum. Dr. Scott considers that at least one-half of the whole prisoners may be assumed as dissipated characters, and that at the very lowest computation 500 must have been regular systematic drunkards, from whom all drink was suddenly abstracted; and he goes on to state as his decided opinion, that the sudden taking away of spirits, &c., does not produce delirium tremens. In *every* case the prisoner had symptoms of the disease on him when admitted—that is, they were all restless, irritable, &c., and I have no doubt but that in many instances the crimes committed were the effects of this disease." (p. 20.)

Here, then, as Dr. Peddie observes, it has been shown that hundreds of individuals among the public at large, and of the criminals committed to our gaols, leave off or are suddenly deprived of the stimulants to which they had been previously addicted without being affected by the delirium or by anything approaching to it! A debtor incarcerated in the Dumfries gaol had twice had delirium tremens, and was in the practice of taking, when admitted, on an average, one bottle of spirits and upwards of three ounces of the tincture of opium daily. Every drop was withdrawn without any bad symptom following. We have ourselves suddenly and purposely debarred an *alcoholatre* of a bottle of brandy, of an uncertain quantity of gin in his coffee, and of several glasses of wine, which he had been accustomed to take daily, for such time until it became so expensive a practice that his wife was obliged to substitute "British brandy" in lieu of the foreign. No delirium followed, though we were consulted for symptoms of chronic alcoholism, such as have been described by the Stockholm physician, Dr. Huss. Thrice last year we attended the proprietor of an hotel who five years before had an attack of drunkard's delirium. On the first two occasions that we saw him, he was suffering from debauches, and fears were entertained that other attacks would ensue. On each occasion he was rigorously debarred from all excitants, and nothing beyond a transient sleeplessness, a little wandering at night, and a very slight tremulousness, was superadded to the general *malaise*. On the third occasion the debauch had continued longer, and we saw him led drunk from his half-finished brandy and water to his bed. The sleeplessness continued for two nights, considerable tremor existed for one day, as also startings, as if from an electric shock, when he was suddenly addressed or his door opened, together with marked delirium at night; yet no true paroxysm was completed, though he was debarred as before from alcoholic drinks. In each of these instances, the impression made upon our mind was that the supervention of a fully-developed paroxysm was *prevented* by arresting the process of alcoholization to which the system was being subjected. But the question may fairly be asked of us, How it is that so great an error as we are combating has come to be so generally adopted? In the first place, we must reply, that on a narrow scrutiny of the statements of many of the supporters of the popular doctrine, it will be found that even they are forced to admit that *some* instances of delirium tremens do occur without any previous privation of the usual stimuli—e.g., "Sometimes," says Dr. Watson, "however, it comes on in men who are perpetually fuddled, even although they have not interdicted their usual allowance in drink."*

"In publicans," writes Dr. Carter, "who live in an atmosphere charged with alcohol, who, though seldom absolutely intoxicated, are always more or less under the influence of intoxicating liquors, who are sipping from morning to night, instances of well-marked delirium tremens occurring in such persons have fallen under our observation."†

In the second place, the *delirium cum tremore* of the surgeons' wards has mostly afforded the data to argue upon, it being quite overlooked, too, that such patients often meet with their injuries only from being actually intoxicated at the time of their reception, or that they are afterwards under the influence of *shock*, as well as that the stimulants have been withdrawn. Dr. Peddie observes:

"To me it is apparent that habitual excess in the use of stimulants is alike the exciting and the predisposing cause of delirium tremens, and that if a suspension or diminution of habitual supplies be at any time attended by symptoms of the disease, these are not to be regarded as resulting from change in the quantity consumed, but as occurring in spite of such change, &c. . . . . From overlooking these circumstances, I believe all the statements in regard to the supposed effects of diminution, suspension, or abstraction of an accustomed stimulus have originated. The error is a popular one, and has arisen from imperfect inquiry into the history of individual cases and incorrect observation regarding the circumstances connected with the supposed reduction or abstraction. When called to see a case of delirium tremens, on inquiry as to the habits of the patient, we are frequently informed by his friends that for a long time large quantities of spirits, or wine, or malt, or of all these, and perhaps in addition morphia or opium, had been systematically consumed, but that for some time (a few weeks, perhaps) much less had been taken, and within the last few days little or none; and then the inference is drawn for us that the unfortunate patient has actually brought on the attack by meritorious efforts to free himself of a habit of which he had begun to be ashamed. Now all this is very plausible, but not in accordance with the strict facts of the case, as the individual himself, if put on his word of honour, will probably confess. The statement ought to be that he was formerly in the habit of consuming large quantities of his favourite stimulant, until he found that a much less dose began to affect the system, that then he reduced the amount still further, but experienced an equal if not greater constitutional effect therefrom, and thus, from day to day, reduction was forced on him by his own sensations of gastric irritation, nervous excitement, and muscular debility, these feelings having been, in fact, neither more nor less than the premonitory symptoms of the attack of delirium tremens, and just what might have been expected if, as I have ventured to assert, the alcoholic principle is to be viewed as a cumulative poison." (p. 15.)

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* Lectures, &c., vol. i.

† Cyclopædia of Practical Medicine, vol. i. p. 518.

Further, on the other side of the question, as Dr. Laycock pointedly remarks, the evidence is abundant: multitudes of drunkards cease drinking without suffering from the disease or from any approach to it.

If it be true that delirium tremens is an acute paroxysm of poisoning by a specific agent which has been excessively and continuously used, and which, upon the principle of accumulation (as is the case with mercury, lead, iodine, strychnia, &c.), gives rise somewhat suddenly to such paroxysm, and if also in by far the majority of cases the outbreak is the direct result of a debauch rather than of a refrainment from the toxic agent, theory would strictly warrant us in concluding that the continued administration of the specific poison would be contrary to the method of treatment which should be adopted upon the supervention of the attack. But since few comparatively have looked at the matter in these lights, as most are ignorant of the natural course of the malady, since the therapeia as well as the pathology of the latter have been based mainly upon cases complicated with *shock*, and since the views prevailing have dovetailed in so admirably with the modern "brandy-and-water" dogmas, it can scarcely be wondered at, perhaps, that the popular error should continue to be adhered to. An error which regards alcoholism seen under the aspect of delirium tremens, as only to be successfully treated according to the old method in hydrophobia—viz., the giving the patient "a hair of the dog that bit." But experience as well as theory supports that view of the question which we are arguing for, and indeed has long done so, and we believe that if the modern school of exaggerated stimulation had not come to the rescue, it would have been agreed upon by all observant practitioners that the administration of alcoholic excitants as part of the treatment was not good practice, whatever views might continue to be held regarding the pathology of the disorder. Nearly thirty years back, the extensive experience of Dr. Wright at the Baltimore Almshouse Institution, led him to declare* that "On the whole, I am sincerely convinced that the administration of spirituous drink is not generally necessary, nor beneficial in tendency, either as a preventive or a remedial means." Dr. Ware denies† the propriety of giving spirituous liquors, and Dr. Carter observes,‡ "The accustomed stimuli should be withdrawn without hesitation, neither wine, nor spirits, nor strong malt liquor should be allowed at all." Klapp, Coates, Cross, and Baron, well-known American writers upon this disorder, are not less decided than are Wright, Ware, and Carter, and in Dr. Craigie's essay will be found a strong protest against being led astray by "the error and fallaciousness of this mode of treatment."

"To form a just idea of the propriety of treating patients in this manner, it would constitute an important subject of philosophical inquiry to ascertain what are the principles of pathology—nay, what are the grounds of common sense upon which patients labouring under methystic brain fever are made to swallow half a bottle of spirits in twenty-four hours, sometimes with a quantity of wine and ale at the same time, while they are firmly bound down in a strait-waistcoat, and the feet secured by the ankles to the bed. What member of the human race, it may be asked, if treated in that manner, would not be driven mad, or worked into a state of furiosity in which violent muscular action would be succeeded by the exhaustion of death?"*

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* The American Journal of Medical Sciences, vol. vi. p. 32. Philadelphia, 1830.

† Op. cit., p. 59.

‡ Cyclopædia of Practical Medicine, vol. i. p. 514.

Dr. Cahill, although believing that in the case of *habitual drunkards* it is necessary to give them their accustomed stimulus to prevent sinking, nevertheless makes the admission—viz., that "it should be remembered that the *perfect* recovery of the patient and his exemption from *sequelæ* are in an inverse ratio to the quantity of stimulants administered." (p. 405.)

But it is here that we are so much indebted to the inquiries of Drs. Peddie and Laycock. The former brings the experience of twenty years to bear upon the question, and his conclusions are unhesitatingly condemnatory of the continued exhibition of alcoholic stimulants. He appends the history of nine plain, straightforward examples which were treated by antimony, and not by "spirits," and in which the paroxysms ran their usual and favourable course. He continues:

"I do not say that I would never give a stimulant in delirium tremens. . . . Then, again, I would not hesitate to give an allowance of his usual stimulant to a habitual drunkard, when affected with a wound or ulcer, so as to obtain a healthy action therein, or to administer stimuli, &c., &c. . . . But it is quite another thing to prescribe alcohol when the individual is already manifestly in a state of alcoholic poisoning." (p. 28.)

If the patient be alcoholized at the very moment when he is suffering from delirium tremens, it seems hardly rational, as aptly observed by Dr. Laycock, to propose that more alcohol should be given to him "every two or three hours." Dr. Laycock treated 28 cases (15 are detailed) during last summer, without the use of "spirit." All so treated recovered rapidly. The following history is painfully illustrative of some of the points we are discussing:

"Some time ago, on a Sunday morning, I was requested by his teacher to visit N——, a student for one of the professions at a public-house in the Grassmarket, who had been on the 'spree' for several weeks. I found him in a pitiable state of helpless apprehensiveness and imbecility, yet incessantly praying for whisky. His countenance was haggard, pulse very weak and quick; he was hardly able to stand, and was full of indefinable fears. His tongue covered with a thick creamy fur, epigastrium and hypochondrium very tender on pressure, skin moist and clammy, countenance and eye icteric; he had taken little or no food for several days, and had been wandering into brothels and dram-shops, not able to guide himself. I subjoin his hotel bill.

"Nov. 10th.—Breakfast, 1*s.* 9*d.*; whisky, 5*s.*; fire, 1*s.*; eating, 1*s.* 6*d.*; ale, 5*d.*; wine, 5*s.* 6*d.*; soda water, 2*s.* 1*d.*; coach-fare, 2*s.* 6*d.*; medicine, 3*s.*

"Nov. 11th.—Breakfast, 1*s.* 9*d.*; soup, 2*s.*; medicine, 4*s.* 6*d.*; whisky, 3*s.*; soda, 3*s.*; wine, 7*s.* 6*d.*

"Nov. 12th.—Breakfast, 1*s.* 9*d.*; brandy, 4*s.* 6*d.*; soda, 1*s.* 6*d.*; soup, 1*s.* 4*d.*; whisky, 2*s.* 4*d.*; toddy, 6*d.*; medicine, 5*s.* 6*d.*

"Nov. 14th.—Soup, 2*s.*; wine, 6*d.*, medicine, 4*s.*; whisky, 4*s.*; ale, 5*d.*; fire, 5*s.*

"Nov. 15th.—Bitters, 1*s.* 4*d.*; soup, 1*s.* 4*d.*; soda, 2*s.*

"Total,	Alcoholic drinks	£2	15	6
	Soda water, 11*s.* 6*d.*; medicine, 17*s.*	1	8	6
	Food	0	18	8
	Fire.	0	6	0

"It was with great difficulty he was got out of the public-house and taken to the Infirmary, to be treated in a side room. As his clamour for whisky was incessant, on the ground that he would die if he had it not, I concluded the morbid appetite was due to an intense epigastric neuralgia or feeling of sinking with gastritis, and ordered him morphia, nitrate of silver, and calomel immediately. He was relieved after a few doses, and next day abhorred the thought of whisky. In a few days he was removed to his lodgings, where the treatment was continued, and he was shortly convalescent. He had subsequently a relapse into drunken habits, and another attack of delirium tremens. . . . . The student is an illustration of the moral degradation of drunkards. Although a gentleman by birth, education, and manners, and not without funds, he paid no attention to the repeated hints given him that he was at least under a pecuniary obligation to the Royal Infirmary. One of his teachers looked after him in his delirious state, paid his cab-hire to the Infirmary, and visited him afterwards at his lodgings; in return for this kindness (if I am rightly informed) he accused his friend of robbing him." (p. 297.)

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* Craigie, p. 98.

"*L'opium* jouit d'une vogue presque incroyable dans le traitement du délirium tremens," says Calmeil;[*] and replies Copland,[†] "I consider opium as necessary to the cure of this disease, as bark and analogous medicines are to the cure of ague." *We*, however, along with Dr. Laycock, seriously ask, "*Is* opium necessary to the cure of delirium tremens, or is it a safe drug to administer freely in that disorder?" We reply, both theory and experience impose a negative answer to each query. By the former we are clearly told that it is an error to aim at the fulfilment of an indication sought to be obtained in no other form of poisoning—viz., the dissipation of the effects of a foreign agent before time has been allowed for the elimination of such agent from the frame. By the latter we are convinced that case upon case has been successfully treated without opium, and in those instances where it has been adopted, the supervention of congestion and coma has often appeared to have been favoured by it, or a condition of the patient induced which proved that "the effects of an excessive quantity of this drug very nearly resemble the phenomena of the last stage of the disease, particularly towards its fatal close." We admit, however, that no less an authority than Dr. Watson tells us that "opium must be given in full doses, and it must be fearlessly repeated if its desired effect do not soon follow;" and that Dr. Wood advises "two grains of opium, half a grain of sulphate of morphia, or an equivalent quantity of one of the liquid preparations of the drug, are to be given every two hours, and steadily persevered in until sleep takes place, or a decided narcotic impression is evinced." Dr. Dickson[‡] prescribes it "with unyielding perseverance," giving "a teaspoonful of laudanum every hour in ordinary cases, until sleep is induced," the dose having been repeated in one instance fourteen times. But what is this to Dr. Jackson, who prescribed "from ten to fifteen, or even twenty grains of solid opium every two hours!" And we were lately assured by a surgeon of one of our metropolitan hospitals, that indeed he knew of *no limit* to the administration of opium in delirium tremens until sleep be obtained! But not the less explicit are the opinions to the contrary of men of whose experience and judgment there can be no question. "So far from being beneficial," says Dr. Ware (p. 48), "there is ground to believe that the effect of opium given during the paroxysm is to increase the violence of the delirium, to produce a tendency to convulsions, to prevent the termination by a natural and salutary sleep, to throw the patient into a state of coma from which he does not awake." Calmeil, commenting[§] upon this opinion, writes:

> "We coincide entirely in the judgment of Dr. Ware. . . . . In 1819 it was remarked by Georget that the dipsomaniacs who were under the care of M. Esquirol at the Salpêtrière all recovered in three days' time, and without ever taking opium. The number of dipsomaniacs brought to Charenton within twelve years have recovered, but with few exceptions, without having taken any preparation of opium."

Dr. Atkinson agrees with Guldberg[‖] that opium "should be used with more caution than is wont to be the case;" while Dr. Macpherson has "long had a suspicion that we are apt to over-dose our patients with" this drug. To Dr. Morehead "it has seemed, that in cases treated with free opiates, there is a greater tendency to pass into the third stage, and . . . . a greater number thus treated terminate by convulsion and coma." Dr. Law is "persuaded" that many are the cases in which opium not only fails to be useful, but is productive of positive mischief; whilst Dr. Cahill concludes the history of his cases by pointing out how "it will be seen that opium is not beneficial in many cases, in others that it is positively injurious, and that in all a cure can be effected without its assistance." (p. 404.) Dr. Laycock, after quoting the statements of Drs. Watson and Wood, observes, "How contrary all this is to my experience in the Royal Infirmary, is obvious."

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[*] Dictionnaire de Médecine, tome x. p. 80 (Art., Delirium Tremens). Paris, 1835.
[†] A Dictionary of Practical Medicine, vol. i. p. 502 (Art., Delirium with Tremor). London, 1858.
[‡] Elements of Medicine, p. 655. Philadelphia, 1855.
[§] Op. cit., p. 40.
[‖] Commentatio de Delirio tremente.

> "Experience rather than theory is the safest guide, and that assures us it is never, under any circumstances, a safe proceeding to administer from five to ten or fifteen grains of opium in as many hours. Experience also shows that in delirium tremens, while many have recovered without opium, and some in spite of it, none can be said to have died for the want of it. And if we were to inquire theoretically in what class of cases opium should not, or need not, be given, we should find very few left in which it should." (p. 303.)

Dr. Peddie does

> "Not hesitate to say that it is a very doubtful remedy, even in the most promising cases of the disease, and a most dangerous one in others." (p. 28.)
>
> "If opium is to be used at all in delirium tremens, it must be given in a large dose (in from two to three or more grains, and repeated at intervals of a few hours), and it is thus generally given, the object being to overstep the stage of excitement, and force on the desired sleep..... Sleep is obtained, but it goes on deepening, and as it becomes more profound the pulse becomes smaller and less frequent, the surface of the body covered with a cold sweat, the face pale, the pupils contracted, the breathing slow and soft (although sometimes stertorous). An epileptic fit may now occur and terminate the scene, or the powers of life gradually become more and more depressed, and the victim perishes as if in a profound and gentle sleep. Now, this progress and catastrophe, although viewed as evidence of an unmanageable and malignant form of disease in a bad subject, is nothing more than the common course and result of injudicious treatment. Even Graves, who prescribed opium in delirium tremens in the manner I will afterwards notice, warns emphatically against its premature and incautious use. 'Opium,' he says, 'if given in the beginning, will increase the congestion and bring on subarachnoid effusion. I treated a case of delirium tremens in this way too boldly, and the man died with subarachnoid effusion.' I am convinced that it is in this way very many of the sudden deaths we hear of in delirium tremens occur; I saw it frequently in early practice, and have seen it occasionally since in the practice of others." (pp. 28, 29.)

We need not adduce any further evidence to support the opinion we started with—that opium is no more necessary for the treatment of the drunkard's delirium, than is that which caused it, alcohol. But there must be, then, it may be replied, some great fallacy supporting the dogma that opium is the indispensable agent, the *sinè quâ non*, for inducing what is termed the "critical sleep." Dr. Ware well observed (p. 43), that absence of sleep is one of the most remarkable symptoms of the disorder, and that when the latter terminates favourably it terminates in sleep. It seems therefore, at first sight, reasonable that the treatment should have had for its primary endeavour to bring about this termination in sleep. The patient, it has been emphatically said, "must sleep or die." Speaking generally, no doubt but this is quite true, though exception has been taken to the axiom. From a defective knowledge of the specific toxæmia before us, of the natural history and course of it as a particular manifestation of alcoholism, it was concluded that the "sleep" was the *cause* of the salutary changes and favourable termination of the attack, and that such sleep must be, and could only be, produced by powerful and repeated artificial aid. Now, more recent and cautious investigation teaches us that "sleep" is the *effect* and not the cause of the salutary changes, &c., which have ensued from the elimination of the poison, and thence arises the termination of the paroxysm of a self-limiting disorder. We learn also that when the paroxysm is allowed to go through its usual series of changes without the interference of art, this "sleep" is obtained within a definite period; and on the contrary, that little or no effect is attained by attempting to cut the paroxysm short by opium, unless extreme narcotism be induced. Further, the induction of this hypnotic state, instead of being advantageous to the patient, but adds another serious danger to that which already threatens him. We have lately watched a case—simply here as an observer—in which for two days one drachm of the tincture of opium was given every two hours, a pill of camphor and a grain of solid opium in the intervening hour, as also four pints of beer and two glasses of gin daily. No sleep followed, and the amount of opium was *materially* reduced through fear of the consequences. At length, seventy-two hours or thereabouts having passed over, sleep came on; the opportunity was instantly seized to give some more opium, the critical sleep followed, and which was, we were then gravely assured, brought about by the beneficial effects of opium! This patient, we may likewise state, had taken two glasses of gin daily and a pint of beer from the commencement, nearly three days before the manifestation of the vigilant and delirious paroxysm. Dr. Bennett (p. 412) thinks that much of the supposed beneficial action of opium is dependent "on coincidence with the muscular fatigue and exhaustion which at the same time accompany the elimination of the poison and the tendency to repose." Somewhat upon this principle is to be explained, perhaps, the benefit of the "walking drill," which, according to Dr. Blake's experience in the West Indies, was found efficacious in warding off attacks of delirium tremens in the case of drunken soldiers, not from such exercise proving a new stimulus in place of the rum, to which they had no access, but from its wearing-out and eliminating effects, while the proper nutrition of the body was carefully maintained. We may here, with appositeness, refer to the second and fourth cases detailed by Dr. Cahill, in which sleep followed after fatiguing drives, ordered to be performed as curative means by this physician.

It may be replied to us, that even admitting opium is not always of the value supposed, yet the cases recorded by different writers show how great a tolerance there is of this drug under the circumstances, and that no injury arises from its use, whilst we are also bound to remember the numbers of instances which *seem* at least to imply recovery in consequence of its employment. True it is that there is often apparently a great sufferance of opium shown during the paroxysm, and that patients recover after a narcotism has been forced on before the crisis. But in other cases there is not any such tolerance seen, and though a narcotic slumber be induced, it culminates in the sleep of death, often preceded by coma and convulsion. If, however, this extreme effect be not brought about, any sleep which results is very short and disturbed, and followed by a delirium as bad as before. Even the most strenuous advocates for the opium treatment are bound, we think, in fairness, to admit, with a disciple of their own school (Mr. Philips), that—

> "Every case of delirium tremens is not to be cured with opium, or we should not have nearly 250 deaths annually from this cause alone; neither would Klapp have advocated the exclusive virtues of tartar emetic, nor others that of bloodletting or cathartics."*
>
> "To the 286 deaths," says the Registrar-General, "ascribed by the informants to intemperance, 536 deaths by delirium tremens should be added, making 822 by alcoholism, besides many other deaths by secondary diseases."†

Now these deaths occurred, no doubt, after the employment of opium and stimuli; and it is worthy of remembrance, that Dr. Ware's only cases in which death took place after sleep came on, were those which had been treated by large doses of opium, whilst out of 29 treated "expectantly" only 1 was fatal.

Dr. Laycock's explanation of some of the sources of fallacy here besetting us, is quite satisfactory to our minds, though, as we shall presently see, strongly repudiated by others. He says—

> "It is a remarkable illustration of the influence which usage and routine exercise on the judgment, to find how unsuspiciously as to its bad effects opium is prescribed in this class of affections. Patients who sleep and recover after its administration, sleep and recover it is said *propter hoc*. If they do not sleep they have not had enough, but if they sleep and die comatose, with livid face and contracted pupils, then the disease, and not the drug, proves fatal. Here, I think, is a whole string of fallacies." (p. 303.)

If it be true that opium and alcoholic stimulants singly are to be deprecated in the treatment of delirium tremens, we can but say that their combination is a twofold evil. Yet it is by this union that the disease is treated throughout this island—

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* Ranking's Abstract, vol. v. p. 15. 1847.

† Eighteenth Annual Report, p. 186. London, 1857.

> "The common practice [in Scotland] has been," says Dr. Peddie, "and I have reason to know still is, to give from one tablespoonful to a wineglassful of spirits every two or three hours, either alone or combined with opium." (p. 27.)
>
> "Put their opiate dose," writes Dr. Watson, "into a glass of gin or pint of porter."
>
> "Porter and gin, brandy and wine, are given freely," writes Dr. Dickson. (Op. cit., p. 655.)
>
> "But," says Solly, "I always combine them with opium and ammonia."
>
> "The remedy for the disease is opium. . . . We must allow drams or anything else that has become a bad habit." (Elliotson.)
>
> "If a man has been accustomed to drink largely of malt liquor, a drachm of laudanum will act much more beneficially if taken in a pint or a pot of beer, than if taken alone; a similar remark may be applied to other spirituous liquors."*
>
> "Drachm doses of laudanum," writes Dr. Cormack, "may be alternated every two, three, or four hours, with a couple of grains of the watery extract of opium till the patient sleep. These may be combined with mulled ale or wine, brandy punch."†

The proper answer to these and analogous recommendations is the following—viz.; out of 403 cases treated in the Royal Infirmary of Edinburgh with alcoholic drinks and opiates, during three years and a quarter, 101, or 25 per cent., died; whilst in 24 cases admitted last year under Dr. Laycock, and in 4 private cases, one death only occurred, and which was after the use of opium and alcoholic excitants, the others having been treated upon an "expectant" plan. From 1842 to 1848 there were 35 cases admitted into the Glasgow Infirmary, of these, 17, or nearly 50 per cent., died; whilst Dr. Peddie tells us that during the last ten years he has treated "upwards of 80 cases of the genuine disease, many of them were very severe ones, with uniform success," upon principles totally opposed to those of the school of opium and brandy-and-water. Dr. Morehead writes—

> "I can say nothing of the treatment of delirium tremens by free opiates and stimulants in temperate climates, but I feel myself justified in very positively asserting, that in the delirium tremens of Europeans in Bombay, it is a course of treatment attended with much hazard, and which, when systematically followed, is certain of leading to unfortunate results."‡
>
> "Though, as I have elsewhere remarked, figured statements as data from which alone to judge of the success of medical treatment, are open to very evident sources of fallacy, and must be used with much caution; yet I feel satisfied that I run no risk of misleading others, when I point to the statistics of the European General Hospital, in proof of the greater efficacy of the treatment of delirium tremens by the means and in accordance with the principles here advocated. From 1837 to 1841, the years during which I became convinced, from careful clinical observation, of the evils of an exclusive opiate and stimulant treatment, the mortality from delirium tremens was 24·5 per cent. Whereas, from 1842 to 1853, a period during which I know that the disease was chiefly treated in the manner recommended by me, the mortality was 9·4 per cent. Why the year 1848, in which the mortality again rose to 20·4 per cent., is the single exceptional year of these twelve, I am unable, from the data before me, to explain; but it would be interesting to inquire, by examination of the diaries, of that year, whether there was not then a backsliding into the old and, I fear, still too common system of treatment in some quarters."§

We are quite willing to admit with Dr. Macpherson, that tables of mortality from the disease we are discussing must be looked at with great circumspection, as so little care is often taken in the classification of cases of *ebrietas*, *acute intoxication*, *mania à potu*, *delirium tremens*, and *apoplexy*, to say nothing of not discriminating between complicated and uncomplicated forms of the trembling and vigilant delirium of the drunkard. Whilst Dr. Snowden gives the mortality‖ as low as four per cent., there occurred one of no less than 37⅓ per cent. in the Royal Infirmary of Edinburgh, between July, 1843, and September, 1847, which rate, however, is yet exceeded by that of the Glasgow Hospital, where one of nearly fifty per cent. was attained between 1842 and 1848. Dr. Macpherson remarks:

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* Philips, op. cit.

† Methodus Medendi, p. 508.

‡ Transactions, No. 6, p. 131.

§ Clinical Researches, &c., vol. ii. p. 550.

‖ Craigie, op. cit. p. 86.

> "For the first three years when I was with European soldiers, I thought I could always cure the disease. After a period some sailors in Howrah staggered me; and when I came to the pensioners and invalids of the Calcutta General Hospital, I found that many cases were beyond all treatment." (p. 8.)

Not only are tables fallacious, but statements are directly contradictory. "Seamen," writes Dr. Laycock, "just after leaving the shore, are apt to suffer an attack, and have usually no treatment, yet recover after sleep;" whilst

> "Dr. Coates mentions, that in the port of Philadelphia it is common for sailors, on first leaving the scene of their frolics for a new voyage, to be affected with a degree of the disease known by the name of the horrors, and yet all these persons recover mostly under the use of strong drinks."*

And finally, whilst we have seen how some practitioners have reduced the percentage of mortality from refraining from the use of alcoholic fluids, we are told by others that "in Philadelphia the deaths were one in eight, but have been reduced by the use of stimulants to one in thirty-nine." But with all the difficulties and discrepancies surrounding this matter, we believe that unprejudiced inquiry will lead to the conclusion that the treatment of delirium tremens by large or frequently-repeated doses of opium, and by the continuous administration of spirits, is not warranted by experience nor in conformity with theory. We hold it also to be equally a truth that the withdrawal of accustomed stimulants is never a cause of the paroxysmal outbreak, but frequently a prevention of its more perfect development when only threatening the drunkard. In what the proper treatment of *delirium tremens à potu* consists, and the modifications of it often advisable in those cases of the disorder complicated with "shock," we leave as questions for consideration upon another occasion.

REVIEW VII.

The Cyclopædia of Anatomy and Physiology. Edited by ROBERT B. TODD, M.D., F.R.S. In Six Volumes, illustrated by 2853 Woodcuts and Engravings; accompanied by an Analytical Index for each Volume, and a General Index to the whole Work.—*London*, 1835–59. Royal Octavo. pp. 5427.

IT is not unlikely that many of our readers who glance at the above heading find themselves singling out the same part of it for special notice. One touch of Nature makes the whole world of kin. The wing of Time, like the foot of Death, makes no difference of persons. And a retrospect of twenty-five years gives much the same general vista to all active minds. The objects it comprises may be very different; and the nearer of them, even if similar, very differently lit by the intellect, and coloured by the passions, of the observer. But they are all ranged in strict accordance with what we may term the laws of mental perspective; their lines equally converging; their vanishing points identical; and, at a given distance, even their light and shade (at any rate, their colour) more uniform than might be expected. So that to look back on so large a portion of the term allotted to Man as a quarter of a century, probably suggests to all of us the same text, however different the sermon each may preach to himself thereupon. Misty hopes which have vanished, almost unregretted. True friends who have departed, leaving places perhaps filled up, but memories ever open. And going off into the now scarce remembered starting-point of infancy, the path of daily life with its long series of elevations and depressions, gains and losses, successes and reverses—objects which are continually dwindling down to truer relative dimensions, as the whirling sphere bears us onwards through the universe, and leaves them every hour more distant.

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* Craigie, op. cit. p. 86.

And perhaps the narrowing of such a view from a simply human, to a scientific, retrospect may, while it deepens the sympathies and the responsibilities of the gazer, afford no very different conclusion. Perhaps, indeed, the knowledge which makes the student of modern science in some sense the Priest of Nature, makes him in a certain degree its Prophet also: with the duty of not only seeing, but speaking, aright; of not only recognising, but proclaiming, that awful dawn even now breaking over the mountains, where he stands watching with his fellows. At any rate, the scientific labourer little knows the drift and object of his own pursuits, the true rewards of the toils which he will never live to complete, and with which he can scarcely expect that even his name will remain connected many years after he has gone down into silence, if he ignores the tremendous lesson his retrospect would afford him; or neglects, while gazing round on the new views perpetually opening out on his sight, to look backwards, as well as forwards, on his general course. If he cannot trace ninety-nine hundredths of his most worldly and material knowledge directly or indirectly to Christianity, he is not only ignorant of causation to a degree which scarcely fits him to be even a microscopist or entomologist, but really almost deserves the theological venom which an inaccurate (or even accurate) word sometimes calls down from devout—as distinguished from Christian—writers. And if, amid all the glare and clang of the thousand anvils at which Art and Science are now fulfilling the will of Omniscience, in forging the engines of the world's future destiny; amid the thousand giant forms of knowledge daily starting up into life (like the stones cast backwards by Deucalion and Pyrrha), from the casual observations of men of the past; if, in all these, he cannot trace the onward progress of a world that is daily ripening to

"One far off divine event
To which the whole creation moves,"

—why, then, he reads the lesson of his daily life to as little purpose as he happily once read, at his mother's knee, the prophecy of him who was the philosopher, as well as the prophet, of 2500 years ago; and who, sitting on a height of knowledge, human and divine, approached only with reverence by our own Newton, uttered as the summary and the end of his presagings, the comprehensive prophecy, that "many should run to and fro, and knowledge should be increased," in these latter days on which our lot has been cast.

Perhaps considerations like these may seem out of place in a scientific Review. Perhaps some would even prohibit us from mentioning any topics but such as are capable of being strictly classified under special departments of Medicine or its collateral sciences. But surely, not to dwell on the pleonasm really involved in that vulgar phrase "scientific knowledge," or on the absurdity of supposing that we are to dissect through all the details and husks of knowledge, in order to let its costly kernel drop unheeded; surely it becomes us, as a profession, not to add any wilful concealment or distortion to the errors of that portraiture in which the general public has been taught to recognise our body corporate. The attitude of the medical profession towards religious subjects is wofully misunderstood. From the days of Chaucer, whose physician's "reading was but little in the Bible," every scribe and poetaster has depicted us as comparatively irreligious, addicted to materialism, disinclined to believe, and especially averse to anything like being preached to.

All this arises from an error which, if not inherent to the human mind, at any rate reaches back in the world's annals to long before the historic age. Repeated under various phases, its most definite aspect in the present day is that mistaking of theology for religion, which is, we are convinced, the chief source of the above opinion respecting the medical profession, as well as of that theory respecting the mental influence of medical studies which is generally deduced from it.

In this respect Medicine is likely to gain incalculably from being associated with various other sciences, which, like her, are gradually asserting their dominion over the powers of Nature. The tone of remonstrance—nay, even the very words—she would adopt in defending herself against such an accusation, are daily and hourly being used for similarly defensive purposes by the adepts of cognate branches of knowledge. "Religion," one can almost hear them saying, "is a word which we reluctantly recognise as the conventional epithet for a faith and practice which, with all our minor differences, most, if not all, of us heartily accept and revere. Theology, you tell us, is a science; susceptible of advance, if not of discoveries; and requiring, like other sciences, special study for its comprehension. Let each pursue his investigations in peace, content to know that, if accurate, their results must be compatible and harmonious with each other. In the mean time, it seems a very law of Nature that the variety of knowledge we cultivate not merely knits us together in stricter social union (*emollit mores, nec sinit esse feros*), but almost disqualifies us for that peculiar energy of aggressiveness which you deem it your duty to display. It is our indisputable maxim, that error is only to be attacked by the setting forth of truth. It seems your inevitable practice to denounce what is wrong, far more than to display what is right. Looking to the history of our common religion, we venture to hope that its spirit is at least as compatible with our pursuits as with yours—nay, more, that its progress is quite as much involved in our successes. In the mean time, so soon as it becomes your incontrovertible duty to attack us—when the time has arrived that the task of correcting our errors really presses on you for fulfilment; when the beam of hatred and uncharitableness having been extracted from the eye of the devout world (a feat of ophthalmic surgery which Bowman himself might fail in), you may with authority attempt the removal of any smaller obstacle from our mental vision; when the Discord on one side, and the Driblet on the other, are no longer filled with cursing and bitterness, or with insinuations of slander and falsehood—then we shall gladly listen to you. Only pray let the delegates you select for this missionary enterprise know a little about the persons and studies they condemn; and, if they cannot quite adopt the apostolic maxim of becoming 'all things to all men,' at least let them so far humour our prejudices as to acquire an elementary knowledge of the sciences whose deplorable tendencies they denounce and expose. One theological school is construed to say, that ignorance is the mother of devotion. Another is hinted to have found it no insuperable obstacle to the episcopal bench. But we believe it to be *ipso facto* a disqualification relatively to some audiences, if not to all preachers; and suggest, that to influence us, you must so far do violence to your most cherished instincts, as to open your eyes as well as your mouths. Perhaps you will then find that our creed is singularly like your own; our practice not very dissimilar; our temptations—to some sins, at any rate—much fewer; and our earnestness on all points of real importance deepened, in spite of being widened, by the study of that material world which the noblest minds of all ages have agreed with the Hebrew Psalmist in regarding as one of the chief means of instruction and discipline for the heart, as well as the hand and the head, of its human inhabitant."

A comparison of the progress made by Physiology during the last twenty-five years with that of the mixed sciences most allied to it shows a close similarity in the rate, and still more in the kind of advance which they have effected. In all, we trace a great though gradual progress; and notice a vast increment of materials. And though at first sight it may seem, that there has been very little discovery in the largest sense of this word, few instances in which the torch of science has lit up new fields hitherto quite dark and unexplored; yet perhaps a more careful consideration would suggest abundant and satisfactory reasons for such an apparent deficiency.

Doubtless many of the details of knowledge gained within the above period are of a magnitude and usefulness which almost entitles them to rank as discoveries. Indeed, hardly a science can be named, the mere mention of which does not recall several such acquisitions. In therapeutics we have gained cod-liver oil and chloroform; in chemistry, ozone; in electricity, the researches of Du Bois Reymond; in optics, the stereoscope and the photograph; in geography, the new and gigantic outlines which Barth and Livingstone have assigned to the conformation of the African continent; in engineering and mechanics, a number of brilliant achievements, amongst which nothing but its close relation to physics justifies our selecting the modern theory of marine architecture derived from a study of the wave line. While in those purer sciences which range the universe from the atmosphere of our own world to stars incalculably remote, the discovery of new planets, of laws of magnetism, of theories of storms, and a variety of like results, show that their cultivators, even if less numerous than the throng of zealous microscopists who now-a-days add their mite of information to the treasury of anatomical knowledge, compensate for the comparative smallness of their numbers by a more complete and early scientific training—as well as by deeper insight and more arduous exertions—in those loftier fields of research in which they live and work.

Hence, while the assertion, that this is the age of discovery rather than of discoveries, is perhaps sufficiently paradoxical to alarm an English reader, few would probably quarrel with the statements into which it naturally expands. It is the very number of the results daily and hourly won by modern science which conceals their magnitude; the very richness of the heritage bequeathed us by our scientific forefathers, which depreciates the value of its further accumulations. Much of what we call discovery lies rather in the circumstances under which a new and important fact has been laid bare, than in the mere fact itself; so that the degree of its prominence, and even the duration of its isolation, may render it more impressive than an equally valuable acquisition, which drops at once into its proper place in the body of science, or is rapidly worked out into those larger results among which it almost disappears from view. Thus, on the one hand, it becomes increasingly more rare for the scientific labourer to find, in the common incidents of daily life, the startling fact which, to an observant mind, opens up a whole aspect of Nature, or a large series of cosmical phenomena. And on the other hand, if some such fact be found, even of secondary importance, it is not long that it retains any identity, or admits of any distinct recognition. Diffused by the press over a vast area of the civilized world, it at once becomes the property of a host of active minds. An army of observers, so to speak, attack the subject; or, if we may modify the simile, pour through the breach made in it by the first assailant. Only for an instant do they follow his steps: soon their paths diverge; and by and bye, when the stronghold of Nature is won, perhaps the discovery and the discoverer have alike passed out of knowledge. And it is in this admirable but unconscious unity of working, this organization of scientific labour, which we trace to the teachings of our immortal countryman, that we find the clue to the amazing fertility in discovery at the present time, coupled as it is (necessarily, we think) with a diminished rather than an increased prominence of individual gifts and acquirements. Just as, in the warfare to which we have likened it, the burly but undisciplined ferocity of the Homeric warrior has been replaced by the regimental discipline and the weapon of precision, so among the modern soldiers of science, skill conceals strength and courage, and unity of purpose and of training secures for all the combatants a kind of external resemblance to each other. It is no longer a lonely demigod struggling with Proteus on the wild sea-shore, but an organized army, whose efforts are gradually, though slowly, conquering a dominion over Nature: the Nature sketched by Bacon, to be watched, obeyed, and so won. And while we have abundant grounds for asserting, that even the physical strength of the modern soldier casually attains its full development, however undiscernible it may be when the eye merely glances down the rank and file, so we are inclined to think that our scientific organization, while it certainly secures the mental strength and health of the greatest number, rarely if ever restricts or diminishes these qualities in the individual. In both cases, at any rate, some slight or doubtful advantages might well be sacrificed to the all-important consideration, that the battles they respectively fight are really gained by the discipline and unity with which they have been trained to combat.

It would not be difficult to show, that Bacon was himself aware of this inevitable result of the method of scientific inquiry he aimed to introduce; that he foresaw how, while its results would interest all mankind by their usefulness, by their applicability to the purposes of every-day life, the procedure itself was one which all, in their several stations, might unite to practise. But it may be doubted whether even his prophetic vision fully appreciated the advantages destined to be derived from that principle of coordination which was really involved in the whole of his teachings. It can hardly be denied that the kind of induction he suggests is one which is habitually made use of even by the lowest of intellects, and in the concerns of every-day life; so that, to borrow an illustration of Lord Macaulay's, the process by which a querulous dyspeptic finds out that this or that article of food has been the cause of his attack, is strictly a Baconian induction. And it is equally certain that Bacon implies (what indeed several of his own experiments well illustrate) that the tasks of collecting materials on the one hand and of construing their import on the other, must often be executed by very different labourers; and that, in practice, the former must often be shared between large numbers of persons, and spread over long periods of time. But it is further the singular merit of the principles he deduced, that they not only constitute the best and fittest guides for the philosophic inquirer, irrespectively of the rank of intellect he possesses, and of the special object of his search, but also that they invite the assent and cooperation of multitudes of persons, and promise to render the cultivation, and even the extension, of science a work for every thinking man to engage in. Those who cannot build an edifice of theory may yet help to dig its broad foundations in facts: those who cannot question Nature by experiments so arranged as to elicit a definite and connected answer, may yet listen to her hints and suggestions, as they appear in the incidents of her continuous daily work. Profound and arduous researches may be prepared for and supplemented by casual observations; and the highest efforts of the human intellect, aided by supplies of information furnished by little more than happy opportunities, keen senses, and a truthful mind. To the scientific edifice thus constructed almost every one can contribute something, if it be but a few bricks. At any rate, as regards his materials, he will find no deficiency. The clay to be fashioned is everywhere around him, and there is no lack of straw; he has but to mould it into simple and accurate shapes.

Thoughts of this kind inevitably suggest themselves in attempting to review, however briefly, the progress of any modern science. Still more when the completion of a scientific Cyclopædia is the occasion of such a retrospect. Every good work of this kind is indeed a monument to our great British lawgiver of science. And the obligations to Bacon acknowledged by the French Encyclopædists a hundred years ago are now more striking and prominent, just in the proportion in which knowledge has advanced since that time. Since then, what changes have passed over the world, and what mighty revolutions (in science as well as politics) have swept the country once rendered illustrious by the gentle and high-souled d'Alembert and his scarce inferior colleagues! How amusing now to read of this true gentleman, the first philosopher of his day, being looked down upon by the clever (but essentially vulgar and anile) Horace Walpole! Especially how interesting on an occasion like the present to notice, that with all the necessary faults and shortcomings of the Encyclopædia itself, it still remains a noble product of the human mind, a landmark of its age, a measure of the advance of knowledge up to the epoch of its publication, and above all, a testimony to the value of the Baconian method; which in spite of national* prejudice, a foreign language, and a very different cast of thought, seems to have converted the most highly civilized nation in Europe at a time when many Englishmen were still far from appreciating its importance.

We may be told that to praise Bacon now-a-days is superfluous. That to assert his system is to utter truisms. That, indeed, it would better become those who wish to guard the public against error, to favour that reaction towards dialectics which some of the greatest scholars and thinkers of the age evidently think it desirable to introduce. Our answer to this is simple, but we conceive satisfactory. No thinking man ever supposed—much less did Bacon ever do so—that one particular function of the body or mind could or would be wished to arrogate the office of all, or to govern the whole organism for its exclusive benefit. And no one can reasonably imagine that general training is rendered unnecessary by the enunciation of rules for some special procedure. Least of all can it be argued that scholarship, whether as a discipline itself developing the youthful mind, or as a partial preparation for the particular calling of the adult in after life, can be supplanted or rendered unnecessary by the few and simple rules to which Bacon's philosophical method may be essentially reduced. His object was, not the teaching of mankind, but the advancement of science. And hence, while his method is no substitute for education on the one hand, so neither are its uses restricted to a highly educated class on the other. And though, as a rule, we incline to think that a vast majority of the great discoveries of science are achieved by men who are highly educated, even if self-taught; still it stands as one of the main advantages of the Baconian system, that it claims and utilizes the talents and opportunities of the meanest and least instructed of mankind. It is only by the large views of scientific unity and brotherhood thus suggested, only by this appreciation of the republic (so to speak) of scientific labour, that we can understand the diversity and number of the details daily accumulated, or sift out, from the chaff of innumerable statements and opinions, the wheat of far-seeing theory or of solid fact. A brief or slovenly communication in some little-read Journal or Magazine, an isolated observation by some one perhaps comparatively ignorant of the very science to which it refers, may contain or suggest precisely the clue for which a more philosophic inquirer has long been in search. And as it is rather by the manner than the matter that the greatest "*helluo librorum*" must now-a-days judge whether the essay or book he is skimming really deserves perusal, so it is by the whole cast of an inquiry, by its means rather than its results, that we often have to judge how far these will stand the test of time, or meanwhile claim admission into the body of a science. And what thus holds good of isolated facts or researches is equally true of the science they form by their union. So that the question which we are about attempting, however cursorily and imperfectly, to answer, "What has been the advance of Physiology during this particular epoch?" suggests the preliminary inquiry, "By what means has this advance been sought, and how far have these been in consonance with the principles of Bacon; how far, in short, are they true inductions?"

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* "Comment nous ont-ils (les philosophes) présenté l'encyclopédie. Comme un monument immortel, comme le dépôt précieux de toutes les connaissances humaines. Sous quel patronage l'ont ils élevé ce monument immortel? Est-ce sous l'égide des écrivains dont la France s'honorait. Non, ils ont choisi pour maitre et pour idole un Anglais, Bacon. . . . Les philosophes ont eu recours à l'Angleterre. Ainsi un ouvrage fait en France, et offert à l'admiration de l'Europe comme l'ouvrage par excellence, fut mis par des Français sous la protection du génie Anglais. O honte! Et les philosophes se sont dit patriotes, et la France, pour prix de sa dégradation, leur a élévé des statues!"—Fievée: Lettres sur l'Angleterre. 1802.

Dissection.—It is a healthy character of modern Physiology, that not even the enormous value of the many new methods of investigation it has acquired, has seduced it into a neglect or contempt of those formerly in use. In the mere dissection of the human body, so much had already been done, that the last twenty-five years have not allowed the anatomist's scalpel to work out a great deal both new and important. On the contrary, some of those discoveries of the "law within the law," which it is the general tendency of Physiology to reveal, have reduced many of the details of human anatomy to a comparatively subordinate import; so that it is no longer the fashion to seek eminence by multiplying fasciæ in describing the parts of hernia, or to pin down in the student's memory some hundreds of trivial branches of arteries and nerves, scarcely exactly alike in their size and distribution in any two corpses he may examine. But, while dissection has shown that nerves, as well as arteries, are liable to some vagaries in their course, it has also confirmed the real exactness of their origin and terminal distribution. And we must not forget to add, that this old-fashioned procedure has done much, and has still more to do, in the vast and inexhaustible field of Teratology; of which study we will only say, that it is a pity the rare and fleeting opportunities occasionally met with in midwifery practice, are not more frequently laid hold of by our profession. Plain good dissections of this kind, made rapidly in the recent specimen, would help to fill up many a gap in those noble outlines (for really we cannot call them more) of this subject, which have deservedly added the name of Vrolik to those of Meckel and the older anatomists.

Perhaps, while alluding to dissection, we ought not to ignore the various means by which it is sought to preserve or alter animal tissues, so as to render them better adapted for this process. Just as Gall and Spurzheim improved the dissection of the brain, by modifying and refining the old processes of cutting and slicing its mass, so few of our readers are probably ignorant of the patience and skill with which the scalpel and the microscope have been prepared for, and aided by, the methods of preparation adopted by Van der Kolk, Lenhossek, Lockhart Clarke, Kölliker, Beale, and many other observers. Whatever suspicions have sometimes attached to the results of the chemical reagents thus applied, it is impossible to doubt the great value of the information they have often helped to supply; and the general argument they afford, as to the probable trustworthiness of the observations only secured by so much preliminary toil. Even the very epoch when anatomy sought to prove everything by injections, and was fain to regard the whole body as a mass of bloodvessels, could never have shown anything to equal the beautiful preparations of this kind which Quekett, Rainey, Kölliker, Frei, and others, have made merely a casual incident of their profounder anatomical studies. Indeed the mechanical details of this process are now so severely checked and tested by the microscope, that we can afford to look down on the best productions of the older anatomists, and expect to find ruptures and extravasations in the *chef d'œuvre* of a Ruysch.

The Microscope.—How, however, shall we speak of the instrument which thus obtrudes itself into the field of dissection? Or, how, in a few lines, explain the enormous impetus given during the last twenty-five years to the use of the microscope; the results by which it has reacted on physiological science; and the merits, advantages, and dangers of that assiduous cultivation, which it is now receiving on all sides?

At first sight it is difficult to understand why so valuable an instrument of research, an instrument tolerably constructed, and well known, nearly two hundred years ago, should have suddenly assumed such a vastly increased importance in little more than the last tenth of that period. True, the powers of the instrument have lately been raised, as well as its price lowered. But, to say nothing of many of the greatest discoveries in this, as in other departments of knowledge, having been made with comparatively inferior instruments, or of the improvements having often been the effect, rather than the cause, of an increased use of the microscope—the parallel improvements of the telescope, without any corresponding increase in its use, render any such explanation very incomplete. Nor, indeed, do we think any single and sufficient explanation can be given, why the discoveries of Leeuwenhoek should have been separated by so long an interval from those of Bowman and Kölliker. Doubtless, in the research of this or that particular tissue, the older instrument was of little avail. But it is abundantly evident that, had Leeuwenhoek and his successors bestowed their attention on the worthier objects to be found in the human body, they might have anticipated many of the results only since discovered in the period we are now reviewing.

In point of fact, everything indicates that many causes concurred in this curiously irregular development of an interesting branch of knowledge. The mere instrument is singularly liable to optical defects, and these unusually influential. The badness of a telescope scarcely does more than limit our view; that of a microscope, under parallel circumstances, distorts it. The supply of light, too, is a requirement almost peculiar to the microscope. Then, again, the imperious wants of Medicine and Surgery, as practical arts, doubtless helped to dictate the direction of growth in a science which was only known as collateral to (and even as an offshoot of) them. It was thus the accomplished anatomist, and the skilful operator, who were for some time the chief labourers in the field of physiological research. And it was only when a long line of illustrious men of this class, from Harvey to Hunter and Magendie, had done a good deal of what Anatomy and experiment could effect in conjunction; only when Chemistry had been stripped of the unfounded therapeutic pretensions with which its too zealous admirers had invested it; only when even a Meckel and a Treviranus seemed approaching the confines of their subject, in all the breadth which their genius and industry permitted them to assign it, that the microscope and its cultivation came forward, to push observation to limits unthought of before, and to extend it in many respects far beyond the very range defined by the term "anatomy" itself.

It is unnecessary to insist on the advantages thus conferred on Physiology. The rule, that structure exemplifies (and even indicates) function, holds good of its minuter, equally as of its larger, details. But in addition to this law, which applies, for example, to the minute structure of a muscle, in the same way, though with a great increase of extent, as do those ruder outlines revealed to the naked eye by dissection, the full and complete lesson derivable from the microscope deduces another law, which might almost be contrasted with the preceding as its opposite. Showing, as it sometimes does in the lowest forms of animal life, an absolute deficiency of those structures which minister to a particular function in the higher animals, at the same time that it permits us plainly to recognise what is, to all appearance, a persistence of that function itself; it corrects the error which mere anatomy, in its larger form, could never have exposed. So long, for example, as we could only follow out the mechanical division of the muscular structures with the naked eye, until all further subdivision was prevented by their increasing minuteness, the function of active contractility might justly be regarded as the expression of a peculiar structure. But when the microscope showed this function in lower and smaller forms of animal life, unmistakeably exercised by a membranous expanse, or a solid mass, in which it was easy to prove the complete absence of all differentiation of tissue; from that moment the act of contraction acquired a new and more recondite, not to say a higher significance. Reduced, it is true, to a simpler (or rather, wider) law, it became attached to the substance, rather than to the arrangement, of the contractile mass; and all the structures associated with its organs in the higher species, however they might aid or govern this power, favour or restrict its application, heighten or economize its energy, were shown to be essentially unnecessary to its exercise. To speak logically, they belonged to the *quale*, but they no way involved the *quid*.

But the solid advantages derived from the microscope depend greatly upon another circumstance, which, well illustrated by the latter half of the very Cyclopædia that suggests these remarks, is, in no small degree, attributable to the exertions of a few individuals, and especially of some whom we have already alluded to. The larger advances of microscopical science are of course mainly the results of that systematic and co-operative labour which we have already specified as forming a distinguishing feature of modern science. And it is fair to presume that, long before the gradual development of the subject had attracted wide attention, there was a growing inclination in the minds of many towards minute anatomy. But few writers or books have more helped to form a taste, or to guide a science, than Todd and Bowman in this country, and Kölliker in Germany. Those of our readers who recollect the aspect of general anatomy before the publication of the English and German Text-books just alluded to, will probably agree with us in thinking that microscopical science owes a great debt of gratitude to their accomplished authors. To our English colleagues especially, who found a number of scanty, obscure, and discrepant details; and left a system of general anatomy, the originality and accuracy of which are beyond all praise, especially when it is recollected that it was mainly achieved in the intervals of medical practice. And even admitting (what can hardly be denied) that the researches of Kölliker have since afforded us a far more complete and exact account of many of the tissues, we can hardly forget, both that the 'Physiological Anatomy' of the English authors had the merit of inspiring and guiding the accomplished author of the 'Gewebelehre' in his more protracted and elaborate researches; and that, in respect to its greater brevity, and its closer alliance with physiological considerations, the British Text-book has probably had a more useful, as well as more extensive, influence on the profession at large.

The cautions which apply to the use, or rather the abuse, of the microscope, are unmistakeably deducible from the history of Physiology during the last few years. That there should be casual, and even frequent, errors in observation, was of course only to be expected from the circumstances, both of the objects and its instruments, under which microscopists laboured on the enormous mass of details laid before them. That, now and then, inductions should be hastily made, from too small a number, or too narrow a range of facts, was equally to be expected; and was another kind of error which (like that too early reduction of knowledge to a system against which Bacon had cautioned his readers) its cultivators were pretty sure to share with those of every science;—an error, indeed, so inevitable as to be almost a condition and stage of individual or collective progress. But experience seems to have shown us other risks, more specific to the subject, and especially worthy of being recollected by those who aim chiefly at making the microscope ancillary to Physiology and to practical Medicine.

First let us point out, that there is some danger of microscopical details usurping the place of Physiology; using this word in the sense of those laws of life and organization which are our best guide to the study of disease, Anatomy is not Physiology. And the kind of anatomy revealed by the microscope constitutes but a section of the whole subject. Indeed, just as, in spite of several brilliant exceptions (among which the writings of Struthers and Ward are prominent), it must be confessed that modern descriptive anatomy is painfully deficient in those practical deductions and applications which alone can vivify and impress the dry bones of detail; so the infinitely more numerous and minute details of microscopical science flood some of our Text-books and Lectures to such a degree, as almost to swamp their scanty admixture of physiological knowledge. Indeed it must not be forgotten, that the details furnished by the microscope, sometimes quite irrational in the sense of their suggesting no idea whatever, must often have far less obvious and close relations to function, than those of the larger aggregates of the animal tissues, to which descriptive anatomy refers. It is really exasperating (considering the shortness of life and the value of the attribute of reason) to know how some lecturers and authors are constantly inflicting, on their hearers and readers, measurements which pretend to specify thousandths of lines and inches. Does any one in his senses believe these statements? Can any one assert that the granules or cells professedly measured to the ten-thousandth of a hair's breadth, have actually undergone a rigidly exact mensuration; or that, even supposing the systematic author does not copy his statements from those of some casual observer, the variations in size of the millions of such particles do not range widely on either side of this arbitrary and useless standard? Does the mind really appreciate these magnitudes, or distinguish between tweedle-dum and tweedle-dee to this almost infinite extent? While as regards the other proposition above stated, it is evident that there is scarcely an organ whose function is not more directly, perhaps more accurately, suggested by those features of which descriptive anatomy takes cognizance, than by that structure which the microscope reveals. Its shape, size, relation, arrangement, and even the colour, consistence, &c., of its mass, would in themselves go further to assign the function of the lung or the stomach, or any particular bone or limb, than any mere specification of its minute structure; which if exclusively relied on, would lead us to the inference that the femur of a newly-born quadruped was functionally a different organ than that of the older animal. Indeed, as already mentioned, the laws suggested by microscopical details have all the character of secondary or subordinate principles: invaluable as middle terms of the science, and as conditions which regulate the manifestations of this or that function; but generally allowing us to recognise some higher or more recondite source, some simpler law, for the main fact of the function itself. Rarely or never, through the animal kingdom, do we find special structural details linked in invariable association with this or that function, or group of vital acts. Reduced to its simplest form, locomotion is effected without muscles; volition without brain or nerves; circulation without heart or vessels; secretion without cells or glands. And while it may almost be doubted whether, in respect to the essential acts which are thus shown to be independent of their usual associations, we are much nearer to any full and true explanation than we were twenty-five years ago, it seems certain that the microscope, by subdividing the structures of the organs, rather complicates than simplifies our notions of these acts, and affords absolutely no information upon their nature, or even their agents.

Nor must it be forgotten, that though the microscope itself is simply a means of observation, the risks of error in its use are far greater than such a proposition would seem to imply. In the majority of instances we observe, not so much the structure itself, as the appearances from which that structure is to be deduced; by a process which combines, compares, and analyses them, and thus superadds reasoning to mere observation. And considering the difficulties inherent to such a process, as well as the allowances required for the circumstances of refraction and reflection, and of those derangements of the original structure which manipulation and decomposition have often brought about, it is not to be wondered at that mistakes have frequently been made. But though the casual blunders of the microscope (like those of the stethoscope) are sometimes eagerly laid hold of by persons desiring excuses for their own ignorance, it is but rarely that they have been ascribable to the instrument itself. In a vast majority of instances, they have either been such mistakes of a comparative novice as a moderate experience would prevent and correct; or what is still more common, have been essentially quite independent of the microscope:—errors, not of observation, but of induction; not in seeing the object, but in reasoning from what has been seen, to what is presumed to exist; errors, in a word, such as no instrument could obviate or prevent, unless it were gifted with the highest powers of the human mind. An exactly parallel case may be found in the stethoscope, the sounds really heard through it being sometimes gravely alleged to signify a pneumonia or a tubercular cavity, and the unhappy wooden instrument getting the blame which should attach to the auscultator; who, though he has observed accurately enough, has wrested unmistakeable matters of hearing to logical conclusions which they were quite unable to sustain.

If such considerations be true, surely they suggest some deliberation as to the degree in which the mastery of microscopical details should enter into medical education. Considering the disproportionate burden they inflict on the student's memory, and the slight stimulus or exercise they afford to his higher mental faculties, they really seem to constitute about the worst training which could be devised for a growing (and especially a solidifying) intellect, such as the age and calling of the student of Medicine together implies and requires. A practical familiarity with the instrument is doubtless desirable; indeed, it need hardly be said that such an acquaintance with it is now and then indispensable to accurate diagnosis, and therefore to practical Medicine. But such a familiarity could be so easily acquired and tested by the use of the instrument itself, that a lecturer might now fairly insist upon its being made a preliminary to his teachings; and, after satisfying himself that the requisite opportunities had been given, might limit himself to such a brief subjective description of the tissues as would serve to introduce the physiology of the organs they unite to form. So, too, of micrometry. A real standard is easily to be found (say an average human red corpuscle, in a saline solution of given density), such as every student might be assumed to know, and to which most other microscopic objects might be referred, as some simple fraction or multiple of its size.

Experiment—a term which we may conveniently interpret as implying observation varied by art—has assuredly done its full share towards the recent advances of physiological science. Indeed, that aspect of physiology which is in closest and most practical union with Medicine—the study of life in the higher animals—has probably received more elucidation from experiment than from any other implement of inquiry. Nor is it merely that the number of experimental researches on living animals has increased the bulk of such results. Their quality even transcends their quantity. The admirable "questionings of nature," which the very names of Blondlot, Bernard, Brown-Séquard, Bidder, and Schmidt, Valentin, Volkmann, and many others, probably recall to the minds of our readers, exemplify in the highest degree the co-ordinate working of senses, intellect, and reason; and especially illustrate that obedience of Art to Nature in which Bacon was fain to see the source of all scientific advancement. The elaborate conditions of experiment under which many of these researches were first made, and the no less elaborate variations of these conditions from time to time introduced, well explain the value of their results. Nor ought we to be surprised or disappointed that the results are sometimes discordant and inexplicable; that they occasionally contradict each other, or evade all interpretation. Such discrepancies and obscurities, while they express the vast extent of the science, form themselves the very paths and guide-posts for future advances. Sometimes it is the nicety of its collateral circumstances which prevents a given experiment from reproducing the results of the apparently similar experiment it seeks to imitate; the two being in reality dissimilar to each other. Sometimes the want of likeness is attributable to larger differences, such as greater skill and caution might have prevented; or conversely, belongs to differences too subtle for all analysis, and apparently dependent upon some of the less material powers of the animal organism. But in either case the discrepancies—while they illustrate the intricate character of the science, and enunciate the rule that in physiological research, and with experimenters equally trustworthy, a positive result immeasurably outweighs a negative—also furnish the stimulus and even the means, of further progress. In such encounters with Nature, nothing is so salutary as defeat; and rightly used and appreciated, the failure of to-day ensures, while it increases the success of some future period.

To classify such experiments would demand more space than we can afford in the brief and hasty view we are now attempting. But we venture to presume, that a careful analysis of some of the more striking and systematic of these researches would suggest to many of our readers the same proposition which they certainly do to ourselves—namely, that there is more of concord in the moral and intellectual aspect of physiology than a superficial glance would lead us to infer. Just as the far-sighted law of kindness and consideration to Man and brute might give us the clue to the most economical and expedient way of using muscular force, whether of individuals or numbers; and would certainly serve to group the experience of the Engineer and the Physiologist in intelligible and harmonious conjunction; so the scale of experimental researches shows gradations in which, as a rule, that inquiry which is conducted with the smallest amount of pain and suffering is not only, *ipso facto*, the most trustworthy; but is generally both the most able and accurate in its plan, and also the most successful in its results. Apart from the merely privative effects of pain and inflammation, which often frustrate those painful operations on the living animal too aptly termed "vivisections," such experiments often introduce active disturbances of the most serious kind; elements of confusion and error which many years of pathological research could alone suffice to explain or investigate. So that, as a rule, the more nearly the conditions of experiment approach to those of the healthy life of the animal, the more accurate are the deductions they yield or present, and the more skilful also is the inquirer who has managed to secure these conditions; while it would certainly be found that the more sanguinary operations of those who "carve the living hound" have not only done disproportionately little for Physiology, but have often been deficient in the plan, the reason, the justification, as we may term it, which philosophy generally dictates, and in such painful investigations ought, in the name of humanity, to require. Indeed, had Bacon lived in the present day, he might perhaps have sometimes felt urged to explain, that by the phrases "obeying" and "questioning Nature," he did not mean cutting short her course by wholesale slaughter, or extracting her reluctant answers by protracted and excruciating torture.

Of course it would be easy to wrest such remarks to a sense very far from their meaning. But they are not less true. Philosophically, we might pass from elaborate researches like those of Regnault, Reiset, Valentin, and a host of others, down to observations like those contained in the eloquent and suggestive papers with which Lewes and others are now so admirably amusing, while they instruct, the general reader; and find, in all these studies of natural history, a tendency to larger, better, and especially safer results, than in dissections like those with which many years ago the illustrious Magendie scandalized the English public. Doubtless the latter illustrate the principle by an extreme case. Doubtless, too, it is neither for the scientific press, nor for any individual, to judge what is cruelty and what is not; which vivisections are justified by their promise of indirectly assuaging the sufferings of Man and brute, and which are mere matters of thoughtless curiosity or trivial detail. As regards the heart and the head, which have respectively to decide these two questions, it is "to his own master" that each one of us "standeth or falleth." Hitherto, however, we may fairly congratulate the Physiology of the last twenty-five years on having experimented with animal life and animal suffering far more wisely, and tenderly, and successfully, than it promised to do when Magendie was the chief representative of this line of research. And those who have seen his pupil and colleague, Claude Bernard, perform his brilliant operations, or have witnessed, as many of our readers have done, the exquisite delicacy and skill with which Brown-Séquard experiments on his little friends and victims, must allow that both of them well exemplify the rule just laid down: that, with the infliction of no more suffering than attends the course of many an inevitable disease, they have planned and executed their operative experiments, so as to win inestimable results for the science they cultivate.

The question as to the repetition of such experiments, though perhaps equally delicate, is more practical, and indeed almost solicits a public decision. A special inquirer may decide for himself what amount of suffering and death he thinks it justifiable to inflict. At least he may be presumed to surmise the value of the results he seeks, and their bearing on the subject he pursues. But we venture to doubt whether unorganized curiosity entitles any man to repeat such operations, to add, it may be, a needless confirmation to facts already well established. And we should still more question the propriety of illustrating the ordinary medical course of Physiology by a hecatomb of slaughtered animals. The lecturer who has to teach medical students ought, we think, to treat his subject in a very different way, and draw his chief illustrations from a very different source. Such aimless and useless vivisections not only trench upon the time claimed by larger and more important topics, but are fairly obnoxious to the censure of the great Dr. Johnson, who has indignantly alluded to the hurtful influence the habitual infliction of these sufferings is likely to have on a mind which, like the Physician's, ought to be trained to the constant exercise of humanity;—and which ought, we may add, not so much to have lost its sympathy for suffering, as to have acquired the habit of diverting passive sensibility for pain into the channel of active effort for its relief.

Pathological observation has also been so largely and increasingly made use of as an implement of physiological research in the period we are reviewing, as to oblige us to notice, if not to analyse and appraise, its influence on the progress of Physiology. Directly, the deductions it suggests are at times invaluable; since it substitutes and replaces (so to speak) experiments on the lower animals, with the advantage that the observations it permits are made on the human subject, and without the disadvantages, both philosophic and moral, which we have traced as drawbacks to operations and vivisections.

For example, a disease which divides a nerve, or destroys a particular part of a nervous centre, sometimes does so not only with a completeness and abruptness of limitation which precisely imitates the operative experiment on an animal, but—both from the fact of its not traversing other tissues to gain access to these nervous structures, and from the comparative slowness of its production—avoids many of those disturbing elements which embarrass and destroy the success of an operation. Thus there is hardly a cerebral nerve respecting which we have not what might be made a complete collection of interesting cases, such as confirm and check, or even correct, the sometimes fallacious results of its division by the knife. And while the collection of similar evidence respecting the various segments of the spinal cord, and the nerves of the thoracic organs, has already thrown much light upon the physiology of these parts, it cannot be doubted that future research of the same kind will reveal much to confirm and extend our knowledge of the nervous arrangements of the abdominal organs: that we shall hereafter trace enteritis to lesions of the sympathetic nerve supplying the belly, just as for many years past pneumonia has been shown to be producible by disease or injury of the vagus, and its pulmonic plexus.

But it must be confessed, that these direct contributions of pathology to the details of the science of life are, on the whole, rare and infrequent. And hence, considering the wide range of physiological knowledge, and the extreme accuracy they demand in the medical observer, as well as (what is practically even more discouraging) the immense number of cases which must be constantly watched, it may be for years together, before the advent of the single fleeting opportunity which can alone render one of them directly fruitful to science—it is well to remember that there are other (and perhaps stronger) inducements to these toilsome and minute observations on the diseased and dead body. Even where they afford no new light, and bring no additional or independent evidence, to the physiological laws they enunciate, they supply what is often relatively of more value. For they illustrate rules which, without them, might be easily forgotten, and complete what would otherwise be deficiencies in the subject. Indeed, recollecting the practical object of the physiological studies of the Physician, it might almost be said that these phenomena of disease and death supply precisely that aspect of physiology which is most important for him to recognise, and least distinctly visible from any other point of view. How much light, for example, is thrown on the respiratory function by the various modifications of asphyxia incident to many diseases of the lungs? How much more information may be learnt from the gradual sequence of phenomena, and the number and variety of such cases, than could be obtained from casually witnessing the brief struggles of drowning or strangulation in any of the higher animals? And of similar, if not equal value, are the physiological observations really involved in the study of various other diseases:—as, for instance, in obstructions of the heart or great vessels; where, though we learn little that is absolutely new to Physiology, we are taught, by a process of analysis far surpassing what could be attempted in the existing state of our knowledge, the more constant and essential of those propositions which the science alone must as yet be content to lay before us with little attempt to discriminate their value. Indeed, it is impossible to doubt that the Physician who watches disease as a physiologist, is not only sure to stand on a vantage ground as regards his knowledge of pathology and therapeutics, but is likely to gain a simpler, truer, and broader insight into Physiology itself than is the mere amateur; who runs some risk of being captivated by its fascinating theories, or bewildered by the multiplicity of its details.

Chemistry, the next of the means of physiological research we shall notice, would perhaps be regarded by many as a twin science, rather than as a pursuit ancillary to physiology. And certainly the history of the last twenty-five years would go far to justify this opinion. Great as have been the advances of organic chemistry, and important as has been their reaction upon Physiology in general, the title chosen by Lehmann for his admirable Text-book illustrates the development of a new branch of the science; which, no longer satisfied with studying the mere products of life and organization, aims at an inquiry embracing the chemical properties of the tissues and secretions of the living animal. And though the perpetual flux of these substances opposes vast difficulties to exact research, and especially obscures all exact analysis, by adding to the chemical phenomena of the living body various degrees and kinds of the changes intermediate between life and decomposition, still the results of this inquiry have been enormous, and promise to be even greater. The more so, indeed, that a more practical, though less exact, mode of inquiry has lately been introduced; an inquiry which, by combining with the use of the microscope the application of various reagents, sometimes traces their influence on the minutest particles of the tissue inspected. Inexact in one sense such observations may be; since they ignore all estimation of quantity, and at present, overlook any but the simplest and most material reactions between the tissue and the substances applied to it. But considering what the microscope tells us—the infinite diversity of substances which enter into the composition of the most carefully cleaned fragment of muscle or bone—it is obvious that, in another sense, they claim a much greater exactness than the quantitative researches of the laboratory into such masses of compound tissues can ever hope to attain; and that, however simply they act, they often really differentiate ingredients, which a mere analysis would confound in inextricable confusion.

Lastly (what perhaps might have been more naturally alluded to before), *comparative anatomy* claims a large share in the advancement of modern physiology. Merging, it is true, into anatomy in general by a boundary almost imperceptible, still, in the strict sense of that comparison its name implies, it is scarcely less distinct from ordinary dissection and microscopical research, than is chemical investigation from the physical and mechanical processes it often involves. Indeed, there are good reasons for our bearing this fact in mind, and for our according to the mere process of comparison the prominence its importance assigns it. The details of this or that particular organization make up the materials upon which the comparative anatomist must work. But it is not always he who contributes more or fewer of these details who deserves such a title; it is the width of view, the depth of insight, the judgment in selecting, and the strictness of contrasting the proper details which together make up that constructive power, that "ἀρχειτεκτονικὴ φρόνησις," which the mind at once recognises as the ideal of a "comparative" anatomist.

But, even adopting this somewhat invidious distinction, and declining to regard the amount of materials collected for the comparative anatomist, as really representing the body of his science, it must yet be acknowledged that vast contributions have been received and elaborated since 1835. And while the philosophy and industry of Germany claim their usual large proportion of all that has been done to continue the grand conceptions of Oken and Goethe down to the more recent labours of Carus, Mueller, Wagner, Valentin, and a host of others, our own country may fairly take credit for no less great and honourable exertions. Indeed, what between the facilities afforded by our maritime position and commercial relations, and the genius of those who, with far inferior numbers, represent this department of British physiology, we might almost assert a pre-eminence. At any rate, whether the peculiar interpretation above given to comparative anatomy be accepted or no, we believe that the scientific men of all civilized nations would accord to England, as represented by Owen, that second place which (as in the case of Themistocles) might fairly be regarded as virtually deciding the first rank. And much of this universal estimation depends, we think, on the tacit recognition of what we have so imperfectly attempted to throw into words; the paramount value, in a science of which the very essence is comparison, of those very qualities of intellect necessary to such a task:—qualities which would be scarcely less prominent in the anatomist we have named, even if his whole elaborate theory of the archetype skeleton were absolutely disproved; and which claim scarcely less respect in the fearless and combative development they assume in the writings of Huxley.

Every one knows the old aspect with which the comparative anatomy of thirty years back was looked upon by Physiology. It was by turns a confirmation, a substitute, and a check, for other means of inquiry. A given organ of sense, for example, was found to be in excess or deficiency in some particular animal; and the function of a particular nerve could be traced in its correlative enlargement or decrease. Nature herself thus, as it were, dissected out the nerve; or conversely, removed it, with the avoidance of all the suffering—all the pain and inflammation—which would have followed and disturbed the parallel operation. Nay, further, even in far more recondite parts of the body, a similar pure and harmless variety of dissection or ablation could be thus witnessed; an advance of development in the particular organ being indicated by the evolution, from its textures, of parts unseen before; whilst its regress was indicated by the fusion or suppression of offshoots or complications, no longer requisite for the fewer and simpler offices it had to fulfil. Successive retrenchments of this kind, in proceeding down the animal scale, first reduced the organ to its more essential structures, and then, by a still increasing simplicity, altogether suppressed it, fusing it with others heretofore separate and distinct. And the parallelism of structure and function—both of which increased and diminished in complexity by steps essentially equal and consentaneous—this, the first and chief lesson taught by comparative anatomy, was also the principal guide to the application of its lessons to the study of physiological science. Many, however, as were those exceptions to this rule, which had long ago been foreshadowed by comparative anatomy, and have lately been brought into further view, they are far exceeded in importance by the newer and wider laws now opening out. The unity of the vertebrate plan, as developed by Owen, is a theory so complete and elaborate, as to invite the strictest examination and criticism before it is received in its totality into the body of the science. And it is probable enough that, from the number and minuteness of its details, it includes parts of very unequal accuracy, and deductions of doubtful value. But, whatever the exact details which are to be modified or sacrificed in the controversy this theory bids fair to provoke, it can hardly be questioned that the main propositions enunciated by Owen are correct:—that the limbs of different animals, for example, however diverse in their office, are but slight modifications of bones substantially the same; and that these bones, again, are but modifications of the type recognisable in an ordinary vertebra. And it is impossible not to recognise in such a theory the declaration of an unity of organization, where perhaps we might least have expected it; an unity which, whatever its relations to that section of animal life, to which it has confessedly no direct application, governs so large and important a division of this kingdom of Nature, during so vast a cycle of the world's history, as to suggest even more than it absolutely reveals, and to fulfil one of the highest purposes of the contemplation of Nature, by carrying the mind directly to its Maker.

Perhaps the very details and limits of such a law illustrate that diversity which it is another of the great results of modern comparative anatomy to have established. But this diversity is more obviously deducible from other sources. And nowhere do we trace it more distinctly than in the function of reproduction. Surpassing, indeed, is the interest of that chain of phenomena which Steenstrup, Van Beneden, Kuechenmeister, Stannius, and others, have established, as forming what is termed an "alternation of generations" in many of the lower animals. As an instance of the adaptation of means to an end—that end being the filling up, so to speak, of the very interstices and recesses of living organization with creatures adapted to such habitations; an economy which, at the great feast of nature, gathers up the smallest fragments, that nothing may be lost, —this modification of the reproductive process claims our highest admiration. But its wisdom and skill ought not to blind us to its strangeness; or rather, to the striking diversity it implies. We may range, for example, the whole vertebrate series, with the result of finding that reproduction, always exclusively bisexual, as constantly forms an embryo which undergoes a continuous development. But, in one of the Insect tribe, modern researches assure us that the generation of one sex can be effected without any impregnation from the fertilizing fluid of the male; the female embryo alone requiring this condition, elsewhere essential to the embryo of both sexes. In another, a nursing neuter encloses, and lives only to nourish, a number of similar neuters; the last of which series gives birth to the ordinary sexes, capable of generation. And, in the lower tribes of parasitic life we see, not only an hermaphrodism itself in directest contrast to the law of vertebrate reproduction, but a series of changes and migrations, conducting the embryo to its sexual development by a process which, in many instances, is so complex and obscure, as altogether to defy description in the present state of our knowledge.

Perhaps it ought to be added, that the epoch in question has revealed traces of scarcely less marked variations and diversities in other functions; even where, as in the case of secretion and digestion, evidence is necessarily less easy to verify or collect. The assimilation, for example, of many of the lowest forms of infusory or parasitic life (like that of the embryo generally), seems to be a process in which solution, conversion, and absorption, have to each other a relation of degree and kind very different to that witnessed in the digestive act of higher animals. Indeed, in the scanty waste of these simple organizations, the two first of these three processes seem often reduced to a minimum, which nothing but the imperfections inseparable from our observations, and the general laws of Physiology, forbid us from regarding as sometimes a *zero*. And in like manner, while we are quite justified in looking with extreme suspicion at the arbitrary and doubtful rules which, in some of the lower forms of life, occasionally decide this or that offshoot of the alimentary canal to be liver or pancreas—rules sometimes independent alike of the chemistry of the secretion, on the one hand, or the morphology of the secretory organ on the other; and guided mainly by the situation of its outflow into the intestine—so, in the complex structures of higher organizations, like some Insects, it is difficult to avoid the conjecture that we are beholding organs whose purpose, whether of secretion or excretion, has no representative in the Mammalian alimentary canal. These are indeed the *nebulæ* of our science; and we may confidently predict of them that, however increasing minuteness and fidelity of observation may clear up those now lying nearest to our search, it will leave plenty of scope for the discovery of other analogous difficulties beyond them, to be in their turn brought within the range of human reason.

With such instruments, then, has modern Physiology hewn its way. And if it be now asked, "With what results?" the following suggestions may afford some clue to a more systematic answer.

First and foremost, our attention is struck with the enormous flood of details that the last few years have poured forth. Details of every conceivable size, shape, and colour; round and angular; compatible and contradictory; struggling, confounding, often burying each other; the lightest often uppermost; the heaviest apt to gravitate towards the bottom. If facts be the wealth of science, truly an embarrassment of riches!

The influence on the individual of this, one of the chief scientific peculiarities of the present age, has already been alluded to in connexion with the microscope, which has supplied so large an accession of such details. But in passing on to consider its influence on the science, it seems pertinent to premise, that whatever the risk of such details absorbing too large a share of attention, and becoming objects, instead of means of study, they are far more than counterbalanced by the corresponding advantages which they alone conditionate. In a life of probation and training, risk is inseparable from opportunity; and the hazards injurious to one mind are the best discipline of another. And surely by so much as modern science not only accumulates materials for the exercise of judgment, comparison, and the higher faculties of the mind; but tends to lift these faculties out of the sphere of ordinary passion and prejudice, into the region of that light ("*lumen siccum*") to which Bacon compares it; by so much may it fairly be expected to have an opposite tendency to that ascribed to these details. Indeed, we are persuaded that the science of the age must in no small degree be credited with that advance in moderation and forbearance which (however we may sometimes doubt its sufficient prevalence) history shows to have been comparatively far more deficient in times scarcely remote from our own in anything but scientific culture.

As regards their influence on the growth of the science, we may first notice, that such details are valuable, not only from their number, and from the amount of information they supply, but from an import which is quite distinguishable from these characters. Mere discovery is so small a part of the process by which they are incorporated into the science, and mere addition so small a part of their value, that we might almost afford to disregard many of them altogether, were it not for the training and general discipline they supply. Requiring, for example, as they do, to be certified by the agreement of many observers, they form an excellent test of the completeness and accuracy of the means of observation at the disposal of Physiology in general. Then, again, they furnish the materials, and supply the hints, for those *questiones naturæ*, to which the more distinct and systematic progress of the science is chiefly due. They thus often do far more by what they provoke, than by what they themselves effect. Further, just as, in the law of probabilities, the concurrence of certain chances not merely adds, but multiplies, the total probability they form by their union; so with these details, the value of the inductions which rest upon them must be regarded as following precisely the same rule, and rising, as some high power of their mere number, with every new increment of facts.

Nor is the number of such details more striking than their value. Often of too great a magnitude, indeed, to rank as details at all—often, that is, rising to the importance of general facts—they rarely fail to reach a scale of usefulness only falling short of this in the circumstance, that they illustrate some principle not unknown before, or suggest some unexpected modification of it, rather than establish a new law or principle of the science.

And this brings us to a statement sometimes brought as a reproach against the Physiology of the day—namely, its deficiency in great discoveries; the failure of our elaborate means of inquiry to disclose wide laws, or brilliant and luminous theories, respecting the functions of animal life.

We will not stop to inquire whether the alleged deficiency may not in some degree depend on a redundancy, on the number of new truths which now distract the student, and are thus prevented from receiving that steady concentrated attention which each would claim singly from his notice. But, accepting this reproach without demur, we may at least point out that, even assuming its questionable accuracy, it is doubtful whether it does not in strictness involve more of praise than of blame.

Assuming, for example, that the last quarter of a century has brought forward few large and important discoveries; confessing (what is more accurate) that it has established few great and simple theories; nay more, that it has witnessed the utter demolition of many a structure of this kind, which has toppled over, not less by its own weight and bad construction, than by the withdrawal of the facts on which it was founded;

what are we to deduce? Or that the science has receded? Nay, that it has even stood still? That its apparent advance was deceptive, and is now awaiting fresh disappointments and retreats? Assuredly not. On the contrary, there is no better evidence of its progress.

In one point of view, indeed, the formation of theories marks an early stage of science, and their destruction a further stage of its progress. So far, that is, as such theories assume the systematic character, they incur all the dangers of that "too early reduction of knowledge to a system," the baneful influence of which was long ago pointed out by Bacon. And hence an epoch which discards, rather than develops, theories of this kind, is, by so much as it does so, in a state far more advanced, than one which theorizes on all points, and ignores that most difficult (but most necessary) part of human culture—the *un*learning of false knowledge, and the forgetting of false facts.

And the same rule holds good in a still more striking manner of those theories, more unassuming and useful, which disclaim the systematic character, and confess themselves, from the very first, to be imperfect and provisional attempts at generalization. Such views are the scaffolding of the edifice of science. And as the edifice gradually rises by means of the framework they afford, so the transfer and removal of the various parts of this scaffolding is the test and result of what they have done. Often, indeed, some portions of it are permanently incorporated with the edifice itself: to remain, it may be, as visible elements of its structure. But to whichever class they belong—whether they are demolished piecemeal, like the theory of *Phlogiston;* or preserved and adopted, like much of the old Hallerian doctrine of irritability—not only does their value remain essentially unaffected, but (like the living body of which Physiology teaches) their flux, change, decay, removal, substitution, are often not only incidental to their usefulness, but are the very conditions of their having any existence at all.

We therefore find the justification of a given theory, not in its absolute, but in its relative accuracy; not in its giving "the truth, the whole truth, and nothing but the truth," but a nearer approach to this desideratum than the views it claims to refute and displace; not, in one word, that it is true, but that it is useful—and that chiefly in the sense of its preparing for some view possessing a greater degree of this property, which view, once attained, will in its turn oust its predecessor. Or rather, to take a more exact and less invidious comparison, the new replaces the old, just as a dutiful son gradually steps into the place of a wise father, who feels his own work in life is done, and looks with excusable pride on the successor trained by himself to surpass him.

Further, it is in seeking to define the degree of usefulness which justifies a theory, that we light upon the best illustrations of its relation to the progress of knowledge—whether in any given science generally, or in any single student of that science. Taking, for the sake of convenience, the individual first; it may be remarked, that we are of such various mental organization, that some of us must theorize on any subject submitted to our notice. One man's memory is so bad, that he cannot keep a dozen facts together without a string of some kind (so to speak), which brings and keeps them in association or continuity. Another man's judgment is so impetuous, that for the life of him he cannot suspend his decision one moment on any topic started, certainly cannot wait to collect any considerable number of facts. Doubtless these are two extremes of intellectual malformation; belong, it may be, to minds essentially of very different value. But no one is free from some degree of either; few of us, perhaps, from a certain degree of both. And all of us must have noticed, that their joint amount varies, with an almost ludicrous exactness, in accordance with the slenderness of knowledge possessed by the individual. So that, to choose the nearest illustration, just as it is the half-educated medical man who theorizes most on a given case, so an ignorant patient can hardly mention one of his symptoms, without mixing with his description a theory of its production or its cause.

The closest parallel to this rule is to be found in the science which is, after all, only the collective knowledge of a number of individuals. Just in proportion as the details of any given branch of knowledge are numerous and complex, on the one hand; and its laws hidden, or obscure, or remote from our comprehension, on the other; do these provisional theories become not only justifiable, but necessary to its study. They may be inaccurate and even untrue. But their best defence is to be found in the single circumstance that they are indispensable. Indispensable, above all, for purposes of teaching; in other words, for communicating (whether orally or by book) that modicum of knowledge which is all that the most practised lecturer, or the most systematic handbook, can really be expected to impart to those beginning the study of the science. And just as it is the adept by whom these theories are best appraised, who wears them most lightly, and dismisses them most readily, as soon as the facts and ideas of his science are proffered a better clothing, so it is only by such an one that they can be, or ever are, at all dispensed with. In like manner, while it is the advancing knowledge of the individual which enables him gradually to disuse, and at last altogether to forego, these convenient but vague abstractions; so the same steps conduct the aggregate science to a precisely similar result. In the progress of the physiological student, the larger theories are continually being laid aside, to be replaced in his mind by successive sets of middle terms, each of which is smaller and more subdivided, but more accurate, than that which has preceded it. Or groups of details are gradually substituted for the vague, though comprehensive law of which he has learnt the inexactness and deficiency. And hence, though he rises in knowledge, he seems to descend in the scale of his ideas. But he only descends from the scaffold the better to raise the building; or, to adopt the French proverb, "*recule pour mieux sauter.*" And while the very tests we have suggested might be applied to illustrate and explain the degree in which various sciences demand or admit of such theories—why Geometry all but ignores them, Astronomy sometimes uses them, and Physiology always finds in them bad masters, but good, indeed invaluable servants—they certainly fit neatly enough into our more practical studies. They explain, for example, why, in Physic, every addition to physical diagnosis involves so much subtraction from hypothesis; every thorough investigation of any particular disease gives a death-blow to various speculations respecting it; and forms a source of those new complexities and details, which are the destined preparation for any higher knowledge. Nay, more, they are the touchstone for heresy and imposture; and, for instance, condemn Homœopathy, not because it is a rash hypothesis, or even a very large and untrue one,[*] but because it is behind the age, because it ignores the ascertained facts of Physiology and Therapeutics; and outrages the very laws which regulate the reactions of all matter.

Passing on to attempt a brief enumeration of some of the chief results which mark the progress of modern Physiology, we may begin with one which, though falling but just within the period to which the book that suggests these remarks limits our attention, is in many respects the greatest and most fruitful of them all. We allude to what is usually known as the *cell-theory;* a theory which, though at least prepared for by the observations of Müller, Valentin, and others, is deservedly associated with the names of its chief discoverers—Schleiden, and, as respects animal physiology more especially, Schwann.

The permanently cellular structure of many parts of plants had long been recognised, when Valentin, in the year 1835, pointed out the close resemblance of the primordial globules or cells of the vascular layer of the embryo to these vegetable cells. Schleiden soon after detailed the development of the vegetable cells in a very complete and elaborate manner. It was reserved for Schwann to show, nearly at the same time, that the casual resemblances noticed by Valentin, Müller, and others, between some embryonic tissues of the animal and those of the vegetable generally, were mere parts of a vast generalization; that there was[†] "one common principle of development for the elementary particles of all organized bodies: the two kingdoms of nature being intimately connected by this community in the laws of development of plants and animals." And that this principle consisted in the development of both from a peculiarly organized vesicle or cell, which was, in both animal and vegetable, the seat of special and similar forces, manifested by phenomena of nutrition and growth, which proceeded in both in the same or a similar manner. That especially the development and growth of the tissues of the animal was not, as hitherto supposed, by means of a secretion from the surface of its already organized tissues or from its vessels, but was essentially ascribable to the cells or the nascent or growing tissues themselves; cells often preceding the existence of all vessels, and always strictly extra-vascular.

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[*] The other aspects of homœopathy here evade notice; but we incline to think that it has done, or will do more good than harm, and bids fair to prevent quite as much imposture as it causes. Nor must we overlook the advantage of having a Limbo, like Milton's, in our medical Kosmos.

[†] Schwann: Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants. Sydenham Society's translation, by Mr. Henry Smith.

It is assuredly needless for a review like this to follow into details the magnificent outline thus sketched, or to recapitulate the elaborate proofs which have since been gradually added to the careful and numerous observations originally brought forward by its discoverer. No less the basis of general anatomy than the narrative of histological development, it remains to this day unquestionably the chief physiological discovery of the age, and a large part of the information which Physiology teaches. It will be the briefer and better course for us to sum up some of its leading subdivisions, with a view merely to indicate those parts of this theory which subsequent researches have indicated to be chiefly provisional; in short, to be erroneous, or questionable, or defective.

1. As respects the earlier stages of embryonic development, the theory still holds the greater part (if not almost all) of the ground it originally claimed to occupy. If we except some of the lowest and smallest forms of animal life, where the entire creature and the single cell may fairly be said to approach each other in size as well as structure, nascent existence seems invariably to affect the cell form. Nay, more, the stages by which many animals pass through various forms of independent life seem to be generally prepared for by a process of cell-growth, the simplicity and energy of which almost measure and express the rapidity and activity of those changes which adapt the animal to its new phase of existence.

2. In the growth, as well as in the morphology, of the more permanent tissues, it does, however, seem certain that the original cell-theory is by no means to be accepted without exceptions and qualifications, such as every year seems to discover in increasing numbers. For example, Huxley's excellent observations upon the structure and growth of cartilage, and especially of shell,* are, we venture to believe, daily receiving a more complete confirmation at the hands of histologists. And even the lucid descriptions and philosophic views of Carpenter have, we suspect, failed to detach many adherents from the cautious and profound accuracy of Bowman, as to the markings in the sarcous substance of the striped fibre representing a double cleavage, and not a system of cells. While there seems to be still less doubt of the observations of Kölliker, and the subordinate share they establish for the cell in the growth of the same fibre, during all but the earliest stages of its development.

3. Our views as to the proportions in which cell-growth constitutes an ingredient of the various secretions, have been remarkably changed since the first promulgation of this theory. Indeed, the last few years have conclusively shown, that the separation and removal of cells by desquamation form but a small fraction of the total secretion, even in organs like the skin, where such cells are aggregated in many layers, as a thick epithelium. While it is still more certain that the scanty and delicate cell-growth of glands and mucous membranes forms so small a part of their secretions, that it may quantitatively be almost disregarded.

And those details of the same kind to which we must, in philosophic strictness, apply the wise Scotch verdict, "not proven," are even more numerous: so much so that there is hardly a tissue in which it is not extremely desirable that its evolution from the cell-form should be traced out by a far more frequent or contiguous series of stages than have yet furnished the general evidence of identity between the parts of the tissue and those of the cell. Hardly a tissue, that is, in which we are not fain to fill up acknowledged deficiencies of this kind by analogies, comparisons, or even conjectures; perfectly justifiable, exceedingly useful, but falling very far short of actual observation and literal truth; nay, more, open in many important respects to considerable doubt.

On the other hand, however, it must in fairness be said, that by so much as the theory has depreciated as a permanent generalization, by so much has it commanded increased respect as a provisional one, inviting criticism and contradiction, and preparing future progress.

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* Article, Tegumentary Tissues.

Granting, for example, that in some parts of animals, growth and development are effected in all save their very earliest stages, by the extension of a substance perfectly structureless and cell-less; or by the deposition, in such a substance, of discrete masses of inorganic matter, ultimately assuming, by their mutual enlargement and pressure, shapes rendering them easily mistakeable for cells. Granting that, in many more, the steps of the metamorphosis from the cell into the tissue remain imperfectly known; or, so far as they are known, suggest doubts of the genuineness of the metamorphosis, or the specific share of each component part of the cell. Granting that, in all, neither physiologist nor chemist dare at present venture explicitly to assert that "organisms are nothing but the form under which certain substances crystallize." The cell-theory still remains; remains as a scientific *nexus*, though not as a natural and universal truth. Indeed, it is not to compliment its discoverers and supporters at all too highly to say, that its very error bids fair to do more to establish their insight than even its incontrovertible truth; that they were precisely most prescient where they had least of an actual basis of facts. Here and there, perhaps, they lost their footing. But the loss was the excuse and the condition for a kind of flight otherwise unattempted. And when they theorized thus uncertainly, it was not without knowing their deficiencies, and endeavouring, so far as might be, to avoid merely beating the air. Does this statement look like exaggeration? Let the reader who thinks so turn to Schwann's original Memoir, and compare what he there says about the relations of the crystal and the cell, with the remarkable details established (as we think) by Huxley (*loc. cit.*), or the still more striking (and not less original) paper by Mr. Rainey, in the "Original Contributions" of our Number for October, 1857. Or reverting to the nutritive antithesis between cells and vessels almost implied by Schwann, in alluding to that independence of the latter which is possessed and mediated by the former structures, let him consider the curious illustration of this fact afforded by bone, tendons, and even areolar tissue (Article —*Synovial Membranes*); in which, when exposed to a pressure such as would be obviously incompatible with the presence of vessels, capillaries and blood are alike replaced by myriads of cells. Or, lastly, let him ponder on the collateral support which the cell-theory now derives from the views independently deduced from pathological phenomena; especially that view as to the necessary intervention of the cell-form in all processes which form adventitious growths in the animal body; which, though figuring prominently in the lectures and writings of one of our few British teachers of Pathology ten years ago, has only just completed that cycle of export and import which our dear countrymen seem to regard as equally necessary for developing the excellences of Pathological doctrine and of Madeira wine.*

One point only can the writer of these remarks suggest, as altogether escaping notice in the various writings on the subject; and this, perhaps, from its simple and obvious character. It is, that contrasting the animal cell, whether as a nascent or permanent structure, with other tissues, we may trace a comparative uniformity (or limited range) in size; such as may perhaps be contrasted with that seen in vegetable cells, and certainly suggests that the forces of which it is the centre or agent are distinguishable from those of inorganic Nature, in the absence of all evidence of radiancy or action at a distance. In other words, the power of the cell seems to be only conditionated by absolute contact; or at any rate, to use the proper technical formula, it varies inversely as some very high power (far exceeding the square) of the mutual distance of the reactive particles immediately concerned. In this respect it resembles, but apparently transcends, Electricity and Magnetism.

In the function of *generation and development*, the vast advantages afforded by the microscope to the study of the minute details of this process, have been almost surpassed by those arising from the number and diversity of the modes in which the subject has been approached by different observers. And hence the steady and regular progress of our knowledge in reference to the evolution of the various organs in the higher animals, great as is its value, is less striking, in a retrospect like this, than the number of important doctrines which the last twenty-five years have either placed on a far more solid footing, or brought newly into view. Spontaneous generation has been as far disproved as the nature of the subject, and the physical relations of the particles concerned in such a theory, can permit. A variety of intermediate stages (as already alluded to) have been established in the lives of many of the lower animals. Many of these stages, again, have been distinguished as peculiar modifications (or rather exceptions) of the ordinary generative act. In other animals, still more marked exceptions to this process have been established. In the higher Mammalia, an original duplicity of sex, in so far as the possession of germs of both the essential organs—testicle and ovary—has been shown to prevail up to a certain epoch of embryonic life; and the subsequent relics of the suppressed organ have been detected at all periods of existence, in a way which permits a satisfactory theory of Hermaphrodism, if not of the phenomena which follow the removal of the testicles or ovaries in adult life. The import of menstruation, and the nature (if not the locality) of impregnation, are also essentially discoveries of this epoch. The whole development of the maternal organs during pregnancy has been traced out with a fidelity and minuteness unknown before. And even the cattle-breeder has contributed his quota to our knowledge, by giving us *data*, such as analogically entitle us to decide on the extreme limits of human gestation, or concur with our existing physiological information to explain the curious way in which Nature overrules a merely numerical transmission of parental influences to the offspring; or at any rate, often reflects the paternal influence, through the changes impressed by the progeny upon the maternal structures, into the succeeding offspring of the same mother by a different father.

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* The essentials of Virchow's doctrine of cellular pathology were taught, as the writer can testify, by Mr. Simon, in 1846; and some of them will be found in his Lectures, published in the Lancet for 1849, and reprinted as a separate volume in 1850.

In respect to our knowledge of the *nervous system*, the epoch which has elapsed since the year 1834 has introduced so many additions and modifications, that even to enumerate the more important, either in the order of their discovery or of their value, would claim more space than we can afford. At the beginning of this period, the discovery of sensitive and motor functions of the spinal nerves by Bell had remained in unquestioned supremacy for near a quarter of a century; certainly no way approached in magnitude or exactness by the doctrines with which Marshall Hall had just (1833) revived and extended the admirable researches of Whytt, Unzer, and Prochaska, and had thus inaugurated a controversy of *quasi*-theological bitterness, to which one would gladly avoid all further allusion. But as his "reflex function" forms one of the hinges on which turns much of the nervous Physiology of the epoch in question, it can scarcely be passed over in silence; though even now the most dispassionate criticism is not unlikely to give umbrage to some of his most zealous disciples, whose recipe for writing on the nervous system seems almost as simple as that quoted by George Primrose for talking on pictures.* Perhaps, however, one who so greatly admires his great abilities and acquirements, as to be unconscious of any bias save in his favour, may be permitted to point out how inexorably time is asserting (and has indeed already exercised) the functions of dispassionate criticism on this part of his (Marshall Hall's) writings. On the question of originality in any invidious sense we decline to enter. But while we believe that his zeal and energy, and singleness of purpose, did far more to extend and propagate a knowledge of what is known under the singular name of the "reflex function," than could have been accomplished by any other physiologist then extant, we feel bound to add, that the vigour and success of this his apostolic character were balanced by (perhaps involved) defects that will infallibly diminish the amount and duration of his claims to posthumous fame. That the most valuable part of what he propounded is to be found in the writings of his predecessors in this line of research above named, no Englishman with any pretensions to a knowledge of the literature of Physiology can now affect to doubt. And that much of the remainder would have been eagerly disclaimed, at the very time of its publication, by the more thoughtful of his cotemporaries, may be strongly suspected. But he awakened attention to neglected physiological truths; he showed their application to the phenomena of disease; he infected his cotemporaries with his own enthusiasm; and taught his professional brethren in what seems to have been evidently a more vivid and efficient style than was wielded by any one then living. And considering the high degree in which he possessed the above merits, so singular in their rarity, so manifold in their usefulness, it is really little more than a circuitous way of asserting the limitation of human faculties to add (what truth compels us to do), that he did *not* appear to possess some other merits, scarcely compatible with an extreme development of these; that his theories are often crude and arbitrary; his new facts few and scanty; above all, his observations and experiments devoid of that subtlety and exactness which alone fits them to be the agents of a specific inquiry or "questioning of Nature," and of which this Review has lately (July, 1859) laid before its readers so excellent an example in the researches in this department of Physiology by Brown-Séquard.

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* "The whole secret consisted in a strict adherence to two rules: the one, always to observe that the picture might have been better, if the painter had taken more pains; and the other, to *praise the works of Pietro Perugino*."—VICAR OF WAKEFIELD.

It was from a different quarter, and by a different mode of inquiry, that the physiology of the nervous system now began to make a more rapid and uniform progress. Anatomy, in the sense of ordinary dissection, had long been pursued with results increasingly valuable; and had thus arrived at descriptions which, like those of two English anatomists, Solly ('On the Brain,' 1836) and Grainger ('On the Spinal Cord,' 1837), might almost be regarded as exhausting this means of inquiry. The microscope, however, was now imported into the subject. And though the results of its use were somewhat slow of appearing in all their existing completeness of detail, an almost continuous and unbroken series of discoveries of this kind might be traced from the year 1835 up to the present time. Indeed, it is interesting to notice, that the unusual difficulties which the various nervous tissues offer to a satisfactory microscopic research, have rather favoured than opposed the accuracy of such observations, by the singular caution they have enforced. For instance, it was not till ten years after the establishment of the histological contrast between the ganglion-cell and the nerve-tube, that one after another, and with a continually decreasing hesitation, the various eminent men engaged in these microscopic researches throughout Europe suspected, conjectured, figured, and finally described the prolongations or *caudæ* by which the former structure became continuous with the latter. And though we own to an impression that the credit of this important discovery belongs chiefly to the country of Todd and Bowman, the most partial judge must allow that the race for priority was all but a dead heat, and that it is to Germany and its host of admirable observers, in the persons of Müller, Henle, Valentin, Remak, Volkmann, Wagner, Kölliker, and many others, that we mainly owe the first suggestions, and the greater part of those details advanced in the histology of the nervous system, which have been the chief condition of its recent physiological elucidation.

Much, however, as the microscope undoubtedly did towards this elucidation, it was scarcely less furthered by the other means of research, which soon added and combined their results with its simpler and clearer propositions. And to Dr. Todd's elaborate Essay (or rather Treatise) On the Nervous System (which, both in respect to its date and its position in the 'Cyclopædia of Anatomy,' is, to parody an Eastern phrase of laudation, the *navel* of the book) we must deliberately assign the high merit of giving so true and comprehensive a description of this most difficult subject, as to have been not merely a faithful picture of the then state of the subject, but a great advance on all that had been done, and a stepping-stone towards future progress. Incorporating (as it professedly does) the results obtained by many observers in very diverse fields of research, its thoughtful inductions, its steady, even tone of unprejudiced inquiry, and the originality of many of its most important propositions, are too easily overlooked by those unacquainted with the vastness and complexity of the details it sums up. And though, since its publication in 1847, much new light has been thrown on various points of its subject, it still remains so full and efficient a Text-book, as that the new materials since furnished (and even now, perhaps, scarcely incorporate with the body of the science), may best be arranged with reference to it as a *point de départ.*"

Of these new materials, those bearing on the effects produced by the electrical currents of the animal body when applied to the nerves, seem at present to claim but an indirect relation to the physiological phenomena of these tissues in the living body. They throw open a new field for inquiry. And their close relation to the phenomena of locomotion and innervation go far to establish an essential distinction between them and (for example) many of the optical appearances of the tissues, by suggesting that they subserve something more than incidental purposes in respect of both these functions. But hitherto the department of nervous investigation occupied by the admirable researches of Matteucci and Du Bois-Reymond can scarcely be said to have been extended far enough in the direction of the other branches of the study of innervation, to throw much light upon the physiology of the nervous system in general.

It is, however, in just such a harmony (or rather concurrence) of several modes of investigation as that hitherto deficient in the above interesting neuro-electric details, that we can best sum up and test the other novelties of nervous physiology. In these, the fact that the scalpel, the microscope, the healthy animal under experiment, and the subject of disease, afford evidence gradually and increasingly more confirmatory of each other, gives us an increasing certainty of advance in this all-important branch of Physiology. Stilling and Wallach, for example, began by a skilful process of preparation to make the microscope and the scalpel aid each other as they had never done before. But some of their results were doubtful; and few were in harmony with those of the dissectors of ten or twenty years before. Lockhart Clarke, Lenhossek, Schroeder Van der Kolk, and Owsjannikow, followed as original and independent labourers in the same field of research. And though they severally occupy different ground, and even when they discuss the same topics are not free from the contradictions and discrepancies incident to human observations and opinions, it can hardly be doubted, both that their agreements are of greater extent and importance than their differences, and that the progress of such research as a whole is towards a harmony rather than a discord of its results. So, too, with the startling novelties elaborately proved by Dr. Brown-Séquard. It is precisely because his physiological propositions point towards the anatomical details of these observers, precisely because, even in his observations and experiments themselves, the results of the natural and artificial lesion constantly check and confirm each other, that they promise so much for the future of this branch of Physiology. Models of what such researches should be, they seem to guarantee a progress of equal certainty and rapidity. Nay, more, we venture to predict that they will not only add to our knowledge, but that the information they impart will converge from its complex and numerous details into propositions the application of which will assuage much human suffering, perhaps save many a life.

In those functions of the special senses which imply so many peculiarities of innervation as almost to be capable of classification under this heading, the late advances of Physiology have departed from the ordinary rule of constituting a mere accumulation of new facts to others allied to them and already known, chiefly in reference to (1) a more intimate knowledge of microscopic structure on the one hand, and (2) a wider application of physics on the other.

Thus, for example, the elaborate results contributed by the various systematic histologists respecting the structure of the eye, the tongue, the nose, the ear (to which should be specially added the researches of Corti), have quite changed the whole description of the corresponding organs. And it is impossible to deny that much of this anatomical detail is at least pregnant with suggestions of a more strictly physiological character than its nature might seem to imply; that the mind instinctively appreciates a fitness of many of these minute structures for an office connected with the particular sense of which they are the ministers and agents. Without knowing the precise import of the complex arrangements seen in the structure of the retina, or in the wondrous rows of keys occupying the septum of the cochlea, or in the massy ciliated epithelium of the olfactory region, or even in the simpler structure of the papillæ of the tongue, we cannot help appreciating the general relation of each to its special organ; or at any rate to the medium of that stimulus which each is destined to receive in the shape of light, sound, vapour, and liquid respectively. True, this vague recognition of a general fitness is a very different thing from that specific proof of an exact function which alone would satisfy the claims of Physiology as a science. But we cannot doubt that the simple lesson of this kind now being learnt is a step—and a necessary one—in that direction; foreshadowing the more accurate theories which Anatomy alone can never construct, however essential it may be to the furnishing, as well as the selection and combination, of their materials.

The application of mathematics and physics to physiology is a subject far too wide to be even glanced over here; though the names of Wheatstone, Savart, Listing, and many others, suggest a variety of illustrations of its usefulness in inquiring into the functions of some of these organs. Perhaps their value is best evidenced in the eye; where they have shown something more than those probabilities or insufficiencies of existing theories, which, for example, a mathematical study of this kind generally leaves behind it. The simple but beautiful invention of the stereoscope is a singular illustration of what invention in the most conventional sense of the word can (and doubtless hereafter will) effect to lighten the study of Nature, by varying the channels of research. Not less curious, either, as a satisfactory proof of what some physiologists had long* suspected:—that in vision it was the brain, not the eye, that saw; and that it did so only by combining, as well as perceiving the images presented by the two recipient organs, and thus in reality seeing something distinct from both.

In the function of *locomotion*, again, the vast advances of modern physiology received their impulse, about twenty-five years ago, in those microscopic researches which, begun by Schwann and Wagner, were soon so greatly furthered by the well-known Essay of Mr. Bowman, in this country. Since then, the general anatomy of the whole of the locomotive structures of the higher animals has been almost exhausted by extensive and repeated investigations with the same instrument; by none more skilfully or successfully used for this purpose than by Kölliker, to whom we chiefly owe our knowledge of the cellular composition of the unstriped fibre, and many of the beautiful arrangements traceable in the tendons. In bone, teeth, and the various connective tissues, Anatomy has no less advanced our knowledge; though many of its details refer rather to the developement and nutrition of these structures, than to any more directly locomotive office. Nor must it be forgotten, how well the mere discovery of the anatomy of muscle has been seconded by further researches into its function; so that—even setting aside the elaborate researches of Du Bois-Reymond into what may, for aught we know, be properties collateral to that function—the whole phenomena of contraction, as it effects the shortening of the invisibly minute striped fibre, are now almost capable of being summed up in an accurate and systematic description. To the admirable observations of Bowman on the active contraction of fresh muscle under the microscope, have gradually been added so much information on its other relations during the act—its bulk, its sounds, its heat, and even its chemical changes, together with the various incidents which, in the living and dead body, suspend its powers, and stiffen its mass—that it may fairly be said, there are few departments of Physiology with which we have become so well acquainted; or which (an even better test of the extent of our knowledge) unite so harmoniously information obtained from many sources, and concur so well with what we know of other functions.

Two points alone baffle us; but these are closely connected with each other. Anatomically, we are still ignorant of the exact relations to these contractile masses of those terminations of the nervous apparatus, by which their contractions are called forth. And physiologically, we are still ignorant of the nature of this stimulus itself; especially of its relation (either of degree or kind) to those stimuli which provoke the impressions made upon other nerves. These stimuli, it is true, we can scarcely imagine to be as diverse as are those characters of matter which are severally recognised by the organs of special sense. And the phenomena of subjective sensations in these organs, confirm the contrasted results of irritating a motor and sensitive nerve, in indicating the possibility of their identity. But all of these resemblances, close as they undoubtedly are, still remain open to the conjecture, that they involve analogies of action rather than downright identities of the agent; and that not only must the stimulus which normally excites a muscular contraction be (as Brown-Séquard suggests from experi-

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*** Magendie, for example (Cours de la Physiologie, tom. i. p. 74. Paris, 1816).**

ment) more complex and powerful than that necessary to conditionate a sensation—but that the real stimulus which traverses or occupies the motor and sensitive nerves respectively may be quite *sui generis* for each.

In respect to the part played by organs akin to those of locomotion in various other functions, Physiology has advanced with a very different rapidity. In the mechanism of the *circulation*, for example, anatomy has conducted us to results not unlike those just claimed for it in muscular movement: the structure of the heart, the arteries, the capillaries, and the veins, being now known with a precision, which our forefathers would have envied, and which has certainly had an invaluable result on Physiology and Pathology generally. And two other features have greatly assisted this desirable object. The microscope has here been often brought to bear on the very processes of life; and has shown, not merely the dead and altered organ, but the organ alive and at work; the capillaries and smaller vessels actually circulating their contents under circumstances little, if at all, removed from those of health. And the characters of the larger vessels have suggested and favoured the application of physical and mathematical, as well as experimental, inquiries, so complete as almost to have completed our knowledge of the dynamics of the blood from this mechanical side. It is one of the few faults (or rather misfortunes) of the 'Cyclopædia of Anatomy,' that the brilliant researches of our French and German brethren in this department of Physiology have received no sufficient description in any of its later articles; and that an English student of the science has to this day no means of becoming fully acquainted with the elaborate (and yet thoroughly practical) results of this kind, which are connected with the names of Volkmann, Hering, Ludwig, Fick, and others.

The altered aspect of *respiration* in some respects repeats these considerations. Its mere descriptive anatomy necessarily but little changed, the few additions of this kind made to our knowledge are contrasted with much larger and more important modifications in the theory of the mechanism of the respiratory act. These modifications, often deduced from simple mechanical considerations, or from experiments scarcely less simple on the newly killed animal, must be allowed to possess the greatest value in a pathological point of view. Instigated in many cases by a desire to unravel some of the intricacies of thoracic disease, such researches have not only reacted greatly upon the physical diagnosis of lesions of these organs, but have already begun to explain much that concerns the function of other and distant organs, both in health and in disease. When a physician, by merely looking at a new patient, recognises long standing emphysema, or the too well known thorax of phthisis; or when his eye, piercing through the clothing that envelops the chest, singles out the diseased side or region with an accuracy that leaves little for subsequent auscultation to do, save to carry this diagnosis into detail; it would perhaps be as well for him not absolutely to ignore the Physiology of which his practical skill is in some sense the indirect result. And if such an one feels it his indefeasible duty publicly to warn his pupils against the uselessness and danger of physiological studies, at least let him qualify his wise and oracular denunciations of this, scarcely the besetting sin of modern Medicine, by some remarks which may hint that, without the advances won inch by inch by physiological observation and experiment, medical books on the "heart and lungs" would be almost as vulgar and purposeless as popular speeches on the "eyes and limbs." Much as we are indebted on these points to Valentin, Simon, and many other foreign physiologists, the accuracy and usefulness of the information afforded by the laborious inquiries of Hutchinson and Sibson entitle them to the thanks of all those whose daily labours in this field of disease (which we presume is called a "specialty," because it is the most general malady incident to our climate and race), are lightened by the simple and practical grouping now given to the details of this complex subject.

The means of research afforded by chemistry and the microscope have been even more influential in a physiological point of view. The researches of Rossignol, Rainey, Adriani, Kölliker, Moleschott, Van der Kolk, and others, on the minute structure of the lungs, have cleared up what even a few years ago was one of the most obscure and controverted subjects in the whole range of general anatomy. And how admirably these structural inquiries have been paralleled (perhaps considering their respective difficulties, we might say outdone) by the extraordinary experiments and analyses of Regnault and Reiset, Vierordt, Valentin, Brunner, Bidder and Schmidt, we cannot adequately specify here. Again, however, we have to notice one of those deficiencies incidental to the progress, if not to the plan, of this great work—which is on the whole far too meagre in its physiological chemistry. Were the absence of all record or summary of the researches just alluded to from the Articles THORAX and RESPIRATION compensated by their introduction, ever so cursorily, in NUTRITION, we might perhaps overlook what would only be a defective arrangement. But with every respect for the accomplished author of this last Article (in itself very peculiar), we must say, that however it may supply the wants of the reader, it certainly does not do justice to the subject, or to the elsewhere magnificent proportions of the book into which it seems to have casually dropped.

Nutrition, indeed, is daily assuming a wider aspect; not only from the number and magnitude of the details its older and more conventional meaning has gradually come to include, but especially from the new outlines it has acquired, by embracing what were formerly classified as functions distinct from it. Under the name of the interchange of matter (*Stoffwechsel*), for example, it has of late been sometimes made to include phenomena heretofore grouped as absorption, secretion, transpiration, &c.—in short, the whole series of processes and agents by which any given molecule of the living body is gradually formed and removed. And even limiting the word to that earlier meaning which conveniently restricts it to the mere maintenance of such a molecule in an imaginary uniformity of size, texture, and composition, the extraordinary progress lately made by physiological chemistry has greatly complicated, if not embarrassed, its import. To the details of the exact quantitative equation thus assumed to exist between molecular income and expenditure, it adds an inquiry into the qualitative changes also involved. While it certainly implies a variety of morphological details, not only in the blood as the chief agent or organ of the process, but also in every one of the manifold tissues placed in more or less direct dependence upon this all-important fluid.

In a mere mention of some of the chief acquisitions of this department of modern Physiology, classification is of such secondary importance that we may follow the most obvious arrangement, which first traces the new materials added to the system forwards to the blood and the tissues, before conversely indicating the direction and destiny of those parts of these tissues which, after a longer or shorter intermediate cycle, finally leave the body as excretions.

Beginning with *digestion*, the greater part of our existing knowledge of the dental structures dates from less than twenty-five years back; when the researches of Retzius led the way to those of Nasmyth, Owen, Czermak, Todd and Bowman, Kölliker, and Tomes. Our knowledge of the development of the teeth, save in a few unimportant and doubtful details, dates from a similarly recent epoch; indeed, if we except the early observations of Arnold, and those of Schwann and Goodsir, the present aspect of the subject belongs almost exclusively to the more modern of the above histologists.

Throughout the whole of the digestive canal we recognise the same astonishing advance in general anatomy; starting, too, with an equally curious exactness from the commencement of that quarter of a century which is just concluded. Thus, as regards the mucous membranes of this canal, nearly fifty years had elapsed since Lieberkühn's discoveries, and nearly thrice as long a time had succeeded those of Brunner, Grew, and Peyer, when the observations of Boehm in 1835, Sprott Boyd in 1836, soon followed by those of Purkinje, Wasmann, Bischoff, and many others, gradually opened up the complex structures thus brought into notice. With no sudden or great discoveries, with little, indeed, sufficiently marked to be permanently associated with the names of any particular observers (excepting, perhaps, the classical description, and the beautiful illustrations, by which Todd and Bowman almost exhausted the details of the lingual mucous membrane), the anatomy of the whole of this intestinal coat has at length received a full elucidation. And the comparatively simpler structures of the muscular and peritoneal coats, though unknown thirty years ago, had even anticipated this successful inquiry.

The physiology of these structures has experienced an analogous, though hitherto less complete and exhaustive progress; of which the results, however incomplete, evince in a striking degree the value of researches undertaken by diverse processes and many observers. It is true, that in respect both to the mere analysis, and to the active properties of the saliva and gastric juice, we can scarcely be said to know much more than the details of general propositions already recognised before 1835 by Berzelius, Treviranus, Leuchs, Beaumont, and others. But besides that we now follow out into specific facts the often vague and controverted statements of twenty-five years back, the elaborate researches by which these statements are deduced have a value almost independent of their results, since they establish many of those collateral conditions and circumstances of action which, in the living body, are scarcely less influential than the immediate agents themselves. Contrast, for example, the statement of Leuchs, that saliva converts starch into sugar, with the deductions which must now be made from the admirable series of comparative experiments and observations by Bidder and Schmidt on the metamorphosis of this kind effected by saliva and a variety of other secretions and substances; and it will at once appear, how much the question of this change being really a function of the saliva, depends on collateral inquiries as to the rapidity and energy, the time and place, the fresh or foul state, under which a change, in itself so easily producible by many kinds of organic matter, seems to be effected by the secretion of a special living structure. Nor can we avoid suspecting, that it is to similar qualifications that we must look for what may turn out to be cognate answers to the questions, whether it is really the office of the liver to generate sugar, and of the pancreas to dissolve the albuminous compounds of the food.[*]

That the brilliant researches of Bidder and Schmidt just alluded to, aided and suggested as they have been by those of many other observers, only less exact, elaborate, and systematic, should still remain in any degree unsuccessful, may seem a disheartening fact for modern Physiology to confess. With Blondlot, Frerichs, Lehmann, Claude Bernard, and many of our best English authorities in this department of the science, still at issue as to what they see under given circumstances, as well as to what they deduce from such observations: with the functions of the liver, the pancreas, the intestinal juice, the solitary and agminate follicles, still variously and even contradictorily interpreted,—we cannot claim for the last twenty-five years the merit of any complete victory over these obscure and important objects of physiological investigation. But while many of these discrepancies are evidently due to the peculiar relations of the several organs in question—relations no less obstructive of analysis than of experiment, by the impurities and injuries with which they threaten these two processes respectively—many more no less obviously depend on the variety of processes by which different observers have carried on their inquiries. And that this variety is itself an augury and guarantee for ultimate success, is sufficiently indicated by the analogous discrepancies it has already cleared up. The recrementitious, but not indispensable, value of the bile; the absorption of fatty matters by the lacteals of the villi; the mutable function of the solitary and agminate follicles, which, like the tonsils, seem to dehisce or not, as their local exigencies may require; the emulsive action of the pancreatic secretion:—these, and a variety of similar propositions, resting at present upon probabilities very different in degree and kind, show how bright a light has lately been thrown on questions which, twenty years ago, either baffled all inquiry, or were only answered with the most ludicrous[†], as well as rash, conjectures, instead of those sound generalizations, or scarce less, sound provisional theories, which are the results of modern physiological research.

Nor can we overlook the vast importance to Physiology of those general nutritional relations which the quantitative results obtained by Bidder and Schmidt assign to some of the large organs appended to the alimentary canal. The great cycle of intermediate circulation thus established to exist, pouring out large masses of organic and aqueous materials from the blood into the canal, to introduce them again into the vessels, changed or combined by the metamorphoses and admixtures they have meanwhile undergone, is one of the chief discoveries of modern Physiology; not the less truly such, though its data and itself were many years before quite within reach of the speculations which, in so many other subjects, have preceded the deductions won by thoughtful and toilsome researches like these.

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[*] Doubts which are suggested concerning these two organs by the recent observations of Dr. Pavy and Dr. Brinton respectively.

[†] For example, the ingenious (rather than philosophic) theory that the agminate follicles secreted the flatus expelled *per anum*: a theory which it would be invidious, as well as unnecessary, to refer to its respected author.

Intestinal absorption, again, has been widely affected during this epoch of modern physiological inquiries. Already shown by the experiments of Magendie, Segalas, Delille, Brodie, and others, to be in great part effected by the bloodvessels, subsequent inquiry has mainly resulted in applying this conclusion still more specifically to the phenomena of digestion. In this way, not merely has the absorption of the lacteals been gradually almost restricted to the taking up of fatty matter, by a process of minute division and endosmose, in which the exact shares of the intestinal and other structures still remain somewhat obscure and indefinite; but the vascular part of the absorptive act, without being released from all share in the reception of fatty matters, has been shown to take so vast a predominance in the absorption of nourishment as a whole, that the terms "chyme" and "chyle" have quite lost their previous meaning: the former being now known to be the residuum, and not the product, of gastric digestion; the latter a small part, instead of the whole, of that product itself.

The physiology of the *blood* itself has perhaps undergone fewer large modifications than that of most of the other organs of the body. Nevertheless, it would be difficult to select any subject, the present description of which would be in more striking contrast with that it received twenty-five years ago. The mere structural details are chiefly modified by the careful studies lately given to the corpuscles; especially by quantitative researches on the white and red corpuscles in the higher Mammalia, and by the aid furnished by comparative anatomy to the morphology, and even the import of both. But it is to physiological chemistry that we owe the most numerous and important accessions to our information. And among these, analysis has not only distinguished the proximate and ultimate components of the corpuscles and of the *liquor sanguinis*, and striven, with tolerable success, to push the inquiry to an indirect and minute comparison of all the details of each, but has especially attempted the more difficult task of detecting, in the latter fluid, the materials introduced into it from the various organs which it washes with its current. A large and increasing class of substances has been thus formed; midway between the tissues whose effete products enter the blood, and the excretory organs whose office it is to purify this fluid by removing such compounds. And the formation of this class of substances implies another distinction in the action of excretory organs themselves, such as was scarcely foreshadowed by the Physiology of a few years ago:—the distinction between those which do not, and those which do, construct or combine the principles they withdraw from the blood; between those which, as it were, merely filter out a pre-existent compound, like the urea removed by the kidneys from the blood; and those which form the main elements of their secretion, by a slower and more recondite chemical process, such as, to all appearance, constructs in the liver certain important ingredients of the bile.

Among other questions connected with circulation and secretion, few are more interesting than that of the afflux which, on physiological rather than mechanical grounds, was formerly assumed to constitute so remarkable an aid to the mechanism of the circulation. At a time when the physical details of this process were little understood, it was generally thought necessary to assume that there was an attraction of blood towards the capillaries, a *vis à fronte*, as it was termed, a force which was the result of the nutritive changes (especially the exsudation) going on here. Contradicted, however, if not absolutely disproved, by the experiments and calculations of Volkmann and others, as well as by the consideration that such a force would (like that of the respiratory movement) impede in the veins to exactly the same degree as it would assist in the arteries; and certainly no way established by the singularly defective proofs sometimes[*] brought forward; the question of any definite attraction of this kind, with a special or exclusive influence on the arterial blood, must be regarded as still *sub judice.* The recent observations of Ludwig on secretion, have, however, supplied what seems to be a satisfactory proof of a force of this kind, by showing that the fluid which occupies the duct of a gland undergoes, on its stimulation (as by the galvanizing of its nerves), an enormous increase of the pressure measured by a hæmadynamometer in its calibre; an increase which so greatly transcends the simultaneous pressure of the column of blood in the main artery distributed to the gland, as to be evidently in great part independent of intra-vascular changes. Even in this specific and intermittent withdrawal of fluid from the blood, however, it is difficult to see any but an indirect and irregular help to the arterial current; and it is equally difficult to avoid connecting it with that old rule, "*ubi stimulus ibi affluxus,*" to which, after all, we must at present be content to refer so much of the obscure mechanism of inflammation.

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[*] For example, the circulation of the blood in a fœtus devoid of a heart has been held by one author as almost tantamount to an *experimentum crucis*. But it need hardly be said that, unless the mother were equally devoid of this central propulsive organ, the circulation in the child could not have been thrown exclusively on the *vis à fronte*. Indeed, Physiology supplies observations of the cardiac pressure being largely transmitted through smaller and more resistant media than those of the placenta (as in the ureters or veins). And pathology shows what large variations in the force of the current of the blood are compatible with a certain degree of nutrition of tissues.

Time fails to allude to many other of the steps by which this department of the science has recently advanced. The processes of growth, especially as regards their morphology, and the wide changes of the whole structure they often (as in bone) involve; the chemistry of starvation, as elucidated by Bidder and Schmidt; the large nutritional questions to which the investigation of this process of unopposed waste has helped to afford an answer; and the elaborate general inquiries by which Chossat, Regnault and Reiset, Valentin, and others had prepared for the more specific observations of the two first named observers; the structure of the various ductless glands; the chemistry of the urine; these, and many other topics, every one of which might be made the subject of a special description, all conduct us to the same conclusion with which we began. The harvest is ripe. Already, indeed, a crowd of labourers are gathering it in. Each, however, unknowingly, assists all the others. Each, indeed, can do something. If he cannot reap, he can bind the sheaves, or help to store them up. And unless the cloud of war still threatening modern Europe burst in some deluge that reduces all science to a standstill, we may fairly expect that, twenty-five years hence, the Physiology of that day will have advanced over a far larger extent of ground than that of which we have now attempted so hasty and imperfect a retrospect.

Hitherto we have said little about the work itself, the name of which stands at the head of this Article. The lapse of time, and the progress of science, these are the topics towards which we have diverged, far out of the beaten track of ordinary reviewing. But while such a procedure is fully justified, both by the numerous notices which its various Essays have from time to time received in the pages of this Review, and by the sheer impossibility of making the most cursory attempt adequately to discuss them within the limits accorded to this Article, it would be unjust, alike to the public, the publishers, and the Editor, to forego all award of that praise or blame which it is the especial office of the critic to make. As regards the Editor, if the reader, after pondering over the graceful preface by which the work is really completed, thinks that gentleman requires any further excuses for the delay and interruptions which have impeded its progress, we recommend him to seek such excuses in the articles of which Dr. Todd is himself the author, and in the equally well-known work for which he is jointly responsible with Mr. Bowman. The delays in the publication of this Cyclopædia have, indeed, been one chief element of its value; since they have not only made it a history of the advance of the science, but have in many instances been a necessary condition of those researches which its Essays have often specifically published, and have almost always incorporated. And not only does the latter part of the work bring its contents up to the latest date of our knowledge, but the number and grouping of the articles have permitted some of the more recent to modify and complete the unavoidable deficiencies of the earlier ones on the same or kindred subjects—a kind of super-fœtation, or rather alternation of generations, which, whatever it may detract from the unity of the work, seems to us to have been rightly favoured by the Editor. For just as it is one of the chief advantages of a Cyclopædia to be elastic and unfettered in its growth, so its first requirement is goodness of details rather than any mere symmetry or concord, such as the number and diversity of its authors would alone suffice to render impossible of attainment.

The reader of the work will therefore do well to remember the unequal development and value of its earlier and latter portions. And he will scarcely fail to find that among so many authors and articles there are some of very unequal merit. While we have already hinted at some incidental deficiencies which seem to have escaped the compensations—especially in Physics and in physiological Chemistry—above alluded to as having been in other cases supplied by more recent articles.

On the other hand, it must be owned that these, its defects, are far outweighed by its merits, which may be stated as follows—That it brings together a larger amount of information, is more carefully constructed, and more profusely illustrated, than any similar Dictionary or Cyclopædia hitherto published. That most of its materials are to a great extent inaccessible even to the scientific public, and many of its parts (we allude especially to its beautiful Essays on comparative anatomy) are still practically otherwise unpublished. That others of its articles are the choicest productions or studies of their several authors; who wrote little but what they had verified, and cared still less for that dangerous smoothness which sometimes beguiles the reader into a fancied knowledge of the subject, and thus makes the fortune of the book at his expense. Lastly, that most of them are singularly free from the vice of book-making: so much so, that the learning and historical research many of them involve would be little suspected by any one unacquainted with the subjects, or accustomed only to that style of writing which often gains the erudite character—a kind of literary bird's-nesting, which rejects the contents of the eggs, but always carefully strings their shells.

But not even the torrid atmosphere of the present summer is any excuse for undue warmth, or for general censure, especially where pleasanter duties are equally within our choice. We may fairly congratulate the Editor and his subordinate authors on the completion of their task. Not less may we congratulate our own Profession, that in its ranks, toiling reputably by daily practice for daily bread, are to be found so many who have shown themselves well qualified to further, and to teach, a science so intricate and so collateral to their practical duties. And while we may certainly feel proud that researches so toilsome and costly as many incorporated in this work should have been undertaken and carried out (like the whole efforts of British science) without any help from the Government or the public, and by the unaided efforts of individuals themselves, we cannot but express some obligations to the eminent firm by whom this truly national work has been finally completed and set before the public. Indeed, we will go further, and add that they are strictly entitled not only to gratitude, but to a speedy sale of the whole impression of the Book. It is true that the profession is not a rich one; that our Gideon Grays must think twice before they spend six pounds, even though it give them the hundreds of monographs, and thousands of large pages and goodly engravings, contained in these six volumes. And hence, if they are unwillingly compelled to withstand the temptation of getting a whole library of Physiology in one book, or to postpone it to the Greek Kalends, in the shape of Mr. Gladstone's promised abolition of the Income-tax, we can scarcely find fault. But there is a large (and therefore remunerative) class of persons who are, we humbly conceive, bound forthwith to buy, and even to read, this work; and on whom a little persuasion, or even threatening, may not be quite thrown away. It is notorious that an awful "*cacoethes scribendi*" has seized our profession; and threatens to surcharge all our Journals and Reviews with a literature not always entitled to criticism. Everybody is so anxious to teach, to write, and to publish, that listeners, readers, and even critics (the mildest and most indulgent of men), are becoming impatient. The construction of the edifice of science is somewhat impeded by a new confusion of tongues: in which few men can hear, and still fewer understand, their neighbours, because so many are talking at once. And (what is really the unkindest cut of all) few of what we may call our minor medical Essayists are satisfied, unless they can drag into their practical deductions some abstract physiological speculations, often of a very loose or doubtful character. Others, again, distress us with elaborate re-discoveries of anatomical and physiological details known long ago, or with still more elaborate corrections of the casual or unavoidable omissions of some brief or condensed text-book of the science. Many of these amusing authors disport themselves in a region where we must not attempt to reach them. Thus, the amiable and considerate writer who a few months ago blandly suggested castration as a remedy for phthisis, claimed the judicial notice of our Surgical colleague, for a sentence, let us hope, *not* analogous to that of Phalaris. And the American author who lately deduced the infectiousness of tetanus from what seems to a common mind a case of *over*-poisoning of a bullock with strychnia by some negroes actuated by a natural (but indiscreet) yearning after fresh meat, might be fitly left to the criticism of our Toxicological friends. But writers of Physiological Essays will, it is hoped, take it as a friendly hint if we suggest that, before rushing into print with new and brilliant discoveries, they look into the pages of what we must, for years to come, regard as the text-book of British Physiology; or at any rate, if they choose to teach, before they have learnt, the subject, to recollect that these volumes may at any moment supply a bitter and unsparing (because true and impassive) criticism. With the utterance of this mysterious warning and strong inducement to a large class of apparently wealthy writers, we feel that we complete our duty alike to them and to the publishers; who are quite entitled to expect a large and rapid sale for what is as incomparably the best, as it certainly is the cheapest, scientific work of the day—a work indispensable to the Physiologist, and scarcely less so to the Physician.

Review VIII.

First Annual Report of the General Board of Commissioners in Lunacy for Scotland. Presented to both Houses of Parliament by command of Her Majesty.

There is always some danger when there is an acknowledged abuse to remedy, that the authorities put in motion to accomplish the object will set about their task with more zeal than discretion, and that while they are directing their attention to the magnitude of the evils which have called for their interference, they will overlook the reasonable claims to consideration to which those are entitled who, though themselves parties to a most objectionable state of things, cannot fairly be held responsible for them, but must rather be regarded as accidentally committed to a vicious system, from which it would have been very difficult for them to have escaped, and which it may be charity to suppose they never saw in its true light.

Considering, then, the deplorable condition of a large proportion of the insane in Scotland, as disclosed by the Report of the Royal Commissioners, it is very satisfactory to find that the General Board of Commissioners in Lunacy for Scotland, appointed under the 20th and 21st Vict. cap. 71, have approached the subject with singular freedom from prejudice, and have more than justified their appointment by their first report, admirable alike for its tone and temper, for the valuable information it affords, for the evidence of the patient industry which has been brought to bear upon a very arduous and difficult task, and for the dispassionate and candid manner in which all the facts have been weighed.

Nothing is so calculated to ensure the hearty co-operation of those whose proceedings it will be their duty to supervise, as that feeling of confidence in their discretion and fairness which is inspired by the first Report of the Scotch Commissioners, and which we doubt not will characterize their future proceedings. The results of good government will always be found in the sincere devotion of those who are governed, and we feel sure that the spirit in which these Commissioners have inaugurated their reign is calculated materially to lessen the difficulties which are inseparable from the establishment of a new *régime.*

One of the first difficulties which the Commissioners encountered was the correct reading of the Act under which their powers were granted. This, in fact, affords another illustration of the almost impossibility experienced by legislators in making laws so to word them that they are sure in the hands of candid administrators to carry out their own views and intentions. The value attaching to expressions and words so frequently admits of question, that we can scarcely be surprised if in dealing with insanity (a subject about which even the best informed know so little) our lawgivers should fail in the attempt to convey their precise meaning, and that further legislation should be required to define the powers intended to be given to the Commissioners.

In the words of the Report,—

> "The chief objects of the statute are to provide for the building of district asylums for the reception of pauper lunatics, and to insure the proper care and treatment of lunatics generally, whether placed in asylums or left in private houses under the care of relatives or strangers."

It appears here to be assumed that all the world is agreed as to what is "a lunatic," and that consequently no doubt can arise as to the persons who may be properly subjected to this care and treatment; but lest there should be any question in particular cases, an attempt is made to define, for the purposes of the Act, the condition of mind implied by the term "lunatic," and, as usual with all such attempts, the matter is left in as much uncertainty as ever. The Commissioners make the following observations on the statutory definition of lunacy.

> "The word 'lunatic,' the Statute declares, 'shall mean and include any mad or furious or fatuous person, or person so diseased or affected in mind as to render him unfit in the opinion of competent medical persons to be at large, either as regards his own personal safety and conduct, or the safety of the persons and property of others or of the public.' The question here arises, whether the second part of the definition is simply explanatory of the first part, or whether it is an amplification of the definition; whether, namely, every mad, fatuous, or furious person is *simpliciter* a lunatic, or whether to be so accounted he must *also* be unfit to be at large, as regards his own safety and conduct, or the safety and property of the public. It may further be considered doubtful whether it is contemplated that a person, in order to be declared lunatic, must be unfit to be at large as regards *both* his own safety and conduct, or as regards *both* the safety and property of the public, or whether the definition will be fulfilled if he be unfit to be at large, as regards *either* his own safety or conduct, or as regards *either* the safety or property of the public. In practice, the view has generally been adopted, that every person certified to be of unsound mind is, in the statutory sense, a lunatic; but the Board of Supervision appear to be of opinion that no pauper of unsound mind can be considered a lunatic in terms of the Statute, unless there is also reason to apprehend danger. In accordance with this view, it has on various occasions been maintained not only by parochial boards, but also by sheriffs, that fatuous or idiotic paupers, although totally incapable from mental deficiency of acting for themselves, are not lunatics in terms of the Act. The question, therefore, is one of great practical importance, and its early adjustment is extremely desirable."

We think it would have been much more to the purpose if the Act had either constituted some independent authority to determine doubtful cases, or had given to the Commissioners absolute power to declare within a certain time after the patient had been placed under control, whether or not it was a proper case for such control. It is impossible, from the infinite peculiarities of human nature, to define what is natural and what is unnatural in an individual. All the circumstances of constitution, temperament, disposition, habits, occupation, means, health, &c. &c., must be carefully weighed in each case, before a correct judgment can be formed as to the mental condition of any person; it is therefore utterly useless to set up any standard of sanity by which all can be equally measured, and equally useless to attempt a legal definition of that which must necessarily often be a matter of opinion. Natural sympathy for the insane has engendered a feeling of suspicion and distrust towards all persons, whether medical or otherwise, who have to deal with them; and we regret to see unworthy motives unhesitatingly attributed to any medical man who, in the ordinary exercise of his profession, signs a certificate of insanity; whilst those to whose care a patient is entrusted are frequently from this fact only, and without any other ground, denounced as mercenary and dishonourable, without any higher aim than their own pecuniary benefit, regardless alike of the rights and welfare of their unfortunate patients, and of their own character and reputation. One might suppose that the existence of insanity was discredited, and that the mass of the people regarded it as a delusion attempted to be palmed upon them by those who were professionally engaged in its treatment; that, in fact, if there were really such a malady, it was only present when furious maniacal excitement existed, and that when a patient was capable of conducting a rational conversation he could not be insane. He may be mistaken as to facts; he may make an error in judgment, he may reason incorrectly, he may have firm belief in the existence of that of which others see no evidence; but still, it is argued, this does not constitute insanity; and as to extravagant and violent conduct, he may be eccentric, and, under the provocation of interference may give way to a naturally irritable and hasty temper—but still the lawyers will say triumphantly, this is not madness.

But who is to decide? Doctors are suspected, commissioners are ignored, and the duty of determining upon what shall be done in a doubtful case devolves upon whoever is rash enough to take the responsibility on himself, and to risk the consequences of a contrary opinion on the part of a jury who, without any experience whatever, are alone recognised by the law as competent to judge. It seems to us that it would be quite as rational to submit to the decision of a non-professional jury, guided by the evidence of non-professional persons, a question as to the existence of disease in a patient's lungs, liver, or kidneys, at some period antecedent to the inquiry, as to suppose that juries as at present constituted can properly judge of the symptoms of mental unsoundness said to have existed weeks or months previously. There is perhaps no question which more requires the nice discrimination of skilled observers, and none in which such assistance is so entirely disregarded. Can it be, then, that we have been all along deceived as to the existence and nature of this malady, and that the overgrown and costly receptacles for the insane which during the last thirty years have been springing up in every county, and adding so grievously to the burdens of the ratepayers, are in fact as unnecessary as they are oppressive? Notwithstanding the indisposition to recognise insanity, especially in its early stages and mildest forms, we are disposed to believe that even English juries would agree that the thousands who people our asylums are really of unsound mind, and need protection and care. If, then, so large a proportion of the population is really insane, it is surely the first duty of the Legislature to constitute some independent and competent authority to determine what persons can properly and legally be detained under care and treatment. The interests of the insane themselves, not less than of society, require that the responsibility of their detention should not devolve upon their friends, who are thereby frequently deterred from taking those prompt measures upon which the issue of the case so often depends, and so the chance of recovery is lost. If it is right that an insane person should be controlled, there is no reason why the odium of placing him under control should rest upon his relatives; society is interested in his protection and recovery, and the State may therefore very properly be charged with the duty of determining by its proper officers in what cases it is necessary to interpose its authority.

The Act 20 and 21 Vict., cap. 71, provides for the appointment of district medical inspectors; but it appears that no such appointments have yet been made; the Commissioners, however, in speaking of them, say, "We have repeatedly had occasion to feel the want of their services, especially in the case of lunatics whose removal to an asylum appeared to us to be desirable, but in regard to whom a different opinion was strenuously maintained by the parochial authorities." To such officers as these district medical inspectors might very properly be delegated the duty of signing all certificates of insanity, one provisional certificate of any legally qualified medical practitioner being in the interim sufficient authority to take such steps as the urgency of the case required. We have already said that in the interests of society, and of the patients themselves, it is most important that the responsibility of confining or detaining them should not rest with their relatives, neither should it devolve on those who have an interest in continuing their detention; and as it seems that that impracticable animal, "the public," is not satisfied with the protection of two medical certificates, by all means give more, and provide a sufficient number of well-paid and responsible officers to examine every patient before admission into an asylum. In Scotland, the duty of granting an order is performed by the sheriff; but he, not being a medical man, occasionally refuses to sanction the confinement of a patient, because he cannot see with the eye of an experienced practitioner. The Commissioners quote several cases where the sheriff's order was refused, and after remarking,

> "That the best hope of recovery lies in early treatment, and therefore that it is of the greatest consequence to the welfare of a patient that his reception into an asylum should as much as possible be facilitated:"

They say:

> "We are clearly of opinion, that less evil is likely to result from the sheriff accepting as proof of insanity the certificates to this effect of two qualified medical men, and attaching comparatively little weight to the facts quoted by them as evidence of its existence: than from his refusing to attach any value to these certificates, and founding his decisions on the statements of facts which reach him at second hand, from the very men whom personal examination convinced of the existence of mental aberration. A medical man may, from the manner, appearance, and conduct of a patient, be thoroughly convinced of his insanity, and may nevertheless fail, by any statements of facts, to convey the same conviction to another person; and this difficulty will be greatest in the incipient stages of the malady, at the very time when asylum treatment is calculated to be most beneficial."

The necessity that exists for the appointment of independent and competent judges in all cases of insanity, is shown by the fact, set forth in this Report, that the Commissioners have been unable to obtain from ordinary medical practitioners the necessary certificates for the removal of patients to asylums, who, in their judgment, ought to be placed there under care and treatment. Surely the Commissioners should have authority in such cases, if not, some other power should be constituted which should insure something like uniformity of action. It is lamentable to think that the imaginary protection given to lunatics should operate in opposing an insuperable barrier to the proper treatment of their malady at its most critical period, by sacrificing the most valuable time in attempts to reconcile conflicting opinions and overcome the prejudices of those in whose hands the law has left the fate of the patient. We quote from the Report the following remarks:

> "The cases in which the objection of medical men to grant certificates of insanity acted in preventing us from improving the condition of pauper lunatics, were not very numerous. Still, from the important consequences involved, we think it right to advert to them. We have pointed out that, by the Statute, all pauper lunatics shall be sent to the asylum of the district in which the parish of the pauper is situated, unless the Board agree to their disposal otherwise. But the statutory form of the medical certificate of insanity required to place a patient in an asylum, includes an expression of opinion that he is a proper person to be detained under care and treatment; and, accordingly, some medical men, while admitting a patient to be of unsound mind, have refused to certify that he was a proper person to be detained under care and treatment, when the question of sending him to an asylum was also involved. The practical result of such refusal is to deprive the Board of all power to compel improvement in the condition of a patient; and, in this way, a pauper lunatic, for whose care we are legally responsible, if not certified 'to be a proper person to be detained under care and treatment,' is practically removed from our jurisdiction, without being placed under that of the Board of Supervision, whose authority, in matters of treatment, is now limited to ordinary paupers.
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> "Occasionally, also, medical men have refused to grant certificates in the cases of patients suffering under certain forms of insanity, on the ground that they do not come within the scope of the Act.
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> "From the vague and unsatisfactory nature of the definition of lunacy, we have in several instances had no alternative but, with regret, to yield our own views to those expressed by the local medical men, and to leave the patient in circumstances which we considered unsuitable."

The efforts of legislation appear to be directed rather to prevent the undue detention of patients in asylums, than to insure the fitness of all who are admitted, but it appears to us that the latter object is of far greater importance than the former. There is a certain amount of suspicion attaching to those who have been patients in an asylum, and therefore it is of the greatest importance that care should be taken not to place any person under such a disability unnecessarily; but this question having been settled, it is of far greater consequence that his *perfect* recovery should be established than that his discharge should be incautiously hastened; a few weeks' longer treatment may make a certain cure of a doubtful recovery; whereas premature interference, and the knowledge on the part of the patient that it may be exercised at any moment if he is only sufficiently pertinacious, will have a tendency to keep up the excitement of uncertainty in his mind, and may, if it result in his discharge before his medical adviser thinks it prudent, bring about a relapse.

A minute table is given exhibiting the distribution of inmates within the cognizance of the Scotch Commissioners. We gather from this that in a population of 2,888,742 inhabitants, there are 9474 pauper and 1450 private lunatics.

Having ascertained as nearly as they could the number of insane persons in Scotland, the next duty of the Commissioners was to see that proper provision was made for them in suitable asylums or otherwise; they say:

> "We do not conceal from ourselves the practical difficulties which lie in the way of determining with accuracy the number of insane at large who should be placed in asylums. The conclusions at which we arrived were not altogether based on the nature or curability of the malady, but were influenced also by the circumstances in which the patient was placed, and the degree of care bestowed upon him. We asked ourselves whether, in the interests of the patient himself or in those of society, it seemed most desirable to place him in an asylum or to leave him at home, and our decision was taken upon a general consideration of all the facts of each case. For in addition to the mental and bodily condition of the patient, as well as the general circumstances by which he was surrounded, we felt bound also to take into account the constitution of our asylums; and we were conscious that our difficulties would often have been materially lessened had these establishments been based upon the idea of providing a diversity of accommodation for patients affected with different degrees of mental incapacity. There are many persons, for example, whose mental condition requires that they should be placed under the care and control of others, yet whom we would hesitate to deprive of liberty to the extent almost necessarily involved in sending them to lunatic asylums as at present constituted."

We cannot but commend the care and judgment manifested here in deciding a difficult question, and we are disposed to think that the Commissioners have hit the right nail on the head in pointing out the want of a diversity of accommodation for patients afflicted with different degrees of mental incapacity. The report says, beyond all question transference to an asylum is very often calculated to prove most beneficial to an insane patient; but the extent to which asylums have contributed to diminish insanity is not so easily determined, because doubtless a certain number of these recoveries would have taken place out of asylums, under judicious treatment. The following observations appear to us worthy of the most attentive consideration:

> "It cannot be too often repeated, that in the treatment of insanity loss of time is unfavourable to recovery, and that every impediment that is thrown in the way of immediate treatment acts most prejudicially upon the patient by tending to render permanent the aberration from normal action, which, under favourable circumstances, would speedily have subsided. We are therefore of opinion, that asylums are capable of rendering to humanity far greater services than they have yet achieved. There cannot, however, be the smallest doubt that these establishments have, even in times past, proved of great public utility, by undertaking the treatment and management of patients requiring special medical care, and of those whom, from violence or other peculiarity, it is found dangerous or impossible to retain in private houses. Moreover, it has been clearly proved that the discipline of an asylum exercises a most beneficial and curative influence upon many patients who, if left at home, would probably have become confirmed lunatics, and is calculated to ameliorate in a very remarkable manner the condition even of the most intractable incurable cases. It is very certain, then, that asylums prove of the greatest service both to the patients and the public; and therefore the question to be considered is, not whether their extension is required, but whether, as at present constituted, they fulfil all the expectations which led to their erection, and which the expense of their maintenance might warrant us in entertaining. Beyond all other aims, an asylum should have for its object the cure of the insane and the diminution of insanity. Now, in relation to this malady, two important facts have been clearly established,

> first, that one chief cause of the affection is hereditary predisposition; and secondly, that the success of curative treatment depends in a very great degree upon its being undertaken at an early stage of the disease. In the course of our investigations, we have obtained abundant proof that fatuous female paupers frequently become the mothers of illegitimate children, who, in their turn, grow up imbeciles, or become lunatics; and although there is naturally more difficulty in tracing the source of idiotcy or insanity to a paternal origin, there can be little doubt that male fatuous paupers contribute to this evil. In illustration of these remarks, we shall here give the result of our investigations in one county into this painful aspect of insanity. The number of single patients visited or reported on amounted to 349. Of these, 33 were reported to the visiting Commissioners as illegitimate, 22 being registered paupers, and the remaining 11 indigent private cases. Of the 349, 113 were females above 17 years of age. Of these, 22 were in circumstances affording adequate protection to their chastity. Of the remaining 91, 15 were known to have given birth to illegitimate children, and 5 to have borne more than one child. Of the 15 mothers, 3 are known to have been illegitimate, and 12 are at present paupers; of their children, 6 are known to be idiots. There are, besides, in the county, 3 other idiots who are known to be the offspring of insane or imbecile mothers, who are dead or have disappeared. These facts are most deplorable; nevertheless, it would be esteemed a harsh measure to send all such cases to asylums, and yet society has a right to demand that all persons who are supported on charitable funds should be placed in such circumstances, and under such control, as will guard against the propagation of this social evil. This result, we are of opinion, might be obtained by attaching to asylums adjunct houses, in which such patients, and others of analogous character, could be placed, without to the same extent depriving them of liberty as the patients in the asylum proper. And we are further of opinion, that many of the objections at present entertained, both by the friends of such patients and the public generally, in regard to placing them in asylums, would be obviated by the proposed modification of these establishments. Moreover, experience shows that there is frequently great unwillingness on the part of relatives to send to asylums patients who are suffering from the milder and incipient forms of insanity. Yet these are precisely the cases in which removal from the home circle is most likely to exercise a beneficial influence."

One, perhaps, of the most important considerations attending the absolute liberty of the insane, is the certainty that a considerable number will propagate the malady by multiplying the species; and the facts mentioned in the foregoing quotation, shocking as they are to contemplate, only represent one phase of this fearful evil. It is pretty certain that the offspring of patients whose minds at the time of conception are unsound, will be liable as they grow up, to become insane themselves; and we are disposed to attribute a great deal of the increase of insanity to this circumstance, that no precautions are taken to guard against this danger, and indeed no regard is paid to this consideration in deciding whether a patient, recovered up to a certain point, should be liberated unconditionally. Undoubtedly, as the Commissioners say, "society has a right to demand that all persons who are supported on charitable funds should be placed in such circumstances, and under such control, as will guard against the propagation of this social evil." We would claim for society an extended right as affecting all persons, without regard to means, who at the same time that they are distinctly insane, are also in a position to propagate this social evil. The diminution of insanity is quite as important in the middle and upper classes as in the lower grades; and we think that some restrictions should equally be placed on them, considering that it is a question involving the degeneration of our race. At any rate, it would be well that the matter should receive more consideration than it has hitherto done, especially when deciding upon the unconditional liberation of a patient whose perfect recovery is doubtful.

The manner in which the expenses of maintaining pauper lunatics should be defrayed has engaged the attention of those connected with asylums in England; and the Commissioners have found that it has an important bearing on the welfare of the insane poor in Scotland. They say:

> "There is no doubt that the tendency to regard a pauper lunatic as not a proper person to be detained in an asylum is strongly fostered by the smaller expenditure necessary for the maintenance of the patient at home. Under the present law, each parish supports directly the burden of its poor; and, accordingly, the increased expenditure which an asylum generally involves, by exercising a direct effect upon the parochial rates, affords a strong inducement to delay, or altogether to dispense with, the removal of patients. We are decidedly of opinion that it would greatly conduce to the benefit of the insane poor if the rate for their main-

> tenance in asylums were levied on the whole district, or even on the whole country, instead of on the individual parishes to which the patients belong; and we think that it would not be difficult to guard against the reception or detention of cases which might, with more propriety, be detained in private houses, by leaving it to the district medical inspector to determine, with the right of appeal by parishes to the Board, whether or not the case was one which should be refused admission. There can, we think, be no doubt that the present system, by throwing impediments in the way of early treatment, tends to convert into confirmed insanity many cases of excitement or depression, which, under prompt attention, would have proved of mere temporary duration. If the maintenance of patients, when detained at home, fell directly upon the parishes, but became district burden when they were placed in asylums, it is very certain that many of the objections now brought forward against the removal of pauper lunatics would cease to be urged. Should, however, the proposal here made appear too sweeping in its nature, an intermediate course might be taken, by which a fixed and moderate sum should be charged to the parishes, and the balance be defrayed by a rate levied on the district."

The Commissioners believe that considerable success would attend the cottage system of accommodation as an adjunct to the district asylums. It is very gratifying to know that in their experience kind and humane treatment is extensively found in cottages, even under the present system of imperfect supervision, and they have reason to think from what they have observed, that under the immediate superintendance of asylum officers, it could be so fostered in growth as to open up a prospect of escape from the many questions which are every year rendering the care and management of the insane poor a problem of more difficult solution. They go on to observe:

> "In every country of Europe the question of the accommodation of the insane is daily becoming more and more embarrassing, and we see how, in England, notwithstanding the wealth of the country and the humane spirit of the people and of the legislature, the increase in the number of lunatics keeps ahead of all the exertions made for their accommodation. This is a grave fact which deserves our most serious consideration before we commit ourselves to the building of asylums, in the expectation that no further call will be made upon us. No doubt it is theoretically easy to maintain the doctrine that asylum accommodation should be provided for all the insane poor, and that no expense should be spared in supplying the wants of this afflicted class. But the sane poor have also their claims, and the question may be asked, How far is it right that an idiot or a lunatic in a state of dementia or general paralysis, who is beyond all hope of being restored to sanity, and who, moreover, is little able to appreciate kindness or to derive pleasure from the care and attention bestowed upon him, should receive treatment greatly superior to that bestowed upon an aged or infirm ordinary pauper, who though in a sense also incurable, is more capable of appreciating kindness and showing gratitude in return? In England, the poor-house is open to the able-bodied labourer, but in Scotland it is reserved for the aged and helpless poor, and accordingly with us there is not perhaps the same reason for drawing a distinction between the treatment of ordinary paupers and that of incurable pauper lunatics. But there will always be this essential difference between the two classes calling for special consideration in their treatment, that the latter are labouring under a degree of mental incapacity which renders them altogether dependent upon the care of others, and incapable of appealing against harshness or neglect. Still, as we must place a limit on our charitable expenditure, we should beware of making such a distinction in their treatment as might raise a doubt as to its propriety, and must therefore take care not to be too lavish with the one hand, lest we be forced to be too penurious with the other. On this account we lean towards any scheme that will embrace good and economical accommodation for the whole insane poor, rather than to one which, from the expense of carrying it out, will sooner or later be of only partial application."

It is very satisfactory to find the Commissioners able to report that the condition of the insane in Scotland has been already considerably ameliorated since the Report of the Royal Commissioners. Mechanical restraint has been almost entirely abolished, both in public and private asylums, having been resorted to in one or two instances only, in which there appeared to the Commissioners to be good grounds for its application, whilst in no case that came under their observation or notice was seclusion improperly applied or injuriously extended.

The Report is accompanied by various appendices and statistical tables, including correspondence and evidence upon which the Commissioners found some of their opinions and conclusions, and shows the result of the treatment pursued in the various asylums of Scotland. For these and other matters we refer our readers to the document itself, which will well repay the attentive perusal of all who are interested in the welfare of the insane, and merits and receives our highest commendation.

PART SECOND.

Bibliographical Record.

Art. I.—*The Psychology of Shakspeare.* By John Charles Bucknill, M.D. Lond., Licentiate of the Royal College of Physicians; Fellow of University College, London; Medical Superintendent of the Devon County Lunatic Asylum; editor of 'The Journal of Mental Science,' and joint-author of 'The Manual of Psychological Medicine.'—*London*, 1859. pp. 264.

That a physician devoted to the treatment of "the mind diseased," but especially one who has proved himself to be so intimately acquainted with the physiological and pathological psychology as Dr. Bucknill has, should find in Shakspeare much subject for study and contemplation, is not to be wondered at. What but the reality of his characters, their vitality and truthfulness, the absence of mere histrionic conventionalities, has made Shakspeare the poet of the world? He, indeed, as our author well says, is the great mind which, containing all possibilities within itself, and combining the knowledge of others with the knowledge of self, was able to conceive and to delineate every variety of character possible in nature. As Shakspeare's representations of the normal manifestations of character surpass the representations of all other authors of fiction, so do we nowhere find the transition from the healthy to the morbid condition of the mind, the various phases of insanity and the difference between real and feigned madness, delineated as we see it in the plays of our immortal poet. Opportunities, our author tells us, were plentiful for observing the phenomena of mental derangement in the good old days. The insane members of society were not in those times confined in lunatic asylums or placed under the protection of Commissioners in Lunacy. "If their symptoms were prominent and dangerous, they were indeed thrust out of sight very harshly and effectually, but if their liberty was in any degree tolerable, it was tolerated, and they were permitted to live in the family circle or to wander the country." Hence, Shakspeare had not to seek far for subjects of study, and his great mind sufficed "to convert these opportunities into psychological science."

Dr. Bucknill shows how Shakspeare's knowledge of the mental physiology of human life was brought to bear upon all the obscurities and intricacies of its pathology; how he, above all men, had the faculty of unravelling the motives of human action. In the work before us the author analyses, in a masterly manner, the mental phenomena of several of the more prominent characters of the world's poet, and brings ample proofs how, even in her morbid manifestations, Shakspeare mirrors Nature as none else has done. It has been a source of great pleasure to us to read Dr. Bucknill's book, and we hope soon again to peruse it, when, under his guidance, we have gone over carefully the dramas upon which he descants. We congratulate the literary world upon the appearance of a work which must be a great boon to all ordinary commentators, to whom the human mind is only known in a few of its features. But Dr. Bucknill opens to us new views; he gives us additional grounds for paying our homage, we had almost said devotion, to the creative genius of Shakspeare; and his inquiries are not vague and vain speculations, but put before us in language worthy of its subject, arguments based upon a kind of knowledge which no previous commentator of Shakspeare has possessed. The series commences with an inquiry into the characters of Macbeth and Lady Macbeth; its general scope may be gathered from the following introductory remark—

> "Although *Macbeth* is less pervaded with the idea of mental disease than its great rival tragedies of *Hamlet* and *Lear*, and contains but one short scene in which a phase of insanity is actually represented, it is not only replete with passages of deep psychological interest, but in the mental development of the bloody-handed hero and of his terrible mate, it affords a study scarcely less instructive than the wild and passionate madness of Lear, or the metaphysical motive-weighing melancholy of the Prince of Denmark."

Dr. Bucknill commences his analysis by demonstrating that the moral basis of Macbeth's character is by no means one of innate badness, but that his natural tendencies are to bravery and kindness. His ambition is the idol which, under the fostering influence of the baneful prophecy, and of the still more ambitious wife, leads him to the commission of his first foul deed, and having once overstepped the bounds of morality and humanity, his career in wickedness can no longer be arrested. Let us hear our author's words—

> "Macbeth is no villain in grain, like Richard III. or Iago, revelling in the devil's work because he likes it; but a once noble nature, struggling, but yielding, in a net of temptation, whose meshes are wound round him by the visible hand of the Spirit of Evil. Slave as he is to that soldier's passion, the love of fame and power, he is not without amiable qualities. He was once loved even by his arch-enemy Macduff, to whom Malcolm says—
>
> "'This tyrant, whose sole name blisters our tongues,
> Was once thought honest; you have loved him well.'"

Dr. Bucknill urges further arguments on the same side, but he has no wish in that way to palliate the guilt of Macbeth—

> "In a moral point of view this is impossible. If his solicitings to crime are supernatural, combined with fate and metaphysic aid, he is not blinded by them. With conscience fully awake, with eyes open to the foul nature of his double treachery, although resisting, he yields to temptation. He even feels that he is not called upon to act to fulfil the decrees of destiny—
>
> "'If chance will have me king, why chance may crown me,
> Without my stir.'
>
> "Had he with more determination resisted the temptations of the woman, he might have falsified the prophecies of the fiend, and put aside from his lips the poisoned chalice of remorse, maintained from rancours the vessel of his peace, and above all, have rescued the eternal jewel of his soul."

It is impossible for us to accompany Dr. Bucknill in his wanderings through those realms which Shakspeare has opened to us, but which, like so many other good and glorious things that lie within our reach, we but rarely appreciate and love as they merit. We may vaguely admire his works, as a man of taste feels his heart warmed by the picturesque scenery of the mountains and valleys of Wales or Scotland; but deeper knowledge and a larger mind than that which indulges only in dilettantism, is necessary to enjoy to the full, the grandeur or loveliness of nature; and alike, those wonderful representations of man in all the phases in which he treads the earth, which Shakspeare places before us.

Willingly would we dwell longer upon this interesting and fruitful theme. We feel that were we able to devote more space to it, we could but feebly follow in the footsteps of Dr. Bucknill, whose knowledge and appreciation of Shakspeare's characters make him a fit expositor of his favourite poet. No one, we conceive, has so admirably and consistently traced the unity of character that pervades *Hamlet*—the most wonderful, as well as the most intricate of Shakspeare's dramas. Sincerely do we hope that Dr. Bucknill will continue his sketches, and more particularly that he will at once carry out the plan which he has already formed, of giving to the world his views regarding Shakspeare's knowledge of medicine.

Art. II.—*Five Essays.* By John Kearsley Mitchell, M.D., late Professor of Practice of Medicine in Jefferson Medical College of Philadelphia, Member of the Academy of Natural Sciences of Philadelphia, Fellow of the Philadelphia College of Physicians, &c. Edited by S. Weir Mitchell, M.D., Lecturer on Physiology in the Philadelphia Association for Medical Instruction.—*Philadelphia*, 1859. pp. 371.

The five essays which constitute this volume have all—four *in toto*, one in extract—been already before the public of the United States; some of our readers may therefore have become acquainted with them. They now, however, for the first time make their appearance in a collected form, and we may thank the editor for having performed what the 'Charlestown Medical Journal' terms an act of filial reverence in publishing them together. The book itself gives us no clue to the relationship that links the two names together which are found on the title-page.

The first of these essays discusses the cryptogamous origin of malarious and epidemic fevers, in which the author presents us with "a theory, not to be esteemed devoutly true, but as, in the present state of knowledge, the most perfect explanation of the known phenomena of the case; and as the least exposed to the many objections easily brought against any other hypothesis."

This passage conveys the drift of the writer's argument, and the conclusion he arrives at. He brings together much evidence, necessarily altogether of a circumstantial and indirect kind, but to the author's mind incontrovertible. The essay is worth reading, because it is elaborated with much care and thought, though it fails to convince us that the presence of a fungus in the system is the essential and uniform cause of all malarious and epidemic fevers. Suffice it to say that the conclusions of Dr. Mitchell necessarily depend entirely upon the mode in which he views certain facts capable of various interpretations. He gives no positive observations showing the introduction or germination of fungi in or upon the body that are not already familiar to all educated medical men. As a sample of the urgency with which Dr. Mitchell has subpœnaed the witnesses in behalf of his cause, take the following:

> "Immemorially, the sleeping in damp sheets has been thought hazardous to health; but the keepers of hotels and boarding-houses know that the danger is very slight, unless the sheets have been put away in a damp state, and have acquired a *mouldy* smell. The constant practice of the hydropathists shows the little hazard of a wet sheet, while daily experience demonstrates the certainty of at least stiffened muscles and an arrest of the Schneiderian secretions, after spending an hour or two between damp and musty bedclothes. The Scottish Highlanders are said to dip themselves, dress and all, into the sea, when obliged to sleep out of doors, after being drenched by rain. As water is supposed to act unfavourably by means of its coldness, we cannot easily explain the known benefit of this substitution, except by a reference to the acknowledged power of salt to prevent the growth of fungi."

And again—may we be pardoned calling the following, in slang metaphor, a regular sneezer?

> "It may seem rather curiously nice to notice another point connected with this part of our subject; but as you are all students now, and will I hope become true scholars hereafter, I will observe, that every one who searches for knowledge among old books and manuscripts has been occasionally attacked by sternutation, and at least a temporary coryza, when he has

disturbed the dust which has long slumbered within their leaves. As the dust of a room swept daily and the pulverulent clouds of a summer road do not so affect him, he seizes his microscope, and detects the cause of his sufferings in the numerous organic spores which have grown into power to torment among the dampness and darkness of the leafy envelopes."

These passages are merely selected to show upon how weak a basis the whole hypothesis must rest to require support from such evidence. The whole is, however, an interesting contribution to medical literature, and may serve as a fresh starting-point for further inquiries in the same direction.

The second essay, upon Animal Magnetism or Vital Induction, is in every way more satisfactory and complete than the first. The author no longer deals with hypothesis only, but gives us evidence based upon an intimate personal knowledge of the subject; his mode of handling it proves his entire competency to give instruction thereon. The author has experimented upon a very large number of persons, and appears to have investigated, with much discrimination, the proceedings of the various mesmerists with whom he has come into contact. The following are some of the conclusions which he arrives at:

> "Imagination and imitation cannot account for the uniformity of the phenomena of the mesmeric state in persons of all ages and conditions, who are totally ignorant not only of the symptoms to be produced, but of the design of the mesmeriser. . . . .
>
> "The phenomena of artificial somnambulism are,—1. An exaltation of the circulation, without a corresponding increase of the respiration. 2. An obtunded sensibility to causes of pain, and sometimes, though rarely, its total obliteration. 3. The more or less complete obliviousness of the thoughts and events of the mesmeric state, while awake, although the memory of the events of the natural state is strong in the artificial state. 4. The retention of locomotion, and the facility of being led into suggested dreams, are also curious effects of mesmeric action."

To this property of artificial dreaming the author refers the alleged miracles of clairvoyance, intuition, and prevision. He explains the influence of phrenological manipulation in bringing out the manifestations of qualities as by the keys of a piano, upon the same principle of suggestions being acted upon by the patient. The cases brought forward to prove this, positively and negatively, are both instructive and amusing. We need not state that Dr. Mitchell utterly scouts the idea of a mesmerised subject being able to treat disease by virtue of his mesmeric intuition. In these cases we invariably find, that while the pathology or diagnosis is faulty and absurd, the remedies suggested are always exactly in keeping with the previous knowledge of the mesmerised individual. The author has found no difference in the susceptibility of the two sexes to the mesmeric influence; he believes the *rapport* so much spoken of as existing between patients and their mesmerisers to be entirely dependent upon the will of the patient, and to be one of the many hallucinations of the mesmeric state. He therefore entirely denies the existence of any peculiar sympathy between the operator and the subject. The mesmeric influence itself Dr. Mitchell regards as

> "The effect of what the natural philosophers call induction. The will of the operator acts solely on himself, his altered system reacts by proximity on the subject of the experiment by an unexplained power, analogous to the equally inexplicable induction of the mechanician and the presence of the chemist."

Dr. Mitchell does not claim much therapeutic power for mesmerism, while he admits that it is capable of producing frightful disorders, both of body and mind, if improperly employed.

The third and fourth essays treat of the penetrativeness of fluids and gases, while the fifth is devoted to the consideration of "a new practice in acute and chronic rheumatism." The last, we are told in the preface, affected the treatment of rheumatism throughout the United States, and is still a favourite mode of practice in this formidable malady with Dr. Mitchell's countrymen. The treatment is based upon the hypothesis that the disease is dependent upon an affection of the spinal cord, and consists in the application of cups, leeches, or counter-irritation along the spine; the point of application being determined by the origin of the spinal nerves of the affected part.

Art. III.—*On the Treatment of Internal Aneurism by the Method of Valsalva.* By Thos. Brady, M.B., T.C.D., Fellow of the King and Queen's College of Physicians, Professor of Medical Jurisprudence, Physician to Cork-street Hospital, &c.—*Dublin*, 1859. pp. 21.

The object of this paper is to show that the profession have been misled by modern authors into the belief that what has been called Valsalva's treatment of internal aneurism originated with that physician. Corvisart, Pelletan, Dupuytren, and a host of other distinguished names may be quoted among French writers; Hodgson, Hope, Watson, and a similar array, suggest themselves among English authorities as having described Valsalva's method to consist in bleedings, very frequently repeated, accompanied by a gradual reduction of food to a minimum capable of sustaining life. Dr. Brady, on examining Albertini's account of Valsalva, his master's, procedure (Valsalva not having published anything himself), finds that so far from the *coup sur coup* system of bleeding attributed to him, not more than one or two blood-lettings were prescribed at the very onset of the treatment, and that the subsequent treatment was limited to complete rest in bed, enemata, and abstinence from wine, with so "much food and drink, weighed and measured, as would be sufficient to support life; and this not divided into two portions daily, but into three or even four, so that entering the bloodvessels in small quantities it should not in the slightest degree distend them."

The author of the paper before us observes that the real method of Valsalva and Albertini was not that which has been praised as theirs by some moderns, but that those great physicians would have rejected the profuse bleeding system as dangerous and irrational. As a contribution to the history of medicine, no less than as a record of the vagaries of authors, and the blind faith with which they follow one another's statements, Dr. Brady's paper merits perusal.

Art. IV.—*A Guide to the Food Collection in the South Kensington Museum.* By Edwin Lankester, M.D., F.R.S., Superintendent of the Animal Product and Food Collections.—*London*, 1859. pp. 95.

Most of our readers have doubtless watched with interest the large humanitarian spirit which manifests itself in the South Kensington Museum. They will have perceived how much is being done there to bring many things that have hitherto appeared abstruse and technical within the comprehension of the masses, and that in that region at least neither science nor art any longer appears to adopt as their motto the arrogant

"Odi profanum vulgus et arceo."

As an instance of the tendencies that animate the authorities of the great educational establishment to which we have alluded, we would adduce the present guide, which we received from the pen of Dr. Lankester. As a lecturer, this gentleman has already most eloquently discoursed, at the South Kensington Museum, during the past summer, on the subject of food and beverages, and the numbers and behaviour of the audience amply testified to the interest the subject awakened among a class of the community not professionally acquainted with these matters. The various illustrative specimens and drawings to which the lecturer drew attention, are still to be found in the museum; and it is with a view to enable the visitor to view them with an intelligent eye, that the present little work has been compiled. It is "an introduction to the general principles and plan" upon which the food collection has been arranged. Thus, after a statement of the constituents that go to form the bones, the author gives a classification of food. He establishes two great classes: I. Alimentary or necessary food; this he divides into, A. Mineral: 1, water, salt, ashes of plants and animals: B. Carbonaceous or respiratory, heat-giving; 1, starch; 2, sugar; 3, fat: C. Nitrogenous or nutricious, flesh-forming; 1, albumen; 2, fibrin; 3, casein. II. Medical or auxiliary: A. Stimulants; 1, alcohol; 2, volatile oils: B. Alteratives; 1, acids; 2, alkaloids: C. Narcotics; 1, tobacco; 2, hemp; 3, opium: D. Accessories; 1, cellulose; 2, gum; 3, gelatine.

It might be objected that this classification pledges itself too exclusively to a certain physiological theory, while it introduces more of purely medical terminology than would seem to be quite desirable. The amount of information, however, on the various subjects to which attention is drawn, is considerable, and very well put; and though there may be a difference of opinion as to theoretical questions, there can be none as to the propriety of the undertaking.

Art. V.—*Epiphora, or Watery Eye: its successful Treatment by the new method of Dilatation; with Illustrative Cases.* By James Vose Solomon, F.R.C.S. Lond., Surgeon to the Birmingham and Midland Counties Eye Infirmary, &c.—*London*, 1859. pp. 32.

This is a very lucid and practical little treatise, showing clearly the advantages gained in the treatment of this troublesome affection by the operation first introduced by Mr. Bowman. Mr. Solomon's experience in this disease has been ample, as he has treated sixty-four cases by this method alone. He adduces nineteen successful cases, and describes a modification which he has introduced in the operation, and which deserves a trial, as it enables the surgeon to dispense with the services of an assistant. We can only say that, having had some experience in this disease, we are quite disposed to endorse Mr. Solomon's opinion of the superiority of the new method of treating it, and his prediction that the style will soon be altogether laid aside.

Art. VI.—*The Mortality after Operations of Amputation of the Extremities, and the Causes of that Mortality.* By Arthur Ernest Sansom, Associate of King's College. The Prize Essay of the Medical Society of King's College, London, for the year 1858.—*London*, 1859.

It is not often that a student of medicine ventures to lay before the profession any literary production, and when such an event occurs critics feel bound to look leniently on the work. We should not, however, be just were we not to admit that Mr. Sansom's pamphlet stands little in need of any such indulgence; and that (putting aside a few faults of style, the signs of want of practice in authorship) this treatise would do credit to far older writers. We commend it especially to those of our readers who were interested in a controversy which Dr. Arnott originated, as to the mortality after operations before and after the introduction of chloroform. Mr. Sansom shows beyond question that the mortality has been much less since 1842 than previously. That this decrease in the rate of mortality is principally due to improved methods of treatment, and especially to the avoidance of a depressing system, and a less rigid adherence to routine, is our opinion, as it seems to be that of Mr. Sansom. Still it affords a complete contradiction to the views of those who wished to show that the mortality had risen since the introduction of chloroform. On the whole, the conclusion to which statistical evidence has brought Mr. Sansom is, that "chloroform exerts an influence favourable to the preservation of life."

The portion of the essay which treats of the causes of death after amputation, is not to our minds the most satisfactory part of it, though even here the deficiencies which we are obliged to note in Mr. Sansom's statistics are only those which are noticeable also in works of greater pretension on this subject. We allude to the absence of evidence as to that verification of the diagnosis which can only be furnished by post-mortem examination. It is our firm conviction that all these tables, founded in great measure on cases which have not been examined after death, give far too great prominence to "shock" and "exhaustion" as causes of death, and that many, if not most, of those so catalogued will be found to be cases of diseased viscera. Of this very common cause of death we see no notice in Mr. Sansom's tables, yet the practical importance of bearing it in mind and bringing it very fully before the eyes of surgeons will not be disputed. There are many useful and interesting statistical tables in this little pamphlet, besides those compiled from the records of the society above named, and we can assure our readers that it will well repay perusal.

ART. VII.—1. *Conservative Surgery. Reports in Operative Surgery.* Series III. By RICHARD G. H. BUTCHER, Esq., M.R.S.A., &c.—*Dublin*, 1859. pp. 50.

2. *Contributions to the Surgery of Diseased Joints, with special reference to the Operation of Excision.* No. 1. *The Knee.* Illustrated with Engravings on Wood. By P. C. PRICE, Surgeon to the Great Northern Hospital, &c.—*London*, 1859. pp. 48.

3. *On the Treatment of Anchylosis, or the Restoration of Motion in Stiff Joints.* By BERNARD E. BRODHURST, &c. &c. Second Edition.—*London*, 1859. pp. 39.

WHATEVER may have been the case in the great world of politics, there is no question that in Surgery the most rapidly advancing party in popular estimation is that of the Conservatives, and no more worthy representatives of the body could easily be selected than the three gentlemen whose names we have prefixed to this article. The two elder have already attained a reputation which would render our commendation impertinent, while of Mr. Price it is sufficient praise to say that of the numerous pupils who have sat at the feet of Mr. Fergusson, he is one of the most enterprising and distinguished. We need hardly state that in all questions of conservative surgery, and in that of the joints more particularly, all our preconceived opinions are in the main on the side which is espoused by the authors of these pamphlets, and of which their practice appears to have been so successful an exposition. Indeed, the results of amputation of the thigh are so discouraging, that it is no wonder that surgeons should seek a refuge from a plan of treatment which involves certain mutilation, with the most imminent risk of life; and so it was perhaps natural that the advantages of resection of the knee-joint should at first have been rather over-estimated by its supporters, as they have certainly been unduly depreciated by the favourers of the old treatment. Accordingly, it used to be asserted that the mortality after excision of the knee is far less than may be expected in amputation of the thigh, and such, if we mistake not, was the doctrine originally proposed by Mr. Butcher. Whether this is really so or not appears very doubtful from the statistics collected by Mr. Price. These give 160 cases of excision of the knee, collected from all British sources, both metropolitan and provincial, and out of these 32 died—a ratio, as Mr. Price remarks, very much the same as that given by Mr. Bryant, in a paper recently read at the Medico-Chirurgical Society, for the amputation of the thigh on account of disease at Guy's Hospital. (Vol. v. p. 43.)

Now, if we take into account the comparative rarity in country districts of pyæmia, erysipelas, and other causes of death after operations, we can hardly resist the conclusion that, had all these 160 resections been performed at a metropolitan hospital—as at Guy's, for example—the mortality would have been greater than after amputation. Add that 17 of those who recovered were obliged to submit to amputation, and the resulting comparison between excision of the knee, as it has been hitherto practised, and amputation of the thigh, is less favourable than we had supposed, even although previously disposed to suspect exaggeration in the panegyrics with which the former operation had been loaded. This however is not, to our minds, an argument for the condemnation of the operation of excision, especially in the face of cases as that at page 6 of Mr. Butcher's pamphlet, which clearly prove its great advantages in suitable instances; but for greater care in the diagnosis and selection of those in which it is applicable. We are very happy to find that neither Mr. Butcher nor Mr. Price, although they are the warm, are not the indiscriminate advocates of this proceeding in all cases. We cannot refrain from quoting Mr. Butcher's opinion as to the excision of joints in childhood:

> "It is impressed strongly upon my mind—nay, more, I would say it is my conviction, from a long and careful study of these affections, that these severer operative measures will very seldom indeed be either warranted or called for. . . . . . Repair in the young is to be looked forward to, to be depended upon as a certainty; and therefore again I repeat, no hasty removal of diseased joints in childhood." (p. 23.)

The principal object of Mr. Price's pamphlet, besides the collection of the statistics, of which we have already indicated the chief feature, is to call the attention of surgeons to the two forms in which strumous disease affects the bones—the limited and the diffused—and to enforce the doctrine, that in the limited tuberculous affection of the joint ends of the bones excision is an efficient and admirable practice; while of its application in the other form he speaks in the following strong terms:

> "I fear even the most strenuous advocates for excision of this articulation can find but little encouragement to adopt a proceeding, the ultimate result of which must in all probability prove futile to the patient, and deleterious to the cause of conservative surgery." (p. 7.)

Had this caution been present to the minds of all operators, we cannot but think that many of the unsuccessful cases in Mr. Price's list of 160 might never have figured there, and his statistics have shown a more favourable result. It is to be regretted that no clearer rules can at present be laid down for the diagnosis of the extent of bone involved; but Mr. Price's treatise will prove a valuable addition to the literature of this operation.

Mr. Brodhurst's pamphlet is a continuation or supplement of his well-known paper on the same subject in the 'Medico-Chirurgical Transactions,' and he relates the history of several cases in which the practice of forcible extension under chloroform has succeeded in his hands in cases of partial anchylosis of the joints, with or without tenotomy. The results of his entire practice are thus given by Mr. Brodhurst:

> "Of 32 cases which I have submitted to rupture (viz., of the adhesions), the following has been the result: In 11 instances complete power of motion, or nearly complete power, has been gained; in 14, partial, but useful, motion has been restored; and in 7 the limb has been rendered straight, and the joint has remained stiff." (p. 15.)

This list includes cases of soft anchylosis of all the larger joints.

Art. VIII.—*A Handbook of Hospital Practice; or an Introduction to the Practical Study of Medicine at the Bedside.* By Robert D. Lyons, K.C.C., M.B., T.C.D., &c. &c.—*London*, 1859. 8vo, pp. 137.

The object of this little work is to supply the student, during his attendance at the hospital, with a methodized plan of clinical observation, and "with brief but explicit instruction as to the best mode of procedure for investigating any given case, arriving at a diagnosis, and recording its history, symptoms, treatment, daily progress, and termination." It also embraces "a compendium of the preliminary knowledge most essential for rightly interpreting and using the information thus acquired and recorded by the student." The task which Dr. Lyons has here imposed upon himself does not give scope for much originality of view, which would, in fact, constitute a fault in its execution, inasmuch as the end to be obtained is the exhibition of undisputed facts, and of methods of investigation generally recognised as satisfactory. Nevertheless, such a work affords abundant room for the display of extensive and well-digested information, sound practical knowledge, and cultivated powers of observation; and none but an accomplished practical physician could execute it with credit to himself and real benefit to the class of readers for whom it is intended. The manner in which Dr. Lyons has acquitted himself of this difficult and useful, though unambitious task, is deserving of all praise.

The work is divided into two sections, the first of which contains "Directions for the Clinical Examinations of Patients," and the second "Post-mortem Examinations."

In the first section, all that relates to the history of the case, the general physical and mental condition of the patient; the state of the circulatory, respiratory, cutaneous, digestive, and urinary functions, is minutely and judiciously set forth; and we consider as worthy of especial commendation the attention bestowed on those points which collectively form the subject of 'Medical Physiognomy,' one of the most important, and we may add, most neglected, departments of medical science.

The regional anatomy of the thorax and abdomen is very accurately mapped out, and excellent directions are given for the manual examination of the viscera contained in the latter cavity. The subject of auscultation is very judiciously and practically handled, and divested of those frivolous minutiæ by which practical men (and students are often very practical men in their way) are apt to be disgusted, and to imbibe a mistrust in a means of diagnosis which, when kept within due limits, is of inestimable value.

Full directions are given for the examination of the urine; but we think the inspection of the intestinal excretions, and of matters ejected from the stomach, is passed over in too cursory a manner, and might be advantageously dilated upon in the next edition.

The second section, 'On Post-mortem Examinations,' is of equal value with the first, and the directions given are such as might be expected from an able and practised anatomist. We may instance those for opening and exploring the cavities of the heart—a process which is too often conducted in a bungling and unsatisfactory manner.

There is an appendix, containing directions for writing prescriptions, and a short glossary of medical terms. Forms are also added for reporting cases in accordance with the directions contained in the work.

We can conscientiously recommend this Manual as one which, on the whole, fulfils all that it promises; and Dr. Lyons has even contrived to mitigate the inevitable dryness of so condensed a work by occasional lively remarks, which may render it more acceptable to the youthful reader.

ART. IX.—*Summary of New Publications.*

IF we determine the priority of claim of any department of medicine to a mention in this Summary by the number of works in our quarter's list devoted to any particular subject, we must, in the present instance, yield it to State Medicine or Sanitary Science. And right glad are we to see that this most useful branch of medical science not only holds its own, but is daily enlarging its sphere of operations, and commanding greater attention.

The Medical Officers of Health of Westminster, Islington, St. Giles's, and St. Pancras, have issued the Reports on the work done in their respective districts; and the Metropolitan Association of the Officers of Health have allowed the appearance, under their sanction, of Sanitary Tracts for the purpose of diffusing useful information on sanitary matters. The first, entitled 'Vaccination,' is by Dr. Ballard; the second, 'Our Duty in Relation to Health,' is by Mr. Rendle. With these we would mention the admirable publications that appear under the auspices of the Pure Literature Society, and which for variety and impressiveness may serve as a model for all popular writers. Dr. Greenhow's paper 'On a Standard of Public Health for England' is a workmanlike production in the field of Sanitary Science; with it may be mentioned Mr. Fox's inquiry into 'The Vital Statistics of the Society of Friends.' A work on 'Primary Pathology and the Origin and Laws of Epidemics,' by Dr. Knapp, comes to us from Philadelphia; 'A Sketch of the Medical Topography of Bengal,' by Mr. M'Clelland; a 'Report on the

Jails of the Lower Provinces of the Bengal Presidency,' and 'The British Soldier in India,' both by Dr. Mouat; the reprint of Miss Nightingale's paper on Hospitals, read before the Association for Social Science; the Custom-house Commissioners' Report on the Customs, including Dr. M'William's Medical Report, deserve special attention. Nor may we leave unmentioned the *Réforme Agricole*, which, in July, contained an article by its editor, M. Nérée Boubée, 'On the Purification of the Thames,' in which the author proposes artificially to substitute a new bottom for the river, its clay-bed being, according to the writer's theory, the main cause of the decomposition of the putrescible matters conveyed into the stream. The Commissioners in Lunacy present us with their Thirteenth Report; the subject of Lunacy in its State relations is also considered by Dr. Arlidge in a work entitled 'On the State of Lunacy, and the Legal Provision for the Insane.' We conclude this part of our Summary with Archdeacon Stopford's pamphlet on the circumstances attending the recent religious revival in Belfast, entitled 'The Work and the Counter-work.'

Under the head of Medicine proper, we have to enumerate a new and revised edition of Dr. Wardrop's work 'On the Nature and Treatment of the Diseases of the Heart.' A work by Dr. Heale 'On Vital Causes;' a reissue of the first volume of Dr. Thomson's 'Life of Cullen,' which appeared in 1832; and the completion of the second volume of the same work by Drs. Allen Thomson and Craigie. With Dr. Thomson's name we may appropriately couple that of Dr. Wilks, who has just published his Lectures on Pathological Anatomy, to which we shall soon recur. A fourth edition of Dr. Lee's work on 'The Watering Places of England' is before us, with a memoir by Dr. Dufresse de Chassaigne 'On the Value of the Thermal Waters of Bagnols.' Dr. Ayre presents us with a Memoir on his well-known Treatment of Asiatic Cholera, which was read before the French Academy; together with the Report of the Members of the Section of Medicine and Surgery. Dr. Black's 'Researches into the Pathology of Tuberculous Bone;' Dr. Brinsmade's Addresses delivered before the Medical Society of New York; Mr. O'Ferral's introductory address 'On Hospital Instruction;' and Dr. Addison's 'Gulstonian Lectures on Fever and Inflammation,' each merit perusal. The January number of the 'Indian Annals,' and the last number of the 'Transactions of the Medical and Physical Society of Bombay,' are before us, and, as usual, contain much interesting matter. A monograph by Professor Hecker, of Freiburg, 'On Elephantiasis,' with numerous illustrations, discusses the pathology and anatomy of this disease. The 'Ophthalmic Reports,' edited by Mr. Streatfield, continue to appear in quarterly numbers. The only work on our table which surgery can claim exclusively is Dr. Williamson's 'Notes on the Wounded from the Mutiny in India,' well illustrated by drawings of preparations in the museum at Fort Pitt.

Obstetric science brings us the 'Contributions to Midwifery and Diseases of Women and Children,' by Drs. Noeggerath and Jacobi, of New York, the greater part of which is a report on uterine and infantile pathology in 1858, after the model of our own reports. From the pen of Dr. Barnes we receive a 'Clinical History of the Eastern Division of the Royal Maternity Charity during the year ending September 30th, 1858;' from Dr. Donkin an essay on 'Placenta Prævia;' and from Dr. Madge 'Remarks on the Anatomical Relations between the Mother and Fœtus.'

Books that do not come under the four heads under which we have classed the foregoing works, and that have yet to be adverted to, are Professor Greene's 'Manual of the Subkingdom Protozoa,' excellently illustrated; Dr. Bull's 'The Sense Denied and Lost,' in which blindness is considered under all its aspects by one who lost his eyesight; an essay, jointly by the Rev. J. J. Halcome and Dr. Stone, entitled 'The Speaker at Home,' containing some admirable advice for all (and who is not?) likely to be called upon to address an audience; Mr. Lister's further 'Contributions to Physiology and Pathology;' researches by Dr. Davy 'On the Causes of the Coagulation of the Blood;' and last, not least, the translation by Dr. William Moore for the New Sydenham Society, of Schroeder van der Kolk's work on the Spinal Cord.

PART THIRD.

Original Communications.

Observations on the Outbreak of Yellow Fever among the Troops at Newcastle, Jamaica, in the latter part of 1856. By Robert Lawson, Deputy Inspector-General of Army Hospitals, and Principal Medical Officer at Jamaica.

An opinion has long prevailed that the severe forms of tropical fever could not originate, or spread, at a considerable elevation above the sea. The grounds for this opinion seem to be the statement of Humboldt that yellow fever was confined to the low country on the coast near Vera Cruz, and that it did not pass the farm of l'Encero, elevated 3045 English feet above the sea, the heat there being insufficient to develop its germ;"* and that of Fergusson, with reference to the varieties of fever occurring at different elevations in St. Domingo. The remarks of these authorities were no doubt correct for the *time* and *place*, but it must not be thence concluded that all the conditions requisite for the production of those forms of fever, except that of suitable elevation, were present in the cases they adduce. Their deductions, therefore, require to be applied cautiously to other localities, and may, under certain modifications of the conditions, be even found inapplicable to the same localities at another time.

With that disposition so common among us, however, to help out preconceived notions by an appeal to the authority of a great name, rather than to correct them by a careful deduction from facts, the opinions of Humboldt and Fergusson have been pushed much beyond their legitimate bearing; but we know too little of the actual causes of severe tropical fever to be able to say with certainty where they may be produced in sufficient intensity to develop the disease, far less to define the point beyond which their production is impossible.

There is no certain test for the presence and operation of the efficient causes of fever but its occurrence among men or the lower animals; but observations on the latter are too few and desultory to admit of frequent application. For all practical purposes, therefore, observations on bodies of men, under conditions sufficiently varied, afford the only means of extending the information on the subject and deciding doubtful points.

In Jamaica there are military stations which have been in existence for many years, the records of which are available for elucidating some of the laws of the disease. Taking Kingston as a centre, the following stations are included within a circle of about eleven miles radius—viz., Port Royal and Fort Augusta, at the level of the sea, and nearly surrounded by it; Up Park Camp and Spanish Town, at moderate elevation above and some distance from the sea; Stony Hill, eight miles from the sea, and 1360 feet above it; and Newcastle, nine miles from the seaboard and about 4000 feet above it. On the north side of the island lies Maroon Town, about twelve miles from the sea coast, and elevated about 2500 feet above it. With the exception of Newcastle (which was first occupied as a military post in 1841), these stations were garrisoned many years, and the returns for them are given in the statistical reports on the health of the troops in Jamaica from 1817 to 1836 inclusive.

The following table taken from these data, shows the average mortality from fever per 1000 of mean strength, at each of the stations above mentioned, together with the extreme annual variations:

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* Political Essay on New Spain, vol. iv. p. 170. English translation, 1822.

Station.	Annual mortality per 1000 from fever for twenty years.	Extreme annual variation per 1000.
Port Royal	93·9	From 0 to 298
Fort Augusta	55·5	" 0 to 278
Up Park Camp	121·0	" 21 to 479
Spanish Town	141·0	" 42 to 368
Stony Hill	70·5	" 3 to 431
Maroon Town	15·3	" 0 to 34

These results form the best approximation to a numerical expression of the activity of febrific causes in the different localities for the period they embrace. From them it is obvious that on the average the stations surrounded by the sea were healthier than those at a short distance from it, and near the same level; while in the case of Stony Hill,[*] and still more of Maroon Town, elevation has been attended with great diminution of the activity of the causes of febrile disease, though not by their entire disappearance. It is manifest, too, that these causes had very different degrees of activity in different years at the same station, though the periods of increase and decrease were nearly contemporaneous at them all; thus indicating the presence of an epidemic constitution at certain epochs, whatever the nature of that may be.

In 1840 an epidemic period commenced, which continued with variable intensity to the beginning of 1842. It commenced at Maroon Town, and twelve deaths occurred from fever originating there, or thirty-six per thousand of the mean strength within the annual period. In the year 1841–42 there were twenty-four deaths at this station from fever; of these, two in May and five in June, were in men of the 68th Regt., which corps had not been away from the station; the remainder were in the 82nd, which arrived there in the end of June, and many of them were attributed to the low ground. This outbreak was coincident with that which occurred in the 60th Regiment on the south side of the island, immediately on its arrival from the Mediterranean. In the fatal cases at Maroon Town, black vomit was either ejected during life, or found in the stomach after death.

In February, 1842, a case resembling yellow fever showed itself at Newcastle, and terminated fatally on the fourth day. I have not found it expressly stated that the man had not been away from Newcastle, but from the context this seems implied. Staff-Surgeon Hawkey and Staff-Assistant-Surgeon Jameson were sent to examine the case, and their opinion as given in the General Quarterly Report to March 31st, was, "that the real origin of this instance of fever was very doubtful indeed; that the symptoms during life were most equivocal; but that the appearances of the characteristic black fluid in the stomach after death clearly betrayed the morbid agency of febrific miasm."

In October, 1848, a period when yellow fever had been prevailing extensively in the West Indies, a malignant fever broke out in the family of the schoolmaster-sergeant of the 97th Regiment, at Newcastle. The family consisted of himself, his wife, and three children, and a woman who attended them: all these, except the sergeant, were attacked with fever; he had dysentery; and the wife and children died.

The disease was confined to this family, and was attributed chiefly to the emanations from a cesspool to windward of, and within thirty yards of the hut in which they lived. None of these had been away from Newcastle for a considerable period previously. Deputy Inspector-General Dr. Watson, in his official report to the officer commanding, states that the disease was "a fever of a peculiarly low and pestilent nature;" but Staff-Surgeon Dr. McIlree, who was then surgeon of the 97th, and had the cases under his immediate observation, has favoured me with a statement from his notes made at the time, from which it appears several of these were characterized by rapid course, yellow skin, and black vomit; in other words, were decided yellow fever. The hut in which these cases occurred was situated close to the bend of the road immediately in front of the lowest barrack, at a point 3520 feet above the sea.

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[*] Since this was written, I have found, on personal examination, that there is a considerable amount of marshy ground actually among the buildings at Stony Hill, and a considerable portion of the surface seems to be kept wet by springs coming to the surface at various places.

In July, 1850, cases of severe fever began to appear at Maroon Town, and Dr. Maclean, Deputy Inspector-General, in his annual report for that period, states that the fatal cases were characterized by yellow skin, and the formation of black vomit in the stomach.

I have thought it advisable to adduce these facts with reference to the more elevated military posts in Jamaica, previous to entering on the special consideration of the occurrences at Newcastle in the end of 1856. They show that though the high land stations may, in ordinary years, present a degree of health little inferior to that observed in Europe, yet when an epidemic constitution prevails, they are by no means exempt from its influence, and may even, as in the case of Newcastle on the late occasion, suffer severely, though it is probable to a far less extent than the low land stations under similar circumstances.

The military station of Newcastle is situated near the western extremity of the Blue Mountain range, on its southern aspect, and about nine English miles N.E. by N. from the sea-beach at Kingston. Owing to the difficulties of the ground the distance by the road is about fifteen miles. The highest point in the neighbourhood of Newcastle is St. Catherine Peak, which attains an elevation of 5000 feet above the sea, from this the ground proceeds southerly 1600 yards to another peak less elevated, forming the eastern boundary of the space enclosing the station. From the flank of the latter peak a sharp ridge is thrown off to the S.W., though at a greatly reduced elevation, which forms the southern boundary of the valley on that side of Newcastle. The ground slopes away from the peak to the southward, throwing off abrupt ridges intersected by deep hollows, and forms the eastern boundary of the Hope Valley, which drains the whole, and through which the road to Newcastle passes.

From St. Catherine Peak the ridge of the Blue Mountains passes in a westerly direction, and at the distance of 2270 yards there is a small pointed peak, from the southern base of which a sharp ridge runs off about S.S.E., rapidly declining in elevation until it nearly meets the spur crossing from the peak to the southward of St. Catherine's, thus forming the western boundary of the space surrounding Newcastle. The cantonment itself is on a spur given off from the connecting ridge about midway between St. Catherine Peak and that to the westward. This spur has a southerly direction, and falls rapidly as it leaves the parent ridge, maintaining, however, an elevation much the same as that of the western bounding height, at a corresponding distance from its northern commencement.

The cantonment occupies a space of nearly 800 yards in length; and the difference of level between the highest and lowest building is 505 feet. The mess-room is 4050 feet above the sea.* The top of the ridge is so contracted in many places that there is room for single houses only, while its sides descend at an angle which is seldom less than forty degrees, and in some places fifty degrees, below the horizon. At other places it spreads out considerably, giving room for more extensive buildings; but the slopes terminating in the water-courses are everywhere abrupt, and the latter deeply excavated. On the western side of the cantonment there is but one large valley, which is pretty well cleared; to the eastward the valley, as it ascends from the lower part, branches out into a number of smaller ones, separated by sharp ridges, and these generally contain much bush. From the nature of the ground the fall is everywhere so great that water finds a ready outlet, and there is nothing of the nature of marsh to be seen; while, from the frequent rain and the supply from the springs, the main water-courses have always a stream in them.

The soil in the neighbourhood seems to be clay, mixed with vegetable matter on the surface; though where excavated the clay is found stiff and unmixed, and is of a red colour. This clay overlies a bed of marl of a yellowish-grey colour, and that again seems to be bedded in sandstone of a purplish-blue colour, and of remarkable firmness and cohesion; large boulders of this nature are found all over the flanks of the hills, where the action of the rain has washed away the soil and left them exposed. The stratum of clay attains considerable thickness in many places, and in several has been eaten into deep gullies from the action of the surface-drainage, or extensive slips have taken place.

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* Some years ago it was proposed to make a carriage-road from the low lands to this station; when the levels were taken, the elevation of the plateau on which the mess-room stands was found to be 4050 feet above the sea. This information was derived from the plans in the Engineer's Office, in Jamaica.

It has been necessary to cut the ground at Newcastle into terraces, to obtain level space sufficient for building. The face of the scarp in these cases (usually composed of a red clay, sometimes embracing a portion of the marl also) is occasionally left uncovered. Sometimes it is partially covered in, and in others wholly, by a stone retaining wall. The scarp varies from a few feet to twelve or fourteen in height, and there is a passage between the back of the corresponding house and its base, varying from three or four to ten or twelve feet, in different cases.

The houses for the men are of one floor, raised from the ground about two feet on a stone wall, with four ventilating spaces in front and back, and one at the ends, each seventeen inches long and seven inches and a half deep, fitted with open iron gratings, through which there was generally a sufficient draught. The huts are of wood, lined substantially, and closely floored, and open to the ridge inside the roof. They have a door covered with a porch; glass sash windows; and an arrangement in the roof for ventilation, which, if properly attended to, and care taken to admit air below, would always secure a sufficiency of fresh air for the inmates.

The rooms lettered A and B are fifty-three feet long, twenty-six feet broad, ten feet six inches from the floor to the tie beam, and ten feet six inches from that to the ridge; and the roof is hipped at each end. At six feet from the front there was a partition forming a sort of verandah, but with louvre boarding at the upper part, communicating with the rest of the room. There were jalousies in front of these buildings in place of glazed windows, though elsewhere the sash windows were inserted. Allowing for the shape of the roof, the cubic space in these rooms is 16,447 feet in the greater part, and 4074 feet in the verandah.

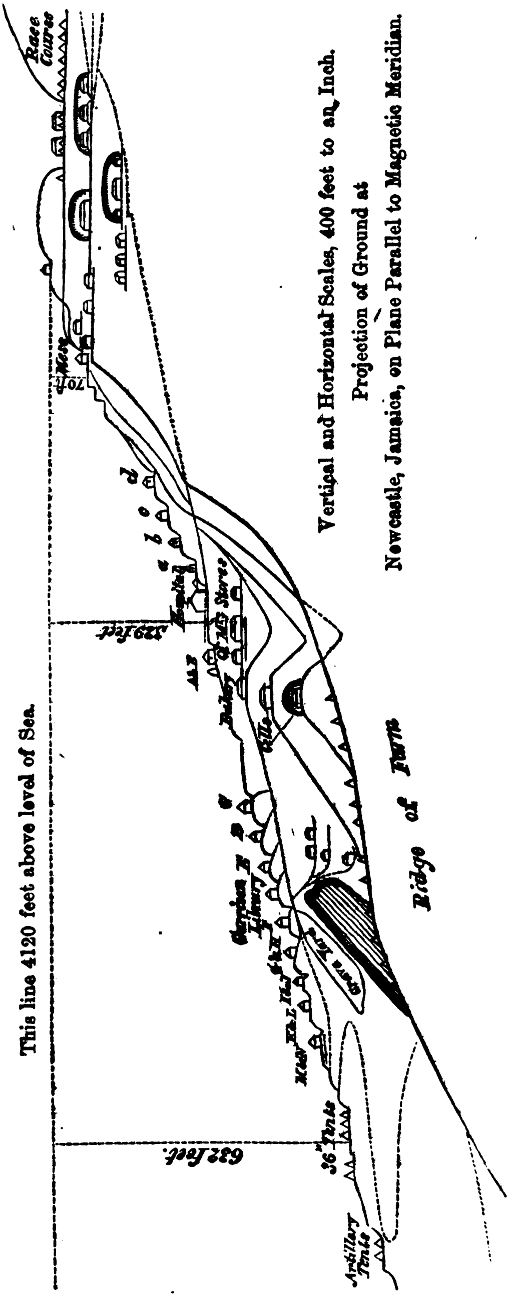
The other rooms, lettered C to N, are fifty-four feet long, twenty four feet wide, ten feet to the tie beam, and ten feet from that to the ridge. The roofs are also hipped. Allowing for this arrangement, the cubic space in these rooms is 18,480 feet.

The hospital is a stone building, surrounded by a jalousied verandah, ten feet wide at the front and sides, and seven feet and a half at the back. The main building is divided into three wards, numbered from 1 to 3, from west to east; each is thirty-one feet long, twenty feet wide, fifteen feet four inches to the tie beam, and eight feet nine inches from that to the ridge. The partition walls are of stone, and reach to the tie beam. The space above is open from end to end of the building, and there is no ceiling to the wards. Each ward has a door, and two sash windows, both in the front and back walls; and over each, a louvre-boarded opening the width of the door or window, and a foot high. The cubic space in each of these wards is 9507 to the tie beam, or 12,219 feet, including that up to the ridge. There is a similar arrangement in the roof of the hospital for ventilation to that already mentioned in connexion with the barracks.

The officers' quarters are of one floor, which is raised from the ground, on walls or pillars, about twenty inches or more, according to the nature of the surface. They are sufficiently commodious, and fitted with glass sash windows everywhere. There are several other buildings about the cantonment for staff-sergeants, and workshops, which are of a similar character. These were for some time occupied by married people during the progress of the sickness. There are also in several places huts of wattle and daub, which were occupied by married people, which have earth floors merely, and are of course not raised from the ground.

The privies and kitchens are generally on the western slope of the hill; the former are all constructed with cesspools, which are not trapped, and at the commencement of the disease were very offensive. During its progress charcoal was used pretty freely, and their condition was much improved.

The barrack cells are in a substantial stone building (see plan), about 400 feet to the eastward of the hospital, and 140 below it, on the edge of a ravine. They are in two rows, of four each, placed back to back. There is a passage of five feet wide in front of each row, with jalousied windows, and the cells open directly from them. The cells are ten feet long, eight feet wide, and ten feet three inches to the eaves, giving a cubic content of 820 feet. Each has a small opening at the side of the door, near the floor, and a


Vertical and Horizontal Scales, 400 feet to an Inch.
Projection of Ground at
Newcastle, Jamaica, on Plane Parallel to Magnetic Meridian.
This line 4120 feet above level of Sea.
Race Course
70 ft
Mess
329 feet
Hospital
Stores
Bakery
632 feet
36th Tents
Artillery Tents
Garrison Library
Ridge of Fern

barred opening over the door of the same width, and about three feet high. In the cells to the eastward there is an opening in the roof for ventilation; in those to the westward there is a similar opening, but instead of leading directly from the cell, it opens into the upper part of the passage already mentioned, and of course does not insure the same thorough ventilation as the other. There was no privy attached to the cells, and the prisoners in obeying the calls of nature had to go to a spot in the bottom of the ravine, where a temporary place was erected. The surface drainage passing through this spot carried off the soil completely.

The guard-room was a small wooden building raised from the ground on pillars. It was originally situated over the centre of the space now occupied by the front wall of the church, but was removed from this to the front of A room, about the end of September or first week in October. The trench for the foundation of the church was commenced on the north side on October 8th, and the ground opened all round by the 16th. The ground was not fairly filled in again before the end of October. The soil (not clay) removed from the trench was employed to raise the surface in front of the new guard-room. This guard-room was occupied until November 5th, when it was vacated, and the men on guard accommodated in marquees on the parade-ground.

At the commencement of the epidemic this guard furnished one sentry over the guard-room, one at the hospital, one at the quarter-master's store, and one at the canteen. At a later period two others were given for rooms C and F, when these were occupied by sick. There was, besides, a small guard of one corporal and three privates mounted over the cells every night, and which was accommodated in a bell tent close to the cells.

In the early part of June, 1856, two companies of the 36th Regiment were stationed at Up Park Camp, and the European artillery at Port Royal. On June 10th, the former were removed to Stony Hill; and the following day, thirty-five of the latter went to Stony Hill, and thirty-six to Newcastle. Four cases of yellow fever had proved fatal at Port Royal among the artillery between the 12th and 30th of May; and a man of the 36th died from the same disease at camp on May 10th.

In June two officers of the 36th died at Stony Hill of yellow fever—one on the 16th, and the other on the 29th; and about July 20th, fever of the same character began to show itself among the men, and continued during August. As this was attributed to the barrack being out of repair,[*] sixty-five of the 36th were removed to camp on August 9th, and on the 20th of the same month they left camp for Newcastle.

Fever continuing at Stony Hill, on August 21st, the men of the 36th who were able to proceed were sent to Newcastle, and the artillery to camp; but several cases proving fatal at camp subsequently, they also were moved to Newcastle on September 19th.

The strength of the force at Newcastle during the last six months of the year was:

	Officers, including staff.	N. C. officers, drummers & privates.
July	12	490
August	14	499
September	18	650
October	20	685
November	18	667
December	19	637

These numbers include one sergeant and eight men who were stationed at the Botanic Gardens, a post in a narrow valley six miles from Newcastle, on the road to Kingston, and about 1100 feet above the sea.

On the detachments from Camp and Stony Hill proceeding to Newcastle the troops were somewhat crowded, thirty-six men occupying each room, and the remainder being in tents on the flat space in the immediate vicinity of the rooms.

In consequence of two patients having contracted fever in hospital, the sick (with the exception of yellow fever cases) were removed from that building on September 21st, and accommodated in marquees on the open space in front of it. The building was whitewashed, and the sick re-occupied it on October 14th; but fever again appearing, they were removed on October 23rd to rooms C and D, which were cleaned for the purpose.

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[*] Recent examination has directed attention to a considerable extent of marshy ground of long standing in the neighbourhood of the buildings at Stony Hill.

The first case of yellow fever which was seen at Newcastle in 1856 was that of Private William Green, 36th Regiment. This man had been suffering from chronic catarrh, and went to the Botanic Gardens on January 23rd, for change. He complained of being out of sorts on June 27th, but being sickly, little was thought of it; becoming worse, however, on the 29th he was removed to Newcastle, where he became yellow, had black vomit, and died on July 2nd. He had not had communication with any case of fever previous to his own attack, and had not been away from the post above half a mile at any time. The sergeant and the men at the post remained healthy, both then and throughout the subsequent epidemic at Newcastle.*

On going to the hospital, Private Green was placed in No. 3 ward, in which there were several other patients; the number under treatment at the time was 20. There was no other case for some time.

On August 21st, the detachment from Stony Hill arrived at Newcastle, and the following day two of them, Privates Brown and Griffiths, went to the hospital with fever; the former died on August 25th, and the latter on the 27th, both being yellow, and having had black vomit. Another man of this detachment, Private Anderson, went to hospital on the 23rd, and died on the 26th, presenting the same symptoms. On August 24th, another, Private Rossu, who arrived from Stony Hill that day, went to hospital, and died on the 28th, with the same symptoms. Private Reuben Smith arrived from Up Park Camp on August 28th, where he had been in hospital under treatment for remittent fever from August 12th to the 27th; as he complained of weakness, he was taken into hospital, though not then placed on the books. On the 31st, fever appeared, while in hospital, and he died on September 7th, with yellowness of the surface, and black vomit. These men were accommodated in No. 2 ward, in which there were several other patients; the total number of sick in hospital at the time increasing from 24 on August 23rd, to 33 on Sept. 7th. These cases may be, perhaps, referred to the low ground; at all events, nothing decided can be said as to where they were produced; the next, however, is more important, and requires special consideration.

Private Henry G. Meloney, 36th Regiment, the next case, arrived from Stony Hill on August 21st, and was accommodated in a tent at the west end of the hospital, and to the north of the orderly-room. This was a delicate-looking man; he had been under treatment for fever a day or two before he left Stony Hill, but since his arrival at Newcastle had been employed on the public works, though weak and sickly. He got wet on September 6th, and had a rigor, and he attributed his subsequent illness to that wetting. He was admitted on September 7th, labouring under low fever, which pursued an obscurely remittent form until the night of the 14th. The following day there was a considerable improvement, and in the evening a decided exacerbation, and the tongue then presented for the first time the red tip and sides so common in yellow fever. The fever continued during the 16th, and on the 17th there was pain in the chest (epigastrium), for which a sinapism was applied with relief, and about midday the fever remitted. There was slight fever during the night, and the following day yellowness of the surface was noted, and in the evening vomiting of brownish matters and much prostration. From this period he gradually became weaker, and died on the 19th, at half-past nine p.m. On opening the body, the liver was found large, pale, and friable, and the stomach and intestines contained a large quantity of black vomit.

It is not easy to determine whether this case should be attributed to the low ground, or whether it was the result of exposure at Newcastle. The man had been sick at Stony Hill, but was at work seventeen days at Newcastle before he got wet, and manifested symptoms of fever there—a long period of incubation, but not longer than has been occasionally observed, so that nothing can be satisfactorily deduced from that.

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* Dr. Bowerbank informed me that there were two cases of yellow fever in this neighbourhood: one on Sept. 16th, which recovered, and the other on Oct. 9th, which proved fatal: both individuals, however, had been away from the locality in the low ground previously.

The course of the disease was unusual for yellow fever, having gone on from the 6th to the 14th, without displaying the character of that disease decidedly. On the 15th, there was either a relapse or an attack of a new fever, which presented the peculiar tongue frequently seen in yellow fever; this remitted on the forenoon of the 17th, and was accompanied by the uneasiness about the præcordia characteristic of this disease; and the following day the yellowness of skin, irritability of stomach, and sinking, and terminated fatally on the 19th. The latter part of the course of the disease was in every respect, therefore, analogous to the ordinary course of yellow fever, and it is difficult to suppose that, had the original attack been of this nature, some of the symptoms it subsequently presented would not then have shown themselves. These speculations are of importance, for if the original attack were remittent fever only, then the subsequent yellow fever must have arisen from causes in operation at Newcastle, and not from poisoning of the system during his residence at Stony Hill; for had that been the case, it would have been brought into action on the first attack on September 6th.

Meloney was treated in No. 2 ward, the same in which Smith, the last man labouring under yellow fever, was, and in the next bed to that in which he was, in the north-west corner of the ward. Smith died at seven A.M., on September 7th, and the body, with the bedding, was removed in half an hour to the dead house; while we have seen that Meloney was actually indisposed the previous day, and he came to hospital at ten A.M. only. His first attack of fever, therefore, arose independently of the hospital or of its inmates; had contagion been superadded to the first attack, it must be concluded that the form would have been aggravated at once, and it is contrary to all experience to suppose that the disease would have gone on for a week, at the end of that time show the improvement it did, and then, from contagion applied a week before, assume the characters of yellow fever.

The question is, therefore, narrowed to this: if his first attack were yellow fever, it was called into action by his getting wet on the 6th, and the state of the system determining that form of disease may have been contracted either at Stony Hill or at Newcastle, there is no evidence to determine which; if the first attack were merely remittent, the subsequent one must be altogether attributable to causes in operation at Newcastle, and if the reasoning given above be correct, these could not have been connected with specific contagion.

On September 17th and 18th, two men who were in the same ward with Meloney, at its south-east corner, became affected with fever, which assumed the yellow form. These were Privates Joseph Austin and Timothy Wild, both labouring under ophthalmia; the former had been under treatment since April 10th, and the latter since August 30th, and it is believed neither had been away from Newcastle since March. Both had yellow skin and black vomit, and Wild died on the 20th; the other recovered.

These were the first cases of yellow fever in men who had not been away from Newcastle, and they arose under circumstances sufficiently suspicious. From the above details it is clear that they can decide nothing as to the causes of the disease, unless that these, whatever their nature may be, were in operation.

The next cases which occurred, however, are of greater use; these were in Private David Monk, who was engaged on the public works, and lived in the piazza of A room, and Mrs. Bell, who lived in B room, which was occupied by married soldiers and their families. These rooms, as will be seen by the plan, are one hundred feet in front of the hospital, and at a considerably lower level, and are ninety-five distant from each other. Monk, while engaged at the public works, got wet on Saturday, September 20th; on the Sunday he was feverish in his room, and the following morning went to hospital; he died on September 26th, yellow, with black vomit.

Mrs. Bell, an industrious, respectable woman, was attacked on September 22nd, and died on the 25th; yellow, with black vomit. So far as could be ascertained at the time, neither of these had been away from Newcastle since the early part of March. Immediately after their decease inquiries were made to ascertain whether either had had communication with those who were previously sick, or had washed clothes for them, or been in any way exposed to contagion, but no trace could be found of communication, in any way, with the sick, or with each other. The husband of Mrs. Bell was not attacked subsequently.

These cases occurred under circumstances so different from those preceding them, that many of the doubtful points surrounding the origin of the latter can be eliminated. They do not appear to have been away from Newcastle for months previously, therefore their attacks cannot be attributed to the influence of the low ground. They do not seem to have had any communication with others labouring under the disease, either directly or indirectly, therefore it cannot be attributed to specific contagion; and they lived in different buildings, and do not seem to have had communication with each other, therefore they afford a stronger proof of the cause being in operation at Newcastle, and somewhat diffused.

While these circumstances were taking place, several cases of fever occurred in barracks, but they presented none of the malignity of the yellow fever, and were returned as Febris C. C. Of these, one, Private Walsh, came from A room on September 2nd. This man had been on guard on August 1st. He belonged to the light company, which had not been away from Newcastle for some time previously. The next was Private Smithson, who lived in a tent between the hospital and B room, who went to hospital on September 11th; he came from Stony Hill on August 21st. The next case was from A room on September 14th. The subject of it, Private William Ball, Light Cavalry, had been on guard on the 2nd. Another case occurred on the 15th, in Private Whilehan, in L room. This man was permanently employed on the public works. The next case was that of Private John Lye, who lived in B room, and was employed as regimental policeman. He was attacked on September 17th. The next case was that of Private Fallon, who lived in H room, and who came from Up Park Camp on the 20th August. He was attacked on the 19th September. Another man, Private George Fisher, arrived from Up Park Camp on September 24th, and went to hospital immediately. On September 26th, Serjeant Freeman was admitted from B room. He came from Up Park Camp on August 20th, and had been on guard on September 17th. Another came from L room on September 26th. The subject of it, Private Sturdy, had been on guard the previous day.

Of these men, Walsh, Ball, Whilehan, and Lye were treated in No 1 ward, and Smithson in No. 2, and all subsequently were placed in marquees outside, when the hospital was evacuated on September 21st. Whilehan had one scruple of quinine and five grains of calomel on the day of admission. Freeman had ten grains of quinine, and Sturdy ten grains of quinine with eight grains of calomel. The others had from nine to fifteen grains of quinine each the second day. In none of these cases were there either yellowness of surface or hæmorrhages of any kind, and they all recovered. They were returned as common continued fever from the first, and nothing arose during their progress to alter this opinion. It seemed proper to introduce them here, not that they have any very important bearing on the case, further than showing that the disposition to fever was, if anything, more remarkable then in the neighbourhood of the hospital and rooms A and B, than anywhere else in the cantonment.

On September 27th, Serjeant Joseph Catton, who was acting as serjeant-major and had been drinking very hard for some time, was taken to hospital at six a.m., affected apparently with epilepsy; there was tenderness of epigastrium, some irritability of stomach, and vomiting of brownish matter resembling incipient black vomit. He died at one p.m., in a fit. On examining the body, the surface was yellowish; the membranes of the brain congested; the mucous membrane of the stomach of a bright scarlet colour, denuded of epithelium around the cardiac orifice, and containing some light brown fluid. The liver had the nutmeg appearance. It may be doubted whether this were a case of yellow fever, but the appearances on dissection agree very closely with those found in that disease. From September 1st he lived in the serjeants' mess-room, a building to the south of B room; his duties as serjeant-major would require him to be moving about much, though they were not likely to have brought him in contact with the sick; but upon this point there is no positive information.

On October 7th, Private Charles Voile went to hospital. In him the disease assumed the form of yellow fever, and he became yellow, and had black stools. He had not been absent from the station for months. He came from B room, the same from which Mrs. Bell, whose case has been alluded to above, lived in, and from the next bed to that occupied by her. This coincidence might be construed into proof of contagion, but it must be received with the qualification that his wife and child, who slept and lived at the same place, and had not had the disease before, as well as many others similarly circumstanced in the same room, were not affected.

On October 10th, Serjeant Bennett, who lived in a tent below B room, near the serjeants' mess, was attacked; he died on the 13th, without being yellow or having any hæmorrhage, but with suppression of urine. His wife, who had not had the disease, was not subsequently attacked. There is no ground to suppose that this man had not had communication with persons labouring under the disease, and therefore by itself it is of no great value in elucidating its causes.

After October 12th, the disease showed itself in several localities, and in a very aggravated form. The first case was that of Private Leather, who had been in the cells, and at hard labour,[*] since September 17th, by sentence of court-martial. This man was employed on the roads during the day, and locked up when not at work. He was employed under the superintendence of a non-commissioned officer, and his work did not take him near any of the sick with yellow fever, or into the rooms from which they came. He was admitted to hospital on October 12th, and died on the 17th; yellow and black vomit was found in the stomach. Here, then, is a third case which arose under circumstances which, had they been arranged to prove the origin of the disease from local causes, could not have fallen out more remarkably. Further, it indicates that the cells were within the range of the causes of the disease.

On October 13th, Serjeant Price, the hospital serjeant, was seized, and he died on the 15th, under a marked form of the disease. He had lived in A room, officers' quarters, above the hospital, and for three days before his attack, in his own room in the hospital. The same day, Private Hickey, from G room, was attacked. He died on the 16th, yellow, with black vomit. He was on the main guard on October 4th, and had been on pass to the village of Middleton two days before admission, which, from the character of the place and the usual practice there, is equivalent to stating that he had been indulging most freely in various ways. These cases by themselves prove little, for Serjeant Price was in the middle of the causes of disease, if these were local, and exposed to contagion, if that existed; while Hickey, in addition to local exposure, had been dissipating and absent from the locality, though it is not known that he was ever exposed to contagion. Several others were attacked subsequently in G room, who had not been away; and on the other hand, there were many men during the course of the epidemic who had been at Middleton on pass, or absent without leave, who never suffered. It is worthy of remark that a man was admitted from the same room as Hickey, on the 11th, with common continued fever.

The next case may be attributable to the low ground; the facts, however, were these, and upon the whole, they seem to warrant its being referred to Newcastle. Ensign G—— had exchanged from the 36th to the 3rd W. I. Regiment; he left Newcastle on the 13th October for Up Park Camp, to join the latter corps. He was guilty of considerable excess that afternoon, and exposed himself a good deal to the sun; the next morning (the 14th) he reported himself sick at camp, and he died on the 17th, yellow, with black vomit. When Mr. G—— first complained at camp, he said he had felt unwell before leaving Newcastle. He lived in *b* room of the officers' quarters, just above the hospital.

On the 15th October, there were five attacks, of which four were returned as yellow fever, and one as common continued fever. Of these, the first was Private Thomas Wild, who had been under treatment since June 16th, for chronic hepatitis. He was in a tent till October 13th, and in No. 3 ward after that date. He had yellow skin and black vomit, but recovered. The next was Serjeant Charles Kierman, who came from

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[*] By the court-martial return, Private Leather was confined Sept. 12th for insubordination, tried on the 15th, sentenced to be flogged, but the sentence was commuted to forty-two days' imprisonment with hard labour. The proceedings were approved on the 15th, and, according to the usual custom, the imprisonment would commence on that date, instead of the 17th, as above; but under either view the man was a prisoner from Sept. 12th, either in guard-room or cells.

Stony Hill on August 21st, and lived in the verandah of B room. He went into hospital on October 15th, and died on the 25th, having been yellow and had black vomit. Two artillerymen were admitted; one from M room,—he was on the guard in the cells eleven days previously,—and the other from a tent on the lowest plateau in front of it. These seem to have been slight cases, and both recovered. A man of the 36th was admitted from K room. He had been on main-guard on the 12th. His case was returned common continued fever, and he recovered.

On October 16th there was but one admission from fever, and that of the common continued form. The subject of it, Private Mills, came from H room. He had been on the main-guard two days previously. On the 17th there were two admissions—one, Captain Oram, from room B. He had come from Stony Hill on August 21st; he died on October 20th, yellow, with black vomit. The other case was of the common continued form, and came from a tent to the west of C room. He recovered.

On the 18th there were two cases of yellow fever; these were Private Gale, who had been under treatment for a sprain in the hospital marquee since the 15th, having previously lived in I room. He had epistaxis, but was not yellow, and recovered. The other was Lieut. Hugo, who lived in C room of the officers' quarters, above the hospital, next house to that which Ensign G—— had occupied. He had walked about twelve miles on October 12, and got wet, and on returning took a cold bath. He was yellow, and had incipient black vomit, but recovered.

On the 19th there was but one seizure—Private Matthew Caffery, an hospital orderly, and much employed about the fever patients. He had been employed in the hospital at Newcastle since May 8th, 1856. He died on the 25th, yellow, with black vomit. There was but one case on the 20th—Private Henry Winters, who lived in B room. He was on the cell-guard on the 19th, and had come from Stony Hill on August 21st. He died on the 22nd, yellow, with black vomit. Two of his children, Susanna and Mary Ann, were attacked on the 22nd and 23rd respectively. The former was yellow, the latter not, and neither had hæmorrhage; both recovered. This man's family comprised his wife and two other children, neither of whom had previously had the disease, and were not attacked subsequently, though the mother nursed her sick children. They occupied beds in the married room, opposite that of Mrs. Bell, the first case in this room.

Three cases occurred on the 21st. One of these, Private Thomas Adams, was under treatment in a marquee in front of the hospital, from October 17th, under the head of Dyspepsia. He had been in a tent near A room before reporting himself sick. He arrived at Newcastle from Up Park Camp on August 20th. There was yellowness of surface, but no hæmorrhage, and he recovered. The other case came from A room. The subject, Private Gribbin, had not been absent from Newcastle for months. He died on October 25th, yellow, with black vomit. The third case on the 21st was Private Patrick McDermott, who lived in I room. He was neither yellow nor had hæmorrhage, and recovered.

One case, on the 22d, has been already alluded to. Another occurred in Private Thomas Gunning, who lived in L room. He had been on the main-guard on Oct. 19th; he had neither yellowness nor hæmorrhage, and recovered.

On the 23rd there were three cases—two of yellow and one of common continued fever. One of the former has been already alluded to in connexion with B room; the other, Mrs. Kehoe, had left that room on October 21st for a tent below M room, from which she came sick on the 23rd, but is classed as if from B room. She had not been away from Newcastle for months; she had black vomit and hæmorrhage from the mouth, but recovered.

At this time the hospital was evacuated, and the other changes in the distribution of the men were made which have been already mentioned. The disease has been traced thus far day by day to show the succession of the cases; but in describing its subsequent progress, it will be better to follow it in each locality where it appeared.

To commence with the race-course. The men from A and C rooms were sent here from October 21st to 25th. One case of yellow fever had occurred in A room on September 21st, and another on October 21st, and none had as yet appeared in men residing in C room. On the 26th one case of fever occurred, which was returned as common continued fever, and recovered.

On the 27th a case was reported as yellow fever, and another on the 28th, which terminated fatally on November 1st; neither of these had yellowness or black vomit. On the 30th there was another case, which terminated fatally on November 4th, with yellowness and black vomit. All these men had lived in A room before coming to the race-course, and the period which elapsed from their leaving it to the attack was quite within the period of incubation, and in these the disease may be fairly referred back to the influences they were subject to in that room. They carried their bedding with them, and that of the men attacked was not exchanged, yet the disease disappeared almost immediately, and it did not affect men from C room, where hitherto it had not appeared.

On November 2nd a case occurred at this locality in a man who had attended Lieutenant Hugo (who had yellow fever on October 18th) until a few days previously; his case, however, was considered more fright than fever, and he never presented a serious symptom. From the beginning of November there was no case among the people here until the 28th, when a case of common continued fever, of a trivial nature, occurred, which terminated favourably. On December 7th one case of yellow fever was brought here from below, which terminated fatally; and another occurred a few days afterwards, but without the disease affecting any one else; these will be noticed hereafter.

The cases of two officers, Ensign G—— and Lieut. Hugo, have already been alluded to. Subsequent to them a female servant of an artillery officer was attacked; she resided in *a* room, the second below the mess-room. She had been at Port Royal on November 22nd, and on returning that day got wet. She was seized on the 26th; there is no reason to suppose she had been in communication with sick.

On the 28th Lieutenant Stuart was attacked; he had been at Kingston on November 21st and got wet, and did not change his clothes; he resided in *a* room under the race-course, the first to the north of the stable, but had his office in *a* room, next above the hospital. Lieutenant Stuart was yellow, but had no hæmorrhage, and recovered.

Staff Assistant-Surgeon Gordon was attacked the same day as Lieutenant Stuart, and had yellow surface and black vomit, and died on December 7th. This officer arrived from England on November 14th, having never been in the tropics before; he went to Newcastle on the 19th, and had diarrhœa on the 24th and following days, and fever on the 28th; he resided in *b* room, above the hospital, the same in which Mr. G. was. Both these officers were among the sick, and Mr. Gordon, the morning before he was seized, had been present at a post-mortem examination of a man who died of dysentery, but in whose stomach black vomit was found.

Lieutenant Le Gallais, R.E., is the last case among the officers, and it is doubtful whether in him the disease be attributable to Newcastle or the low ground; he lived in *a* room, the first below the mess-room. On December 2nd he fatigued himself in the valley between Newcastle and the farm, passing through the bush in search of a place for ball practice. He took a cold bath on returning home. On the 3rd or 4th he went to Spanish Town, felt unwell on the 6th, but stated he had not been quite right since the 2nd; had a rigor on the 7th, followed by fever, and died on the 11th; yellow, with black vomit.

Though these cases may be referred to other localities than Newcastle, yet it is a significant fact that all of them occurred in persons who either lived, or passed a portion of their time, in the houses on the ridge between the mess-room and hospital. Seven of these houses were occupied by Europeans, and persons in four of them suffered, while officers in other parts of the cantonment escaped, though they were as much in contact with the sick, or absent from Newcastle, as those who were attacked.

Proceeding downwards, along the cantonment, the next locality which exhibited fever was the hospital. Its course in this building has been traced to the 23rd, the day when the sick were removed to rooms C and D, but as several cases occurred in these rooms, within a few days after the removal, which were in all probability referrible to influences in operation at the hospital, it is as well to consider them here.

The first case which occurred after the sick were removed was that of Private R. Box, who had been employed as hospital orderly since September 16th, and who was much in contact with those sick with fever. He was attacked on October 24th, and died on the 30th; yellow, with black vomit. The next attacked was Private Sharpley, who had been under treatment since April 9th, for stricture of the urethra; he exhibited symptoms of fever on the 25th, and died on October 30th, with incipient black vomit. Private John Fieldhouse was the next case; he had been on the main guard on October 18th, and lived in N room; he was in the hospital for a few hours on the 23rd, labouring under acute rheumatism; the complaint took the form of yellow fever on October 28th, and he died on the 31st; yellow, with black vomit. The next case was in private John Draine, who had been in hospital from October 13th, with delirium tremens; he was attacked on the 29th, but recovered. Private John Wilson, the next case, had been in hospital from September 10th, affected with ophthalmia; he was attacked on November 9th; he also recovered. The last two cases did not exhibit either yellowness of surface, or hæmorrhage of any kind. These men had all been in the hospital before it was evacuated, and, with the exception of Box, they were all in C room, until the fever declared itself, when they were removed with their bedding to D room, which was employed as the fever ward. The only other case in the hospital was that of Private Joseph Needham, who was admitted from the race-course on November 18th, under the head of dyspepsia, which, on the 22nd, was changed to dysentery; he sank rapidly on the 26th, and died; on examining the body, black vomit was found in the stomach, and ulceration in the colon. He had attended a man sick with yellow fever on August 28th. Needham was treated in E room, among the men convalescent from fever.

It is to be here observed, that though there were a considerable number of men under treatment at this time, none but Needham was attacked with yellow fever, who had not been in the hospital previous to October 24th, where it was then prevailing; and it is further to be remarked, that while occupied by the troops, no case of fever had come from either C, D, or E rooms. From the latter fact it is clear that, up to the date of removal of the troops, the causes of fever were not operating in these rooms with any intensity; and subsequently, when many cases were introduced from other places, they did not affect the hospital attendants, or spread beyond the men who had been exposed to those in operation at the hospital.

After the hospital come the tents, huts, and buildings, near A room, which are mostly situated between it and the abrupt edge of the ridge. The case of Private Smithson, who was in one of these tents, has been already noticed. The next which showed itself here was that of Private Patrick Hart, a tailor, who worked and lived in a tent at the south end of the tailors' shop; he was attacked on October 25th, and died on the 31st; yellow, with profuse hæmorrhage from the bowels. The next case there was Mrs. Lindup, who had been in B room until October 21st, and since then in the shoemakers' shop, which is under the same roof with the tailors'; she was attacked on the 28th, had neither yellowness nor hæmorrhage, and recovered. A boy, named Thomas Moore, who lived in the southernmost of the two huts at this point, was attacked on November 3rd; and another, Frederick Davis, who lived in the south end of the barrack store, on the 6th; both these cases were returned as common continued fever, and presented neither yellow surface nor hæmorrhage, but as both had large doses of quinine at first, it is possible the progress of the disease was checked before it arrived at that point.

On 4th November, a rainy and stormy period commenced, which lasted ten days, materially checking the onward progress of the fever. In the beginning of December, however, cases again appeared at different points, and nearly simultaneously.

The first case in this neighbourhood, in December, was that of Edward Bradish, the son of a serjeant, who, with his mother, lived in the hut next to that in which the boy Moore was attacked. Bradish was reported sick on December 6th. On the 7th, he was removed to the race-course with his mother; the following morning she washed him and put on a clean shirt, shortly after which he threw up a quantity of black vomit, and soon expired; he was yellow. The next case in this locality was that of Serjeant-Major Wildbore, who resided in the north half of the staff-serjeants' quarters; he had been living very hard; he was attacked on the 7th, and died on December 12th;

yellow, with black vomit. Mrs. Bradish, the mother of the boy mentioned above, was attacked at the race-course on December 14th; she was yellow, and had hæmorrhage from the gums, but recovered. Her case can be referred with more propriety to this point than to the race-course, where she was when actually attacked. No cases occurred among the persons on the race-course subsequent to the introduction of these two, though there were then many persons there who had not undergone the disease.

The cases in A and B rooms have been already traced up to the time when these were evacuated, and their inmates distributed elsewhere. The next locality to be considered is that including the quartermaster's store, the cells and bakery, which are situated round the top of a ravine to the east of the hospital, and under the range of officers' quarters, where the disease showed itself. The case of Private Leather, which occurred on October 12th, has been already noticed. The next was that of Private Michael McDonald, who was confined in the cells on October 25th, drunk; he had previously been in I room. He was admitted to hospital on the 26th, under the denomination of common continued fever, and was discharged on November 1st, the case not having any symptom of malignity. Private Marriott, a shoemaker, was the next case; he slept in G room from the middle of September, but wrought in a tent at the north end of the quartermaster's store from August 21st; it is therefore impossible to say with certainty to which place his case is referrible; as fever was common in both, though it seems more likely to be attributable to this one. Marriott was attacked on October 28th, and died on November 1st; yellow, with black vomit. On October 29th, Serjeant McGarry, provost-serjeant, reported himself sick; he had resided in the verandah of A room till the 25th, he then slept two nights in the cell in which Private Leather had been when attacked, and afterwards, two nights in a tent on the flat to the east of the cells; he died on November 1st; yellow, with black vomit. Private Lawrence Gordon was attacked the same day as Serjeant McGarry. Gordon had been absent at Middleton on the 19th and 20th, was in the guard-room from the 21st to the 25th, and in the cells from the 25th to the 29th; he died on November 3rd; yellow, with black vomit. The next case was Private Littlewood, a shoemaker, who since August lived and wrought in a marquee close to the quartermaster's store; he was reported sick on October 30th, and died the following day; yellow, with black vomit.

Robert Hines, a sickly child, who resided with his family in the hut to the northward of the quartermaster's store, was the next case; he was attacked on November 3rd, became yellow, but had no hæmorrhage, and recovered. Private Woods, who had been absent without leave on November 4th, and was sent to the cells on the 6th, was the next case; he was attacked on the 8th, became yellow, but had no hæmorrhage, and recovered.

Here, as among the tents and huts at the opposite side of the hill, the disease now ceased for nearly a month, but reappeared on December 4th. The first case was Private James Warren, a bandsman, who had been in the cells for drunkenness and insubordination seven days previous to his attack; and he had previously been in a marquee occupied by the band, below N room; he was yellow, had black vomit, and died on December 8th. Private Edward Jones was next attacked in this neighbourhood; he was employed in the garrison bakery, and had slept there from September 3rd; he was attacked on December 11th, became yellow, had black vomit, and died on the 14th. The next case in this locality was that of Gunner Joseph Hownsome, Royal Artillery, who was attacked on the 11th; he was confined in the cells the previous day, for absence without leave and returning drunk; he was yellow, but had no hæmorrhage, and recovered. The last case referrible to this neighbourhood was that of Private Richard Eckworth, who was employed in the garrison bakery, and slept there until within five nights of his attack, and afterwards in G room; he was reported sick on December 21st, and died on the 23rd; yellow and black vomit was found in his stomach. There is reason to believe that this man had been feverish for some days before he went to hospital.

It has been remarked above, that no cases of fever occurred in C, D, or E rooms, while occupied by the troops. A case, which was returned as common continued fever, appeared on the 17th, in a corporal who lived in a hut to the westward of C room, and on the brow of the slope; but neither his wife nor son was affected. The hospital cook, who cooked in an open shed at the back of the kitchen, near D room, and also on the brow of the slope, was affected with common continued fever from October 25th. Neither case showed any symptoms of malignancy, and both recovered.

The garrison school and library come between E and F rooms. No case arose there; but in a hut to the westward, and at a somewhat lower level, a girl, Margaret Jackson, was attacked on November 6th; the case was returned common fever, it displayed no malignancy, and she recovered. The family consisted of the father, mother, and two other children, none of whom were subsequently affected.

In F room, below the garrison library, there was but one case of fever during the whole course of the disease; it occurred on October 23rd, was returned common continued fever, displayed no feature of malignancy, and recovered.

From C to F room the ridge is so narrow, that there is space for one room only on each terrace; below F, however, it spreads out a little, and admits of two rooms on each, placed end to end. The rooms on the first terrace below F are G and H; the fall being rather abrupt, the scarp behind these is faced with a retaining wall of stone and lime, which is about eight feet behind the back wall of these rooms, and reaches as high as their eaves. This wall has five rows of holes in it for drainage. Immediately to the west of G room a gully commences, which is eaten deeply into the clay soil; and, ninety feet N.W., and on the same level, there is a privy with a cesspool, which is occasionally offensive. There is now a small building to the west of G room—a washhouse, but it was erected after the commencement of the fever, and not used during its progress. The position of the gully above noticed is such that the ascending current of air through it, caused by the sun's heat during the day, is thrown on the end of G room, and into the space between the back walls of it and H and the high retaining wall behind them, circumstances to be borne in mind.

In G room, during the course of the epidemic, there were two cases returned as common continued fever, and six as yellow fever; of the latter, four died. There were, besides, the cases of Marriott and Eckworth, previously mentioned, which might be referred to this room, though the evidence is on the whole stronger for their being placed as they have been above. In H room there were two cases classed as common fever, and three as yellow, two of which were fatal. In I room, in front of G, there were four cases returned yellow fever, but only one death; and in J room, at the end of I, there was not an attack of fever during the course of the epidemic.

The cases of Rowlands, on October 11th, and Hickey, on the 13th, from G room, have been already noticed. The next was Private Wright, who was reported on October 26th, had yellow skin and black vomit, and died on the 28th. This man did not appear to have been on guard since September 28th, nor absent from Newcastle for some considerable time before. After Wright, Private Price was affected on November 4th, had yellowness of surface and black vomit, and died on the 8th; he had been employed on the public works until two days before his seizure. On November 8th there were two attacks in this room; one, Serjeant Aren, was on guard on the 6th, and was out the whole night, in consequence of the guard-tent blowing down during a storm; he became yellow, but had no hæmorrhage. The other, Private Ryan, attended yellow fever cases on October 25th, in hospital, and on November 3rd was confined in the guard-room for absence without leave. Both recovered.

Here, as in the neighbourhood of the hospital, the disease now ceased for a time. On November 11th, as mentioned above, the men from F, G, and H rooms went to the farm, taking their bedding with them, and were succeeded by others from the other rooms. On December 3rd, a case of yellow fever made its appearance among the new men; the subject of it, Private Hearsey, had been employed on wood-cutting fatigue for the week previous to his attack, but had not been away from Newcastle, or the high ground behind it; he died on the 7th; yellow, with black vomit. On December 13th there was a case of common fever in Private Noonan; this man had attended fever cases in hospital on November 4th, and been on guard three days previous to his attack; there was nothing malignant in his case, and he recovered.

In H room, the cases of Fallon and Mills, on September 19th and October 16th respectively, have been already referred to. The next case of fever from this room was Private Joseph White; he had been on the cell-guard on October 27th, and was attacked on the 30th. He became yellow, and had hæmorrhage from the gums, but recovered. Whether in this case the disease should be attributed to the exposure on guard at the cells, or to causes operating in H room, there is no means of discovering. The next case was that of Private Edmund Butler; he was attacked on November 4th, and died on the 12th; yellow, with black vomit. He had attended fever cases in hospital on October 21st, and was on escort duty as far as the Gardens on the day he was attacked. Serjeant Brough was the last case in this room; he was on the main-guard on the 6th, when the guard-tent was blown down, and the men of the guard exposed to the rain. He had to be relieved in the evening, and was sent to hospital at once; he died on the 8th, having become yellow, and having had black vomit.

In I room there were but four cases of fever during the epidemic. These were returned yellow fever, but none of them were either yellow, or had black vomit. One of these, Private McDermott, has been already noticed. The next was Private Lucas, who had been on the main-guard on November 2nd, and employed on wood-cutting fatigue on the 8th. He came to hospital on November 8th, and recovered. In this room, as in the several localities mentioned, the disease ceased from this date until December 3rd, when John Bergen, a drummer, was attacked; he recovered. On December 9th, Private John Rostron was attacked; he was a stone-cutter employed on the public works, and got wet on the 8th; he died on the 15th. This was the last case in this room. Neither of these men had hæmorrhage, or became yellow.

In J room, as already stated, there was no case of fever of any description during the continuance of the epidemic.

In K room there were four cases, three of which were returned as common continued fever, and one only as yellow fever. There was no death. The first case, that of Private Poole, who came under treatment on September 15th, has been already noticed. The next was Private David Bell (not the same whose wife died), who was returned as being affected with yellow fever; he was seized on November 9th, and had neither yellowness of surface nor hæmorrhage, and recovered in a few days; he was employed on the public works. As in other rooms, there were no fresh cases in this from November 9th, for upwards of a month. On December 26th and 29th, two cases of common continued fever showed themselves, but neither displayed any trace of malignancy, and with them the disease terminated here.

In L room, at the end of K, there were in all six cases of fever, and but one death. The two first were ordinary fever; they occurred in September, and have been already referred to. A case of yellow fever, Private Gunning, on October 22nd, has elsewhere been noticed. On the 25th, another man, Private William Price, was attacked; he had been on the main-guard on October 16th, and had cooked for his company in a kitchen on the brow of the slope, to the west of M room, for five days before admission; he had epistaxis, but was neither yellow, nor had any other hæmorrhage; he recovered. Private S. Beard was the next case; he had been on the main-guard on October 25th, reported himself on November 3rd, and died on the 5th; yellow, with black vomit. On November 8th, Private Pemblett was attacked, he had attended fever cases on October 30th; he had yellowness of conjunctivæ, and hæmorrhage from the gums, but recovered. With Pemblett the disease ceased in this room.

M room, on the terrace below K and L, was occupied by the artillery, and they had besides a number of men in tents on the lowest of the three plateaus, below this room. Two cases of fever were admitted on October 15th. One from this room, and the other from one of the tents; from this date until December 17th, there was no other case of fever among them. On that day Bombardier Lee, who lived in M, and had been on the cell-guard on December 12th, was seized; and on the 22nd another case came from one of the tents, who had been on the main-guard on December 8th; both were returned common fever, neither displayed any malignancy, and both recovered.

In N room, at the end of M, there were three cases of fever. The first, Serjeant Matthews, was employed on the public works. He was attacked on November 2nd. The next, Private Johnson, was attacked on the 9th. He had been on the main-guard on the 6th, during the storm. Neither of these displayed symptoms of malignancy, though classed as yellow fever. The next and last case, however, did. This was Private Leadom. He had been on the main-guard on Nov. 15th, and was attacked on the 21st, became yellow, and had black vomit, and died on the 25th.

Immediately below M and N rooms, the road makes a sharp turn in front of them, and below this there are three terraces, which were occupied by tents during the greater part of the epidemic. On the east end of the upper terrace, a marquee was placed; immediately in front of it, on the centre terrace, and about eight or nine feet lower, another marquee was pitched; to the right of this was a hut of wattle and daub, and to the right of that a number of bell-tents. On the other terrace, at a considerably lower level, were the artillery tents. The position of them will be easily understood by referring to the plan.

On October 29th, the band of the 36th, which up to that time had occupied E room, and had not had a single case of fever, were removed to the two marquees above noticed. The ground was somewhat uneven, and the men levelled it by cutting down a portion of the neighbouring clay-bank, and filling up the irregularities. They also cut trenches around the tents, to carry off the water. While doing so, they reached some decaying thatch, a little under the surface, which gave out a very disagreeable odour, lasting for some time. After these occurrences, the first case of fever showed itself in this locality in Private Boyle. He lived in one of the tents to the west of the hut, on the centre terrace. He had been on the main-guard on October 28th, and had been drinking hard. He was attacked on November 2nd, and died on the 8th; yellow, with black vomit.

The next case was that of Private Tuer, of the band, who resided in the upper marquee. He was reported on the 4th, but seems to have been complaining a day or two before, and died on November 6th; yellow, with black vomit. Private Hogan, also of the band, was the next case. He was in the lower marquee. He was reported on the 7th, and died on the 10th; yellow, with black vomit. Private Dove, also of the band, though employed as clerk in the orderly room, was seized on the 7th likewise. He resided in the upper marquee. He was yellow, but had no hæmorrhage, and recovered. Private McCulloch was seized on the 9th. He had been on the main-guard on the 6th, and got wet. In him there was no symptom of malignancy, and he recovered. He lived in one of the tents to the west of the hut.

From November 9th, fever ceased here, as in the other parts of the cantonment, until December 5th, when Private Duff, a bandsman, who lived in one of the marquees, was attacked with ordinary fever, displaying no symptom of the more severe form of the disease. He recovered. On December 6th, the band was removed from this locality, and encamped at the farm along with the other troops there, and one of them, Private Connors, who had resided in the lower marquee, was attacked on the 9th, and died on the 10th; yellow, with black vomit. This man had been drinking very hard for some time, and the previous evening had attended the funeral of Assistant-Surgeon Gordon. Connors' case may possibly have arisen at the farm, but it seems more probable that it was connected with this position.

There were two cases in huts to the east of the cantonment, some way down the slope. These are situated near the top of a gully, but a little to one side of it. The first case from these huts was Serjeant Lane, who lived in the centre hut of those nearest the barracks. He was attacked on November 6th, and died on the 8th; yellow, with black vomit. He had been on the main-guard on October 28th. No other case occurred in this locality until December 3rd, when a woman (Mrs. Holmes) who lived in the hut nearest the farm was attacked. She had yellow surface and black vomit, but recovered. This woman had been in Kingston on the 24th November, and walked back, carrying a large parcel. Mrs. Holmes managed the washing for the hospital, and the personal clothing of the patients sick with fever was sent to her, and washed by black women in the neighbouring brook. On questioning this person as to whether she had counted the foul linen or had it in her house, she stoutly denied both, and stated that she always sent her eldest son (who had never had yellow fever) for it to the hospital, and made him count the articles out to the women who washed them, outside the enclosure, around the hut.* The family in the hut, besides Mrs. Holmes, consisted of the father and three children, none of whom had previously had the disease, and none of whom were subsequently attacked.

On November 11th, two companies which had occupied D, F, G, and H rooms, and several tents, were sent to the farm, a small ridge running off to the southward from the high ground, and nearly parallel to that on which the soldiers' barracks are placed, but lower, and separated from it by a winding ravine. Its position will be readily understood by inspecting the plan. These companies took their bedding with them from the rooms they had occupied before they moved.

The first case of fever which occurred here was on November 22nd; it was returned as common fever, displayed no symptom of malignancy, and recovered. Here, as elsewhere, the operation of the causes of fever seems to have been suspended or modified in some way, until the first week of December, when another case of common fever presented itself on the 6th. On the 11th, another case was reported, which was returned yellow fever. The patient, Private McGowan, became yellow, but had no hæmorrhage. The same day, another case, also denominated yellow fever, in Private Field, was reported. He was neither yellow, nor had any hæmorrhage; he had been drinking very hard for some time before he was attacked. The last case, Private McDougal, was attacked on December 14th. He had been absent at Kingston without leave till December 3d. His case was ordinary fever, and there was no symptom of malignancy in it. All these cases recovered.

During the progress of the epidemic, men were sent from barracks to attend those sick of fever, in addition to the regular orderlies. These men went on duty about ten A.M., and remained in attendance on the fever cases in the fever ward, rendering them all the assistance they required, until the following day at the same hour. There were 156 men so employed; of these, forty-six were on similar duty a second time, seven a third time, and one a fourth time. Three of these men had had common continued fever shortly before they were so employed, and one yellow fever. Some of them may have had yellow fever before, but from all that could be earned, very few only were ever affected with this disease. During the epidemic only eight of these fatigue-men were affected subsequent to being engaged near those sick with fever. They were as follows:—

Names.	Last attendance on fever cases terminated.	Date of Attack.	No. of days elapsed between attendance and attack.	Forms of disease.	Room.
J. Needham	Aug. 29	Died Nov. 26	92 (died)	Dysentery, with black vomit in stomach.	Died in E room; came from race-course.
Edm. Butler	Oct. 22	Attacked Nov. 4	13 (died)	Feb. icterodes	H.
Jas. Pemblett	Oct. 31	" Nov. 8	8	Ditto.	L.
Patrick Ryan	Nov. 2	" Nov. 8	6	Ditto.	G.
George Lucas	Nov. 3	" Nov. 8	5	Ditto.	I.
Jos. Hearsey	Nov. 10	" Dec. 3	23 (died)	Ditto.	G.
Mic. Noonan	Nov. 5	" Dec. 13	38	Feb. c. c.	G.
Jas. Higgins	Nov. 30	" Dec. 26	26	Ditto.	K.

Taking these cases as they stand, the period which elapsed between the exposure and seizure, in the first ninety-two days, was far beyond that in which contagion usually operates. The man came to hospital with dysentery on November 21st, and died on the 26th, having black vomit on the stomach, and he was never in D room, which was the fever ward, but was treated in E room, which was used as a convalescent ward for fever cases, and in which there were none who were not convalescent at the time, and moving about. Under these circumstances, it does not seem reasonable to refer the supervention of symptoms of yellow fever on those of dysentery to the action of specific contagion after his admission to hospital.

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* Such is the statement Mrs. Holmes made to me, but I do not believe it; for on asking her the reason for adopting the precaution of keeping the soiled linen outside, she said she was afraid of infection; but on being asked why, if she feared that, she exposed her son to it, she could not give any satisfactory explanation. That the boy was much employed in the manner stated there is no doubt, but I question whether he was exclusively so.

In the second case in the above list (Private Butler), thirteen days elapsed between the last exposure in the fever wards and the attack, a period not incompatible with the action of specific contagion, were it existing. The subject of this case came from H room, and on referring to the list of attacks in that room, it will be seen that another man there was attacked on the 30th October, five days before Butler, though it cannot be positively asserted whether he got the disease in the room, or from exposure on guard at the cells, and another man was attacked two days after Butler, the immediate exciting cause being exposure to rain, on guard, the same day. Butler had been on escort duty as far as the Gardens, on the morning of the day on which he was attacked. This man, therefore, had been exposed to specific contagion if it existed, and likewise to the action of the causes of disease in operation in the hospital before it was removed, within a sufficiently recent period; he was exposed also to the causes of disease in and around H room, and to the air of the lower ground; but the men who were attacked on the 30th October and 6th November in the same room, were exposed to neither the first nor last, so far as can be ascertained. The weight of this evidence, therefore, is in favour of some local cause.

The third case in the list is that of Pemblett, who contracted fever eight days after having attended sick. He came from L room, and was attacked on November 8th. In this room, too, cases had preceded Pemblett's, in men who had not been in contact with sick, or away from Newcastle, though they had been exposed on guard or otherwise within a short period of their attack, and it is unreasonable to conclude that his attack could have proceeded from contagion, while there is no ground for assuming that those who preceded him arose in this way.

The next of these cases was that of Private Patrick Ryan, who became sick on November 8th, having been in contact with fever cases last on the 2nd. This man lived in G room, from which several fatal, distinct cases of yellow fever had come within the preceding four weeks, none of whom had attended fever cases or been particularly exposed to them. Ryan was confined in the guard-room on November 3rd, it is believed for absence and drunkenness. His case did not present any of the more characteristic symptoms of yellow fever, and he recovered. In the face of such evidence, no one can attribute this case to personal communication with the sick.

The next of these cases, that of Private G. Lucas—the last exposure was on November 3rd, and he was attacked on the 8th. This man lived in I room; he was neither yellow nor had any hæmorrhage, and recovered. He had been employed on wood-cutting fatigue on the day of admission, an employment involving considerable exertion and exposure. A similar case had occurred in the same room, on October 21st, in a man who had not been in attendance on sick; and two others occurred subsequently; so that here, as elsewhere, the attendance on fever cases seems to have been the accidental circumstance, and not the essential one in the production of the disease.

The case of Private Hearsey is the next. This man lived in G room, where, as already stated, there had previously been much fever. His last exposure in attendance on fever cases was on November 10th, and he was attacked on December 3rd, the interval being twenty-three days. This man died on December 18th; yellow, with black vomit. He had been on wood-cutting fatigue for the week previous to his attack, and was therefore undergoing considerable exertion and exposure. The date of the attack, too, is the same as that on which several other cases occurred in other parts of the cantonment, who had never attended fever cases, nor, so far as is known, were exposed to contagion. Here again, it is not only impossible to refer to contagion as the cause of the disease, but the whole weight of the evidence is against that view of the case.

The next of these men who attended fever cases who were attacked was Private Noonan, who resided in the same room with Hearsey. Noonan's last exposure in the fever wards was on November 5th, and he became sick on December 13th, or thirty-eight days after exposure in this way. Noonan had been on the main-guard on December 10th. This case did not present any trace of malignancy, and he recovered.

The last of these men who was attacked was Private James Higgins. This man lived in K room, and was in attendance in the fever wards to the morning of November 30th; he became sick on December 26th, twenty-six days after the exposure; his case was returned Febris C. C., and presented no trace of yellow fever; he recovered. Higgins was employed on the public works previous to his attack.

These facts have a very important bearing on the question of the propagation of the fever. They show that out of 156 men, taken indiscriminately from the different rooms, very few of whom could have had yellow fever before, and who afford 210 instances of exposure for twenty-four hours to the emanations from the sick in the fever wards, that only 8 were subsequently affected with fever of any sort, of whom 3 died; while, from the remainder of the troops in the cantonment, amounting, at the commencement of the epidemic, to 523, there were 89 attacked with fever, of whom 38 died. Putting those numbers into the form of a centesimal ratio for the sake of comparison, they stand:

	Total strength.	Attacked per cent.	Died per cent.	Died per cent. of attacked.
Men *who attended* fever cases	156	5·1	1·9	38
Men *who did not attend* fever	523	17·0	7·3	43

These numbers most fully warrant the conclusion that exposure to the effluvia from the sick was not an active cause in propagating the yellow fever at Newcastle in 1856; and if it be considered that of the eight men who attended on sick and were subsequently themselves attacked, four presented none of the prominent characters of yellow fever, while of the four who did, three came from rooms in which persons had previously been attacked with decided yellow fever, without communication with sick, so far as is known, there is no alternative but to limit the conclusion in these cases still more, by excluding the operation of specific contagion altogether.

The following conclusions seem fairly deducible from the preceding details. To render them clearer, the principal facts have been appended:

1st. That yellow fever prevailed at Newcastle, in 1856, in well defined zones, alternating with others which presented a much smaller amount, and, for the most part, a different form of fever, attended with a much smaller mortality. These zones embrace:

	Cases.	Deaths.
A. **Officers' quarters above mess-room and race-course encampment (including last two cases at race-course, the others being referrible to A room)**	2	0
B. The buildings between the mess-room and parade-ground, including officers' quarters, hospital, A and B rooms, bakery, cells, and all tents and huts in the neighbourhood (excluding first six cases in hospital as referrible to low ground, and the last as indeterminate)	60*	31*
C. Rooms C to F inclusive	4	0
D. Rooms G and H inclusive	13	6
E. Rooms I to N inclusive	19	3
F. Tents on two upper plateaus below M and N	7	4
G. Artillery tents on lowest plateau	2	0
H. Huts near graveyard	2	1
I. Farm encampment	5	0

In consequence of the frequent changes which took place, it would be very difficult to ascertain the mean number exposed in these localities; and the persons being exposed at different periods of the epidemic, the resulting ratios of attacks and deaths would not be strictly comparable. The rooms from F to N, however, were occupied during the whole course of the disease by soldiers, and as each room contained on the average thirty-four men, the comparative progress of the fever in these may be determined with tolerable accuracy.

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* If the cases which occurred in the officers' quarters be set aside, as of doubtful origin, these numbers will be, cases 54, deaths 28, presenting exactly the same characters for a more limited locality.

				Per-centage of strength.		Per-centage of deaths
Rooms.	Strength.	Attacks.	Deaths.	Attacked.	Dead.	on attacks.
F	34	1	0	3·0	0·0	0·0
G and H	68	13	6	19·1	8·9	46·1
I to N	204	19	3	9·3	1·5	15·8

These ratios show conclusively that the causes of fever operated with much greater intensity in some localities than in others, and that these unhealthy spots were distinctly circumscribed. It is questionable how far the three deaths in the rooms from I to N were due to these localities, one of the men having been permanently engaged on the public works, and the other two having been on the main-guard (which brought them into an unhealthy locality), one nine days and the other six days, before his attack; were these cases produced by exposure on guard, the healthiness of these rooms would appear still greater.

2nd. That bodies of men moving from an unhealthy to a healthy locality soon lost the disease, though they carried most of their bedding and their clothing with them; and, in the healthy spots, did not communicate the disease to others who were from other healthy spots.

The light company moved from A and C rooms to the race-course, about October 24th; four of them, from A room, were attacked up to the 30th (one of them decided yellow fever), but no man from C room was attacked. On Dec. 7th, a woman and child were removed from near A room to the race-course; the child had decided yellow fever, and a week after the woman was attacked, but the disease stopped with them.

After the removal of the hospital to C and D rooms, no patient except Needham, or permanent hospital attendant except the cook, who had not been in the regular hospital, was attacked with fever. Previous to this removal, and immediately after it, several hospital attendants and patients suffered from the disease in its most aggravated form, but all had been exposed to the morbific influences in the first situation.

The married people and their families were removed from B room on October 21st, and distributed in various places. Three cases occurred within the next two days, but then the disease ceased among them, except in one woman, who went to the shoemakers' shop to reside—itself an unhealthy locality.

On November 11th, two companies were removed from F, G, and H rooms to the farm, and carried most of their bedding with them, yet the aggravated form of fever did not continue among them.

3rd. Persons going from healthy to unhealthy localities to reside, caught the disease.

The Band removed from E room (where they had been quite healthy) on October 29th, to the upper two plateaus below M and N rooms, where fever appeared among them; and there are many instances in which people went to the cells, or went to the hospital, from healthy localities detailed above, in which they were soon after affected with the disease in an aggravated form. An argument will undoubtedly be advanced here by some, that the Band having occupied E room previous to removal, were within reach of the contagion from D room, then the fever ward. This is met by the fact that the first case in this locality was that of Private Boyle, who lived here before the arrival of the Band, and did not belong to it, and had not been near the sick, so far as is known. Boyle came sick on November 2nd, and Tuer, the first of the Band, on the 4th, though he seems to have been unwell a day or two before. Both died of decided yellow fever. The fact stated above, too, of those who actually attended the sick suffering much less than those who did not, is adverse to the idea of the Band having been affected by contagion previous to removal.

After the removal of the men from F, G, and H rooms, on November 11th, F and G were occupied by men who, up to that time, had been in I and F rooms; and fever appeared among those in G in December.

4th. The activity of the causes of the fever seems to have been much diminished by a course of wet weather commencing on November 4th, accompanied by a severe storm, and heavy rain on the 6th and 7th; and yellow fever disappeared after Dec. 21st, on the accession of strong cool northerly winds.

The admissions from fever of every description in November and December were as follows:

November 2nd	3	December 3rd	3
“ 3rd	3	“ 4th	1
“ 4th	3	“ 5th	1
“ 5th	0	“ 6th	2
“ 6th	4	“ 7th	2
“ 7th	2	“ 9th	2
“ 8th	5	“ 11th	4
“ 9th	4	“ 13th	1
“ 10th to 20th	none	“ 14th	2
“ 21st	1	“ 17th	1
“ 22nd	1	“ 21st	1*
“ 26th	2	“ 22nd	1
“ 28th	3	“ 26th	1
		“ 29th	1

5th. The disease showed itself in its most malignant form in persons who had not been away from Newcastle for many months, and who were not exposed to others previously affected with it, or to their clothes, or other media usually considered as likely to convey contagion.

The cases of Private Monk in A room, on September 21st, and of Mrs. Bell in B room, on the 22nd; and again, Private Leather, on October 12th, from the cells, are clear instances of this.

An additional proof of the possibility of such an occurrence is given by the appearance and progress of the fever in the family of the schoolmaster-serjeant of the 97th, as recorded by Dr. McIlree, in 1848, noticed above.

6th. Persons in contact with sick in a healthy locality did not contract the disease more frequently, or indeed as frequently, as those in barracks.

The list of the men who attended the sick of fever in hospital shows that of 156 who were so employed, giving 210 separate instances of exposure for twenty-four hours to the emanations from the sick in the fever wards, only 8 were afterwards attacked with fever—a smaller proportion than among those in barracks, though their exposure to contagion, were it existing, was of course infinitely greater.

Further no hospital attendant, or patient, except Needham, who had not been in the hospital building, contracted fever after the sick were removed from it and the surrounding influences; though, had the contagion been the exciting cause of the disease, there was as much or even more reason for the attendants becoming affected after the removal than before it; 32 of the 41 deaths of soldiers having occurred after the removal, and the ventilation of the rooms not being better than that of the hospital.

Taking these facts together—and in this argument they must be taken together, unless they can be shown to be unfounded—they appear utterly opposed to the view that the disease either arose from, or was propagated by, specific contagion.

That a cantonment of about 800 yards in length, on a narrow descending mountain ridge, should present four well-defined healthy zones, alternating with three others in which a disease supposed to be contagious prevailed, while the communication from one extremity to the other was free and unrestrained (save with those actually sick in hospital), is contrary to all experience, and of itself, were there no other evidence, would go far to overthrow the idea of specific contagion having acted; but with the additional weight of the evidence adduced above, this position seems quite untenable, and there is no alternative but to look for the explanation of such circumscribed effects to the influence of causes equally local in their operation.

The first unhealthy zone, noticed above, embraces the buildings between the mess-room and the parade ground; by referring to the projection it will be seen that these follow the course of the ridge, pass on to the hospital, B room of the men's quarters, quartermaster's store, bakery, and cells, and form a sort of crescent round the head of a deep abrupt gully, which at its upper part runs nearly east and west. The valley may be said to have a southerly exposure, as the bounding height to the north is much higher than that to the south, a point that will be hereafter alluded to. On the western side of this zone, A room, the tailors' and shoemakers' shop, and some huts are situated; these are immediately over the upper extremity of a watercourse, and a considerable gully, formed by a land-slip; both the watercourse and gully having a southwesterly exposure, and descending at an angle of about 40° below the horizon.

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* The last case of yellow fever.

The next unhealthy zone comprises G and H rooms. As mentioned above, there is a high retaining wall supporting the bank behind these rooms, about eight feet from them, and as high as the eaves; about thirty feet from this space, and directly in a line with it, a gully commences, which drains that part of the barracks, and is so placed with regard to G room as to throw the current of air which ascends through it on the end of that room, and into the space between it and the wall at its back.

The last situation where the disease showed itself severely was on the upper two plateaus below the barracks, where it appeared after the disturbance of the ground in levelling, and the exposure of some decaying vegetable matter from cutting trenches to carry off the surface water. There is no gully or watercourse near these spots, such as those above mentioned.

There is a large gully on the east side of the barracks, beside the grave-yard, but it is wider than those just referred to; it is less steep, and from the form of the ground at the top, the ascending currents of air through it are not directed immediately on any of the buildings. That it was not perfectly innocuous may be inferred from the occurrence of two cases of yellow fever (one of which proved fatal) in persons living in the huts on its northern side. At all other points where the men remained healthy, though the ground of the ridge occupied by the buildings was narrow, the ground on either side of it sloped away gradually for a little distance before terminating in the steep descent of the mountain side; and there were no gullies or ravines opening near the houses, or the natural undulations of the ground had more or less of a northern exposure.

The valley terminating to the east of the hospital had its bottom and sides covered with the thick bush usually seen in such localities; this grew over its whole extent, from the lowest point in the plan almost to the road under the quartermaster's store; between this road and the store there was a quantity of old thatch, which had been pulled off the roof of the store, and thrown down there in June, 1856. This had been covered with earth, and it was not until going over the ground early in 1857, that, on examining the ruts cut in it by the water, the straw became apparent, and led to further examination, when it was found extending over an area of some thirty or forty yards.

The gullies to the west of the hill did not contain any bush; but there was a small quantity of open bush beyond them, on the side of the hill. A quantity of refuse—such as bones, ashes, pieces of cloth, &c.—had gradually accumulated over the slope and about the outlets. This was cleared away in the middle of October, and the men employed on the duty complained of the unpleasant odours evolved, but, unfortunately, it was not remarked whether any of them were subsequently attacked with fever. The large gully near the grave-yard, besides having a more gradual slope than the others, is almost completely clear of bush; its bottom and sides presenting the bare red clay, with little or no vegetation.

All these hollows have a southerly exposure; and, during the prevalence of the fever, the sun passing to the south of the zenith, shone into them some portion of the day, causing ascending currents of air through them, which inpringed on the buildings around their upper outlets. It has been shown that in these buildings the disease was more prevalent and fatal than in others; and a very short removal from the course of the current seemed to give almost complete immunity from the disease.*

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* In January, 1857, there was an excellent illustration of the influence of the form of ground in determining the direction taken by the ascending current through the valley to the east of the hospital. Some of the thatch alluded to above was being burnt in the hollow in front of the quartermaster's store, and it gave out a pungent ammoniacal odour. There was a north-easterly wind blowing fresh at the moment, and the odour from the burning thatch was quite strong under M room. When there I stated to Drs. Foss and Jopp that, from the form of the ground, I anticipated we should find the odour quite perceptible on the hill above the hospital, though to the north of the point where the fire was, and 200 feet above it, and on proceeding to the rooms C and D of the officers' quarters, we found the odour very powerful.

The privies on the west side of the hill were frequently offensive during the progress of the epidemic. The emanations from privies have been referred to as exciting causes of fever elsewhere; and the occurrences at Newcastle, in 1848, show they are not innocuous here; but they do not seem to have exercised any marked influence during the late epidemic, for rooms D and E, and I and K, which are near privies, were almost free from fever; and while G suffered severely, F, which is nearer the privy, escaped.

The meteorological phenomena were not observed during the progress of the epidemic with as much minuteness as would have been desirable. The thermometer was registered daily at six A.M., two P.M., and six P.M.; but, unfortunately, the instrument was kept in the surgery, and its indications were much affected by the temperature of the room—standing higher in the morning, and lower at two P.M., than it would have done if properly exposed. On October 23rd, when the hospital was evacuated, the thermometer was placed in a small house, jalousied all round, through which the air could pass free at all times, and there was an immediate and considerable change. The mean temperature as indicated by the thermometer was:

	Six A.M.	Two P.M.	Six P.M.	Mean.
July . . .	67·5° . . .	74·2° . . .	72·5° . . .	70·8°
August . .	67·1 . . .	73·8 . . .	72·0 . . .	70·5
September .	67·4 . . .	73·7 . . .	71·8 . . .	70·6
October .	65·3 . . .	72·7 . . .	70·1 . . .	69·0
November .	62·2 . . .	69·2 . . .	64·2 . . .	65·7
December .	60·1 . . .	68·8 . . .	63·5 . . .	64·5[*]

This table shows that the often repeated opinion, that the causes of yellow fever could not exist unless where the mean summer temperature reached 80°, is erroneous, the disease having prevailed, as an epidemic, at Newcastle, with a mean temperature 10° lower, and continued until the mean temperature had fallen 5° more.

More heat did not seem sufficient to call the causes of the disease into operation; for in July and August there was none, and, though a few cases occurred in September, it did not attain its greatest force until October and November, when the temperature was diminishing. It ultimately disappeared about December 21st, when cool weather came on, accompanied with strong northerly winds and some rain. It will be remembered that the disease stopped about November 10th, that a few cases occurred in the latter part of that month, and that early in December they were more numerous, but that the last was on the 21st of that month. Taking the mean temperature for periods of ten days in each month, they are—

	Six A.M.	Two P.M.	Six P.M.	Mean.
November 1st to 10th	62·5° . . .	70·2° . . .	67·0° . . .	66.4°
November 11th to 20th	62·7 . . .	68·8 . . .	62·9 . . .	65·7
November 21st to 20th	61·5 . . .	68·5 . . .	62·7 . . .	65·0
December 1st to 10th	61·2 . . .	70·0 . . .	65·8 . . .	65·6
December 11th to 20th	61·2 . . .	70·7 . . .	63·3 . . .	66·0
December 21st to 31st	58·2 . . .	66·0 . . .	61·5 . . .	62·1

The periods of aggravation of the disease were thus coincident with increased temperature during the day; such increase, however, was accompanied by a clearer sky and stiller state of the air than when the mid-day temperature was less. There were unfor-

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[*] Since April, 1857, meteorological observations have been made at Newcastle more systematically. The results for July to December, 1858, are given below; they agree pretty closely with those for 1856 as to temperature and distribution of rain, while in 1858 there was no serious disease:—

1858.	Temperature			Mean monthly dew point.	Rain in inches.
	Mean minimum.	Mean maximum.	Mean of month.		
July ..	.. 64·3°	74.8°	68·0°	60·9°	4.11
August	.. 64·4	74·8	68·5	60·9	6·42
September	.. 65·0	74·8	68·6	61·8	11·10
October	.. 63·7	73·1	67·9	61·5	12·12
November	.. 62·8	72·3	66·0	61·0	15·24
December	.. 60·8	70·7	65·2	59·1	5·15

tunately no observations of the absolute maximum temperature of the day, or of the amount of the sun's radiation.

The dew point was not observed during the course of the epidemic, but the quantity of rain collected was as follows:

Months.	Inches.	Wet days.	Remarks.
July	0·00	0	
August	5·52	12	Well distributed.
September	5·27	12	Ditto.
October	8·37	5	On 5th, 12th, 15th, 16th, & 17th.
November	17·30	14	Between 4th & 13th, 12·3in. " 18th & 21st, 5·0in.
December	5·00	5	On 17th, 22nd, 24th, 26th, & 28th.

From this table it appears that the occurrence of the disease was preceded by a moderate quantity of rain; and its cessation, in November, soon took place under the influence of the heavy rains which commenced on the 4th. The causes of the fever, however, do not seem to have been altogether removed by the rain, but their action merely suspended, or rendered less intense for a period, but soon resuming their force with a return to dry and warm weather. Thus, after the cessation on November 10th, the rains went on to the 14th, and on the 15th there was fine weather, which was terminated by heavy rain on the 18th, continuing to the 21st inclusive. A case of yellow fever occurred on the 21st, which proved fatal. A man died in the hospital with black vomit on the 26th; and other cases appeared in the officers' quarters above the hospital, though it was not before December 3rd they again showed themselves in barracks.

This fact of the suspension of yellow fever under the influence of heavy rain is one of much importance; it has been observed in every epidemic of yellow fever at Sierra Leone; and while cases of the yellow form of the disease, in its most malignant character, have shown themselves in the breaks of the rains, or at their termination, the disease which appeared during the continued heavy rain was always pure remittent fever, and of that there was generally no scarcity. Can this be explained on the assumption of yellow fever being propagated by specific contagion? It seems much more in accordance with fact to conclude, that the origin of yellow fever is intimately connected with (though not altogether dependent on) some local emanation, the production of which is either suspended or modified by heavy rain, but which, on a return to dry weather, may again be produced with its former properties.

The above facts with reference to Newcastle seem to leave open no other conclusion, than that the yellow fever there in 1856 arose from local causes. Whether similar causes were in operation there in other years, and if so, why they did not lead to a similar result, are questions that the present information on the subject does not admit of being answered. It would seem, however, that in addition to the ordinary local causes of disease, an epidemic constitution is necessary to account for the prevalence of fever.

Much difference of opinion seems to exist as to the nature of an epidemic constitution; some limiting its influence to a comparatively circumscribed locality; others claim for it a more extended operation, but assert at the same time that its effects should be manifested by the same form of disease in all places within its sphere of action. Both views seem the result of overstrained deductions from too limited observations; as there is reason to believe that a more extended investigation would show that an epidemic constitution influences mortality from all forms of disease, and that its operation may be traced nearly contemporaneously from Hindostan to Mexico, and from Lapland to the Cape of Good Hope.

It will be sufficient, to prove the existence of an epidemic constitution in the present instance, to state that during the summer of 1856 yellow fever prevailed pretty extensively in the West Indies and around the Gulf of Mexico; and was therefore sufficiently general to warrant the conclusion of there having been something in operation beyond mere local influences.

While engaged on this paper I learned that some of the medical officers at Newcastle were of opinion that yellow fever was imported there from Stony Hill. With the view of getting every information on this point, I called for the opinions of Staff-Surgeon Foss, Surgeon Jopp, and Assistant-Surgeon Tobin, 36th Regiment. The first and last have expressed their belief that it arose from local causes, and was not imported. Dr. Jopp thought it had been imported, and his reasons for that opinion are contained in his official 'Report of the Newcastle Epidemic of 1856.'

[We are indebted for Mr. Lawson's able and interesting account of this epidemic to the courtesy of Mr. Alexander, Director-General of the Medical Department of the Army. Independently of the merits of the communication, a record of an outbreak of yellow fever at a station about 4000 feet above the sea level—an elevation considered, until of late years, to bestow an immunity from this scourge of hot climates—must be extremely interesting, more especially to those engaged in the investigation of climatological pathology. We are sorry that we cannot afford space for much valuable matter contained in the appendices to Mr. Lawson's paper. Among other subjects of interest, we more particularly allude to the Reports on the Epidemic by Staff-Surgeon Foss, Assistant-Surgeon Tobin, and Dr. Jopp, Surgeon to the 36th Regiment. The two former gentlemen briefly state their opinion that the disease at Newcastle in 1856 was of local origin, and not imported into that station; while Dr. Jopp, in a lengthened communication, adopts the view that the fever first appeared in the low lands, and was subsequently propagated to Newcastle by contagion.]

Art. II.

Remarks on Anæsthesia, and the Agents employed to produce it. By George Hayward, M.D., late Professor of Surgery in the Massachusetts Medical College, Boston, U.S.A.

The discovery by which surgical operations can be rendered painless is one of the greatest connected with our profession, second only to that of vaccination. It is a blessing to the human family that cannot be overrated; and having been among the first to make a successful use of it in surgical practice, I thought that a brief sketch of the history of anæsthesia, and some remarks on the comparative value of the agents employed to produce it, would not perhaps be uninteresting.

It was my fortune to perform the first capital operation on a patient rendered insensible by the inhalation of sulphuric ether. This was done on November 7th, 1846, at the Massachusetts General Hospital, Boston. On September 30th preceding, Dr. Morton, a dentist, administered it to a man from whom he extracted a tooth, without causing pain. Almost immediately after, he requested the late Dr. John C. Warren, who was at that time the acting surgeon at the hospital, to use it at that institution. Dr. Warren consented. It was inhaled by a patient, with partial success, on whom Dr. Warren operated on October 16th. The operation was the removal of a nævus from the face. On the day following I extirpated a large fatty tumour from the arm of a female, who was made wholly unconscious and insensible by the inhalation of the ether. The operation lasted seven minutes.

At that time Dr. Morton was, I thought, the only person who knew what the anæsthetic agent was. On November 1st I took charge of the surgical department of the hospital, and in a day or two after Dr. Morton asked me if I were willing to allow him to administer his "composition," as he called it, to a female whose limb I was about to remove above the knee. I told him I would not, unless I knew what the article was, and felt confident of the entire safety of its administration. He at once told me that it was rectified sulphuric ether. He allowed me to communicate this to my colleagues, with an understanding that it should not be made known publicly, until he had obtained a patent, for which he had already applied. On the following day the operation was performed, in the presence of more than two hundred spectators.

It rarely falls to the lot of a professional man to be the witness of a scene of more intense interest. The operating room was crowded. Many were obliged to stand. Besides the class of students in attendance on the lectures, numbering more than a hundred, and many of the principal physicians and surgeons of the city and neighbourhood, there were present several clergymen, lawyers, and other individuals from the various callings of life. When I entered the theatre, before the patient was brought in, I found it, to my surprise, filled in every part, except the floor on which the table stood, with persons on whose countenances was depicted the almost painful anxiety with which they awaited the result of the experiment they were about to witness. I simply told them that I had decided, with the advice of my colleagues, to allow the patient, on whom I was to operate, to inhale an article which was said to have the power of annulling pain. The patient was then brought in. She was a delicate-looking girl of about twenty years of age, who had suffered a long time from a scrofulous disease of the knee-joint. It had at length suppurated; there were extensive openings into the cavity of the joint; the cartilages were ulcerated and partly absorbed; the bones carious, and symptoms of hectic fever had already made their appearance. As soon as she was well arranged on the table, I told her that I should let her breathe something which, I hoped, would prevent her from suffering much from the operation, and that she need not be afraid of breathing it freely.

As the ether was at the time administered by means of a large and clumsy instrument, which required to some extent the co-operation of the patient, it was desirable that the amputation should be done as rapidly as possible. Everything, therefore, was arranged with this view. I decided to perform the flap operation. One person was to compress the artery, another to withdraw the flaps, a third to hand the instruments, and a fourth to watch the pulse. I grasped the patient's limb with my left hand, and held the amputating knife behind me in my right, carefully concealed from her view. The mouthpiece of the inhaling instrument was then put into her mouth, and she was directed to take long inspirations. After breathing in this way a short time, the nostrils were compressed, so that all the air that went into the lungs must first pass through the machine, and of course be mixed with the vapour of the ether. She breathed with perfect ease and without struggling, and in about three minutes from the time the instrument was put into her mouth, Dr. Morton said, "She is ready." A death-like silence reigned in the room; no one moved or hardly breathed. I passed the knife directly through the limb, and brought it out as rapidly as I could, and made the upper flap. The patient gave no sign of feeling or consciousness, but looked like one in a deep quiet sleep. Every other person in the room took a full inspiration that was distinctly audible, and seemed to feel that they could now breathe again. The second flap was then made, the bone sawed, five arteries were tied, and as I was tightening the ligature upon the sixth and last, she groaned, being the first indication of sensibility that had been given. Nothing more was done than to bring the flaps together, cover the stump with cloths dipped in cold water, and apply two or three turns of a roller to keep them in place. Her consciousness soon returned; she was wholly ignorant that the operation had been done. For some time she would not believe it, and said that she had felt nothing till I tied the last artery. The operation lasted a minute and three-quarters, not including the time required to tie the arteries. I did it rapidly, though it has been done in less time, because I feared that the insensibility might pass off, and we had no means then, as we have now, of continuing it as long as is necessary.

Patients who have inhaled ether, when its effects are at first passing off, are usually bewildered, not easily contented, and by no means inclined to do as they are desired. It would be almost impossible to persuade one of them at such a time to breathe through the instrument that was then in use. At present, fortunately, we can keep up the state of anæsthesia as long as we wish, by administering the agent employed for this purpose by means of a sponge. This simple contrivance was first used at the Massachusetts Hospital.

The patient whose case I have just spoken of recovered rapidly from the operation, was in good health when I left home eleven years after, and I have no reason to suppose that she is not so at the present time.

It will be readily believed that a result so successful, and witnessed by so many intelligent persons, made it impossible to doubt the anæsthetic power of the agent employed, and what this was very soon became known. In an almost incredibly short space of time, numerous operations were performed on persons rendered insensible by the inhalation of ether in various parts of the United States and Europe, and there is hardly a country in Christendom in which it has not been thus used to a greater or less extent.

The Anæsthetic Agents.—These are sulphuric ether, chloroform, chloric ether, and amylene. The two latter are now rarely used for this purpose, and probably never will be again. Chloric ether is simply a tincture of chloroform. There are two kinds, one the concentrated and the other the chloric ether of commerce. The first is composed of one part of chloroform to nine of alcohol; and in the other, there is one part of chloroform to fifteen of alcohol. It can be prepared by mixing the two ingredients of which it is composed in the proper proportions, and if the alcohol which it contains is evaporated, nothing but chloroform remains. It is evident that it derives its anæsthetic properties from the chloroform, and it is therefore as unsafe as that article; for the alcohol, though it renders it less efficacious, does not make it more harmless.

Amylene, the chemical elements of which are equal parts of carbon and hydrogen, has caused death in several instances. There have been so many fatal cases in proportion to the number in which it has been exhibited, that no one hereafter will probably be sufficiently reckless to use it.

Chloroform was first employed by Professor Simpson, of Edinburgh, who thought that it possessed "various important advantages" over sulphuric ether. He said that it was more portable, more agreeable to inhale, less exciting, and that it gave a greater control over the patient. That it is more portable and more agreeable to inhale, I admit, but that it is less exciting and a more efficient anæsthetic agent, I deny. But the principal objection to it is, that its inhalation sometimes causes death. Its advocates admit that this has occurred in sixty cases, while others believe that there has already been double this number. But be the number what it may, so many have died from its inhalation, that many persons are in favour of abandoning its use altogether. Death produced by it cannot now be attributed in every instance, as it was at first, to the impurity of the article, or to the exhibition of too large an amount, or to the want of skill or judgment in the administrator. There have been several fatal cases recently, where the chloroform was said to have been of the purest character, and a small quantity only inhaled, and this, too, in the presence and under the direction of intelligent, well-educated, and careful men.

The truth is, that chloroform when inhaled acts on the system in a way that is not yet well understood, and may destroy life in spite of the utmost caution. Its effects are sometimes so sudden, that no foresight can prevent a fatal result. Unless some means, therefore, can be discovered that will render its inhalation safe, common prudence and a regard for human life would seem to dictate that it should be no longer used in this way. It is true that the state of unconscious insensibility produced by it is a blessing of countless value to those who are to undergo severe surgical operations, not only by rendering them painless, but at the same time disarming them of their terror. And these are not the only advantages of anæsthesia. It in great measure prevents the shock to the nervous system which not unfrequently defeats the skill of the most expert surgeon, it enables him to operate more deliberately, removes all necessity for haste, which is often the result of the sufferings of the patient, and makes the performance of some operations comparatively easy, which in the ordinary state of the system could hardly be done at all. It is not therefore to be wondered at that professional men are reluctant to abandon the use of chloroform, and their unwillingness might be excused if there were not a substitute equally efficacious, as easily administered, and entirely safe. That rectified sulphuric ether is such a one I have no doubt. I have witnessed its effects on several hundred patients upon whom severe surgical operations were performed, and all of them were rendered motionless, unconscious, and insensible.

In no instance was there any alarming or serious consequence. It does not act as speedily, perhaps, as chloroform, but in no case were more than eight minutes required to produce complete anæsthesia. It can be effected in much less time when atmospheric air is not allowed to mix freely with the vapour of the ether. This is the method pursued in the hospital at Naples, where no other anæsthetic agent is used; and I saw a patient undergo a severe surgical operation there without the slightest suffering, who was brought into this state by inhaling the ether only a minute and a third! But when administered thus rapidly, it is apt to produce a distressing cough and sense of suffocation for a moment, and there might be some reason to fear asphyxia from the exclusion to too great an extent of atmospheric air. Professor Polasciano, however, told me that he always gave it in this way, and had never seen any more troublesome symptoms than those I had witnessed in the case just alluded to. These, though distressing to the patient, were of short continuance, and by no means alarming.

There is no doubt in my mind that sulphuric ether should be used as an anæsthetic agent to the entire exclusion of chloroform. It is as efficacious, and I should say without hesitation, after having seen chloroform administered by others in many cases, that ether produces a more complete state of unconscious insensibility. Its effects pass off sooner, and less vomiting, nausea, and headache follow its inhalation. It is as easily administered. All that is required for its administration is a bell-shaped sponge with a concavity large enough to cover the nose and mouth. If the patient breathes it gradually, little or no irritation is produced in the larynx and air-passages, there is but little if any cough or sense of suffocation, nor a distressing or unpleasant symptom of any kind.

There may be some persons to whom the odour of ether is offensive and irritating, but they are comparatively few, and even they can be brought under its influence without any very great annoyance.

The quantity of sulphuric ether required to produce anæsthesia depends very much on the manner in which it is administered. If the patient is made to inhale it rapidly, and the atmospheric air is to a great extent excluded, a small amount will be sufficient. From four to eight ounces may be regarded as the average quantity. It is rare to meet with a case in which less than four ounces will be used; and in protracted operations, in which it is desirable to keep up the state of insensibility for a length of time, I have often given more than eight ounces. The ether should at first be poured on the concave part of the sponge; one or two ounces will be enough for this purpose. When the inhalation is going on, it is better to pour the ether on the outside of the sponge, so as to avoid the necessity of removing it from the face. From half an ounce to an ounce should be used at a time in this way, till anæsthesia is produced. When this takes place, the patient is wholly unconscious, and has no control over the voluntary muscles. He is unable to raise his eyelids when told to do so, and gives no indication of hearing or consciousness, if spoken to in a loud tone. The pulse usually becomes slower than the ordinary standard, though at the beginning of the inhalation it is quicker.

It is, I am confident, a perfectly safe anæsthetic agent. I have not been able to find any well-attested case of death from its inhalation. There may have been such, but they have never come to my knowledge, though I have taken unwearied pains to obtain information on this point.

It has been said, that this may be attributed to the fact that ether is not extensively used, but that if it were, there would probably have been as many fatal cases in proportion from it, as from the inhalation of chloroform. But this statement is not strictly correct; for though ether is not employed as an anæsthetic agent to any extent, if at all, in Great Britain or many parts of Europe, it is used in Lyons, Naples, and is almost the only one that is administered in the principal hospitals of the United States of America, where its now familiar properties were first discovered.

I have given it in several hundred cases, and witnessed its exhibition by others in as many more. I have administered it to infants not three weeks' old, and to persons more than threescore years and ten, and have never in a single instance seen an alarming or distressing effect produced by it. On the first introduction of ether into surgical practice, it was not thought safe to allow persons to inhale it in whom there was reason to believe there was any disease of the heart or lungs, or who had any tendency to an affection of the brain and nervous system. But for some years past I have been in the habit of administering it to individuals of this description, and have as yet had no cause to regret it. In such cases I have thought it prudent to have the vapour of the ether inhaled more slowly, so that it may be more diluted with atmospheric air than under ordinary circumstances; of course, the patient could not be brought as soon under its influence as when taken in the ordinary way.

The state of the system which is produced by the inhalation of ether is that of narcotism, similar precisely to what is induced by drinking immoderately wine or other alcoholic liquors. It is a state of intoxication more transient and less dangerous than that from alcohol. Its effects pass off sooner, because the vapour of the ether begins to escape from the lungs as soon as the patient ceases to inhale it; while alcohol taken into the stomach is carried into the circulation, and mixes with the blood, and in this way acts longer, if not more powerfully on the brain, though its narcotic effect is not so soon produced. It is possible that life might be destroyed by the inhalation of ether, if it be continued uninterruptedly for a great length of time and a great quantity inhaled. Fatal congestion of the brain might thus be produced, as sometimes happens when alcoholic liquor has been taken to excess. But no person of ordinary prudence would administer it in this way. Long before the occurrence of such a result, symptoms of an unequivocal character would indicate the approaching danger.

When death follows the inhalation of chloroform, on the other hand, there is no merciful premonition. The late Dr. Snow, whose experience on the subject was perhaps greater than that of any other person, thought that "*sudden palsy of the heart* is the cause of sudden death from chloroform." In death by asphyxia, the heart beats for some minutes after breathing has ceased; "whereas in some cases of death by chloroform, the breathing has been proved to go on up to the time the pulse stopped, and after it."

With the hope that those who may have occasion to employ any anæsthetic agent will at least make a fair trial of *rectified sulphuric ether*, I respectfully submit these remarks to my professional brethren.

ART. III.

Nutrition, Inflammation, and Ulceration of Articular Cartilage. By RD. BARWELL F.R.C.S., Assistant-Surgeon, Charing Cross Hospital.

IF a careful physiologist and pathologist, who does not assume too much as granted, and who examines each difficulty before galloping over it, will study minutely the present doctrine of the nutrition of articular cartilage, as it may be gathered from all writers on the subject, he will feel persuaded, at the end of his labour, that there is a deficiency somewhere in the chain of reasoning, and elsewhere something unnecessarily assumed. In fact, he will encounter many propositions which are not proven; some of which he cannot receive; and he will find his belief barred in several directions.

The first difficulty he will meet with is this, that almost all observers have assigned as a source of nutriment for articular cartilage the vessels of the synovial membrane and its secretion; but if he examine a fine vertical section of any articular cartilage in any animal, he will find those cartilage-corpuscles which lie near the attached surface well developed, and containing each from two to six nucleated cells, and near this surface not only do the cells in each corpuscle, but also the corpuscles themselves, tend to arrange themselves perpendicularly to the surface; and when a corpuscle divides, as takes place by generation of cells within it, it does so in the same direction. As the object is passed under the glass towards the free surface, he will be struck by a change in this respect; the cells no longer remain in the capsule so constantly perpendicular to one another, and as they divide, they do so as frequently horizontally as in any other direction; at last the divisions and the groupings all tend strongly to the horizontal; the cells themselves are flattened in the same direction, till at last they become mere scales, three or four layers of which (fig. 5), lying close together, form the extreme free edge of the section; that is, the unattached surface of the articular cartilage consists of three or four layers of flattened cells, lying quite close together and overlapping each other's edges. Now, to suppose for one moment that the nutriment which supplies growth to this body should be derived from the free surface, thus consisting of flattened cells, is to think directly contrary to all that we know of nutrition by cells. And therefore, as the growth of these cells evidently takes place from the attached portions, so is it evident that there must be some means of supplying nutriment to that surface. Moreover, the presence of large loops of vessels in the cancelli, immediately next the joint, points to the same fact.

Thus, it being clear that the nutriment of cells growing from a deep surface could not be derived from a superficial one, it becomes necessary to examine into the anatomy of the cartilage and bone, and their mode of attachment to each other. A paper, on the "Organization and Nutrition of Non-vascular Animal Tissues," by Mr. Toynbee, is published in the 'Philosophical Transactions' for 1841. The author describes the epiphysal cartilage, its vascularization and conversion into bone, and continues thus:

> "In adult life, when the epiphysal cartilage has been ossified, the cancelli of the latter are separated from the articular cartilage by a layer of bone, to which may be given the name of articular lamella. The nature of this lamella is worthy of particular attention. It is composed of two sets of osseous layers; the one, dense and thick, is continuous with the vertical fibres of the cancelli; the other, delicate and thin, principally composed of osseous corpuscles, is situated at right angles to the latter, and fills up the interspaces of the vertical fibres. Is this articular lamella complete? I have never been able, by the aid of the microscope, to discover any orifices in it, nor have I been able to force mercury through it."

Todd and Bowman, in their 'Physiological Anatomy,' vol. i., p. 93, give Toynbee's account of this articular lamella, and refer to his paper. Even Kölliker, whose care and accuracy are so well known, has given an inexact account of this bony layer. He says:

> "The condition of the bone, immediately under the articular cartilage, deserves especial mention; it consists, indeed, at almost every joint, of a layer of imperfectly-formed bone-matter, true bone-tissue being only found deeper. This layer, which is from 0·04 to 0·16 of a line in thickness, is formed of a yellow, generally fibrous substance of bony hardness, which, indeed, is really ossific, but which contains no trace of Haversian canals, and no formed lacunæ. Instead of the latter, one finds round or oval bodies lying together in heaps or rows, which in section appear very dark, and which therefore might be taken for bone corpuscles filled with granules of lime. By adding oil of turpentine this error may be avoided; and we find that, as with the real osseous lacunæ of dry bones, the dark appearance is due to air, and the formation in question consists of thick-walled granular cells, still retaining their contents (fat and granules), showing here and there traces of canaliculi and partly calcified; in other words, that they are undeveloped bone-cells."*

This is a circumstantial account, as far as the black bodies in the articular lamella are concerned; they lie together in rows, between each member of which there is a layer, more or less thick, of the compact lamella, and thus there is no communication between each such cell (as there are no canaliculi), that is, fluid cannot pass through the dense bone material from one to the other, as Toynbee seems to have believed when he wrote that "this thin layer has already been stated to be almost entirely composed of osseous corpuscles, which, without doubt, assist to convey the fluid from the cancelli into the cartilage" (loc. cit. p. 172). Thus, according to the results to be obtained by studying the observations of the authorities on this subject, the bony cancelli are cut off entirely from any communication with the joint-cartilage by a dense ill-developed impenetrable structure.

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* Mikroskopische Anatomie, Band I. p. 318.

In fact, although we have at one side of the articular lamella evident demand for nutriment, on the other evident arrangement for its supply, yet in the lamella itself there is no means of carrying the supply to the demand, not even the ordinary means which would have existed had it consisted of the usual bone-tissue with its lacunæ and canaliculi.

On making some pathological investigations on the diseases of cartilages, I could not avoid being profoundly impressed with the apparent contradiction above described, particularly as it threw some of the morbid processes under examination into even a worse confusion than itself laboured under. It thus became necessary, in my mind, that the nutrient mechanism of articular cartilage should be fully investigated, the present physiology and pathology of that substance being based upon notions, which, as they involve contradiction, must necessarily be either false or imperfect.

It first appeared desirable to make out the mode in which the cancellous structure close to the joint end of the bone was arranged, the more so as in the above mentioned 'Philosophical Transactions' the structure is figured as a set of regularly disposed dark squares, surrounded by lighter margins, like a series of picture-frames hung close together; and as room for tortuous vessels in such structure could hardly be imagined, I undertook a series of investigations to establish how their spongy texture was formed, how the vessels ran in it, and in what exact manner the lamella shuts out the cavities from the joint. For these purposes many sections were made of the joint ends of bone, and examined under a low magnifying power; it was then seen that the cancelli are not regular square cavities symmetrically arranged, but form large holes in the sections, of various shapes, and without any regularity of arrangement. The bony scales which divide these cavities are crowded with the ordinary bone cells and their branches, which, under a glass magnifying only ten diameters, look like little dots; the articular lamella has rather a lighter appearance, and even with so low a power may be seen to be of variable thickness. Fig. 1 represents a section of a part of the lower end of the human tibia. Several bones were examined belonging to different mammalia, rabbit, sheep, pig, ox, dog, horse, &c.; the only difference between them appears to be that the smaller the bone, the more compact is the tissue, more especially in little animals, as the rabbit, where the blood channels are relatively not nearly so large and numerous as in the analogous joints of the human subject. (Fig. 1.)

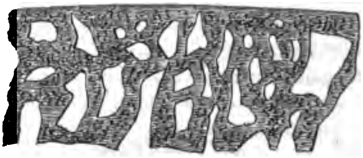
Fig. 1. Cancelli and articular lamella from lower end of human tibia, magnified about 10 diameters.

A like section examined under a ten times higher power, shows that, besides the irregular large cancellous openings, there are running through the bony plates normal Haversian canals, with their concentric system of laminæ and lacunæ. The articular lamella is lighter in colour—i.e., more transparent than the rest of the bone; in it bone cells and canalicula are absent, but there are several black opaque spots of an oblong form, with the long axis at right angles to the lamella, and two or three of these arrange themselves at a certain distance from each other in interrupted rows having the same direction. The lamella is a little darker, a little more brown, like ordinary bone, near its attached than its free surface; it looks as though at this part it had been stained. In no instance does the lamella lie immediately over, and never shuts in, a cancellous cavity; on the contrary, ordinary bone structure always intervenes between such cavity and the lamella. In places the osseous tissue surrounding a cavity near the margin of the lamella encroaches thereon very much; in other places, where the cavity is deeper from the surface, the osseous tissue recedes, and the lamella projects into the bony structure; thus, the articular layer is very uneven in thickness, its free edge is also serrated rather finely but unevenly. (Fig. 2.)

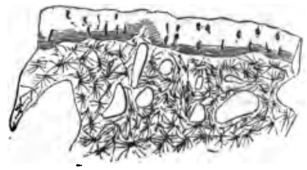
Fig. 2. Cancelli and articular lamella from lower end of human tibia, magnified 100 diameters.

In examining an injected preparation of the articular end of a bone, it will be found that near the joint, immediately under the lamella, is a rich plexus of vessels, which forms a series of loops, and that in the curved portion of each loop the vessel appears dilated. This plexus is not in the long bones derived from the common nutrient arteries, but springs from some of the numerous branches which surround the joint, and they are probably the remains of those vessels which Mr. Toynbee has described as supplying the epiphysal cartilage at the time of its ossification. In each one of the cavities near the articular lamella is a vascular twig, which does not entirely fill the space, but is surrounded more or less by loose fibrous tissue and by fat. In no place does a vessel touch the articular lamella, ordinary bone structure always intervening, as has been already stated.

On applying higher powers, one sees in many sections little more than this, particularly if they be mounted in Canada balsam; but in those, whether made by grinding down the bone or by cutting thin slices with a sharp knife, that are preserved in fluid, indications were seen which tempted me to go on examining the structure of this articular lamella, until at last I convinced myself that it in reality consists of a series of very minute parallel tubes, which run in a wavy course from the bony to the cartilaginous surface. Among these, but having no special, if any, communication with them, are the bodies mentioned by Kölliker as undeveloped bone-cells. In some sections—those, namely, which are not made quite parallel with the axis of the joint from which they are taken—the tubes of the articular lamella cannot be made out, but the section is minutely dotted from those tubes having been cut across. Fig. 3 is a tolerably successful representation of this structure. It will be seen that certain portions of the articular lamella are rendered darker than others, and this is a condition very difficult to make out; yet perhaps I may pretty confidently affirm that it arises from the tubular structure having become so bent in those lines that the canals have been cut through, giving a brown, darker, and finely mottled character to that part. This structure is similar in every mammal in which I have examined the lamella, but perhaps it is plainer in the rabbit than in any other I have yet seen.

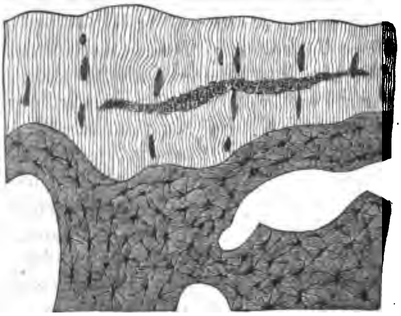

Fig. 3. Cancellous structure and articular lamella from upper end of the human humerus, showing tubular structure of the lamella, magnified 500 diameters.

Having thus succeeded in ascertaining the structure, as seen laterally, it seemed advisable to view the same part from above; for this purpose a joint end with as flat a surface as possible, was chosen; either end of the tibia in most animals answers this purpose sufficiently: the cartilage being scraped away, a small piece of the articular lamella was detached, and ground thin enough to be transparent. In this view the black spots or undeveloped bone-cells are less elongated, all the rest of the section is studded with dots, which, under a sufficient power, and where the section is very thin, appear as small round holes. This structure is the same in all animals that I have examined. (Fig. 4.)

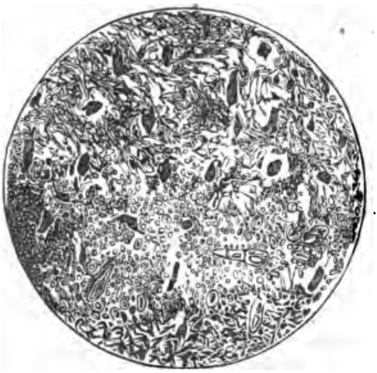

Fig. 4. Articular lamella, section parallel to surface, from lower end of human tibia, magnified 700 diameters.

Thus the articular lamella, supposed until now to be a compact, impenetrable layer of bone, is in reality a structure as tubular as dentine, but the tubes are much finer and less straight; they do not shine as black as the lines in dentine, probably because they are finer, and therefore do not refract light to the same degree. They are, I believe, the minutest set of tubes yet discovered in the body, certainly in any of the hard portions, and some of the tubes lately reported to exist in the softer parts are, to say the least, doubtful. It is necessary that they should be sought for under a high power and with a very good light, which can be varied by Gillott's condenser, or by other means.

Articular cartilage has been so often described, that it must be needless to do more than refer to the sketch already given of the method in which the cells lie, and again to insist upon the fact of the greater crowding of these bodies near the superficial surface, and on their ultimate drying into scales, which overlie each other on that surface—an arrangement which has caused many observers to believe in the existence of an epithelium. If the superficies of fresh cartilage be shaved off thin with a very sharp knife, the section will indeed have the appearance of a layer of epithelial cells; but if a thin slice through its substance be examined, the gradual horizontal arrangement and flattening of the cells will leave no doubt as to the true structure of its superficies. (Fig. 5.)*

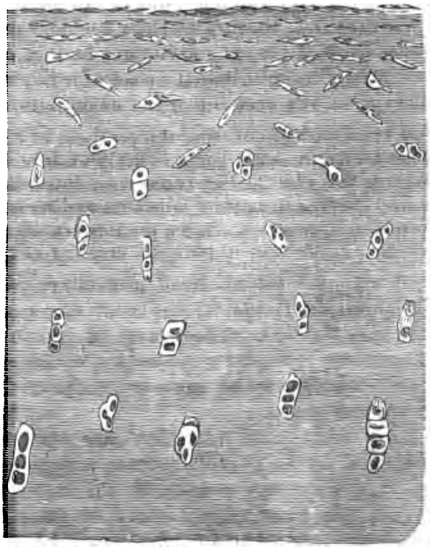

Fig. 5. Cartilage from human astragalus, magnified about 700 diameters, showing the perpendicular arrangement of corpuscles at the lower part, gradually curving into an oblique, subsequently into a horizontal, position, and drying into scales.

Some observers, Mr. Toynbee among them, found that in the fœtus vessels run across the cartilage, even into the middle of joints. In neither a fœtal hare nor calf that I had the opportunity of examining, could I discover any such arrangement, nor any trace of it in a stillborn child.† Nor have I been able to discover epithelium overlying the cartilaginous surface: what Mr. Bowman took for that structure was, I believe, the superficial layer of cells as above described, which, in the yet unused joint, is finer than when it has been subject to wear and tear. The absence or presence of vessels upon the fœtal cartilage is of importance, because if present it would establish the fact of a structure lining a cavity being nourished by its free surface. Besides, as it is clear that, at least during intra-uterine life, there is large provision for nutrition of the cartilage from the deep surface, the presence of such vessels would show that a structure having one free, one attached surface, might be nourished from both. Moreover, it would prove that a cellular structure might be nourished from the surface towards which the cells grow. But the two first facts would be isolated, and the latter is hardly conceivable; and as others besides myself have failed to discover this arrangement, there must have been in the observation of such vessels some occult source of error.

Thus articular cartilage is not so dead and unorganized a material as is generally imagined, as is proved by the care taken to supply to it nutriment in finely divided streams—the most available form possible for use. The great vascularity of the parts, which furnish this supply (a vascularity consisting of dilated and dilatable vascular loops, which are not destined for the nourishment of the bone, since this structure has its own Haversian canals,) shows that the vital actions of cartilage must be active; and although we shall probably never be able to make experiments proving their rate of growth, yet have we on record one or two cases, which show that other changes may be very rapid, so as to confound the conventional idea of the inertness of articular cartilage. Mr. Arnott has reported a case of this sort.‡ A man was bled, and six days afterwards was attacked with phlebitis; six days after its commencement—i. e., on February 4th—pain in the left knee, with some swelling, was observed; on the 8th of the month the man died.

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* This arrangement has, however, not been described as is here done, because it is so difficult to procure sections sufficiently thin, that go all through the cartilage, the outer layers of cells breaking easily from a very thin slice. It may best be managed by cutting away cartilage and articular lamella from the cancelli, scraping the osseous matter away, then laying the cartilage on a piece of cork, and beginning from the formerly attached surface, cutting slices with a well made and sharp Valentin's knife.

† Kölliker could not make out any vessels on the surface of fœtal cartilage.

‡ Medico-Chirurgical Transactions, vol. xv.

The cartilages on the femur and tibia were over a large space so deeply eroded as to lay bare the bone. All which ulceration must have occurred in from 96 to 120 hours.

Mr. Mayo* gives a less rapid case of a boy who had received an injury to the skull. Four days afterwards a joint of one of the fingers and one of his ankles became painful and swollen. He survived the accident only three weeks. Examination of the swollen joints showed that the cartilages had disappeared from them almost entirely.

Sir B. Brodie† gives the case of a boy who had injured the knee, and died in twenty-two days; the account of the examination is as follows:

> "The cartilage covering the condyles of the femur, and that covering the head of the tibia, were found in some parts entirely absorbed, so that the bone was exposed; while in other parts it was absorbed on the surface towards the cavity of the joint, the layer of it next to the bone retaining its natural adhesion and its natural structure. The cartilage in these parts was formed into grooves, having an appearance as if the greater portion of it was removed with a chisel. There was no purulent or other effusion into the cavity of the joint."

A man fell on his head from a considerable height, and was brought insensible into St. Thomas's Hospital; the next day acute synovitis began in the right knee; on the fifth day he died. There were of course the signs after death of synovitis, but the cartilages were sound, except in a spot larger than a sixpence on the inner condyle of the femur, whence it had entirely disappeared; the edges of this ulcer were perfectly smooth, clean, and sharp.

Now the alterations which take place in cartilage are all dependent upon changes in the cells, for in every structure which consists of cells and an inter-cellular part, each cell has a certain district of the latter substance under its maintenance and control; hence the changes in texture or state of the hyaline substance of cartilage are all secondary; and the ordinary mode of classifying the structural changes in cartilages, according to the alterations of its inter-cellular substance, is as false as every classification built upon secondary facts must be. The error is one not of words merely, or it might well pass unchallenged, but is one of essentials, and confounds together certain forms of malady which should be kept distinct, and separates others which have no, or only accidental, differences. Thus the division of the diseases of cartilage into fibrous, fatty, and granular degeneration, according as the hyaline substance is split into fibres or studded with oily or granular matter, must be false, for both the latter changes are mixed with the fibrous transformation of the inter-cellular substance. In fact, fibrous degeneration, as Mr. Birkett and other writers have called it, is not itself a disease, but an accompaniment of almost every morbid process which takes place in articular cartilage.

The basis of a true division can only be laid in the changes taking place in the cells; and these may be divided into—1st. Alterations in their contents, or degenerations; and, 2nd. Alterations in their activity.

1st. *Changes in the Contents of the Cells.*—The degenerations which the cells undergo are albuminous or granular and fatty. Granular degeneration of the cells advances pretty far before it affects the hyaline matter; it begins by a deposit in the cells contained in one or more cartilage corpuscle, of fine opaque granules. The cells enlarge somewhat, but never develop other cells or nuclei; after a time the cartilage corpuscle bursts, and gives forth the cells, which in their turn let free the granules, but the nuclei entirely disappear, and do not come into view when treated by acetic acid. As this disease of the cells reaches a certain point, the hyaline substance becomes striated and slits into fibres.

Fatty degeneration is a much more frequent disease. Many joints of subjects brought into the dissecting room (no symptoms having been observable during life) are found to have their cartilages ulcerated. These ulcerations are lined, and partly filled, with a fibrous structure, and many of them are due to fatty degeneration. The cells near the ulcer are filled with fat globules, generally very small, which completely conceal, afterwards destroy, the nucleus, and render the whole cartilage corpuscle opaque and of a brown colour. The hyaline substance is likewise studded with fat granules, and has undergone a like change of hue; it becomes striated, and ultimately splits into fibres. This appearance is well known, but it sometimes happens that in examining joints not otherwise diseased, the cartilage will be found in one or more slightly raised specks to have lost its lustre, its grey translucent appearance, and to have become of a dull yellowish hue. On cutting vertically through these spots, they are seen to be of a triangular shape, the base being at the free surface, the apex extending more or less deeply into the tissue. A thin section placed under the microscope shows that below the point where the change in colour is perceptible, there is an accumulation of fat in the cells of one or more corpuscles; farther on, more such bodies are involved, and are more crowded with fat granules. When the cells are completely fatty, there will be seen proceeding from them rows of small oil globules, like strings of beads, separated from each other by bands of the hyaline substance, of various breadths, each such band being divided into fibrillæ, among which an occasional oil globule is visible. This is, I conceive, the first stage of fatty degeneration, and serves to show that a fibrous condition of the hyaline substance is not to be considered as a disease apart from the other alterations of cartilage, since it follows both forms of cell degeneration.

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* Ibid., vol. xix., p. 51.

† Diseases of Joints, p. 98, fourth edition.

2nd. *Changes in the Activity of the Cells.*—The activity of the cells may be either decreased or increased. Of the former little is known; but thinning of cartilage does certainly take place, and in such specimens I have found the cartilage corpuscles smaller than usual, and even close to the bony attachment, forming lengthened horizontal bodies in which the cells are very small, with a hardly perceptible nucleus, and look already like the flattened scale of the free surface.

The morbid condition which originates in increased activity of cells is that to which I would more especially call attention, as taking place in all inflammatory disease of joints. The cartilage corpuscles in this state increase in size, and instead of possessing only from two to six nucleated cells, contain from twenty to sixty of all sorts—brood cells, young cells, and bare nuclei. The corpuscle subdivides also more frequently, thus giving rise to a greater number, and they often burst and discharge their contents freely into the hyaline substance; or if this takes place on the surface, they leave a depression at that spot. Mr. Redfern's third, fifth, sixth, and other observations are of this sort of disease. It may terminate either in simple erosion of the cartilage, that is, total disappearance at the spot of that structure, the edges of the cavity looking sharp and clean "as though cut with a chisel;"* or it may give rise to conversion of the hyaline structure into fibres previous to its disappearance. The former of these conditions is the most acute, the latter the more chronic form. That this increase of growth should be attended by loss instead of augmentation of substance is to be accounted for by the excess of demand by the cells over the supply of nutriment, an excess which must be supplied by the inter-cellular substance. In the acute form of the disease, where the cell-generation is excessively rapid, the hyaline substance disappears bodily; in the less acute condition, alternate lines in the direction of the cell-force are absorbed and left, whence fibre must result. The cells, as we have seen, tend to the vertical in the deep, to the horizontal in the superficial part of the cartilage; and the fibres, as they form, take the same direction (fig. 6). The great distinction to be observed between this form of disease and the cell-degenerations above mentioned is, that in the former rapid generation of cells takes place, brood-cells giving birth to progeny, the nuclei dividing and forming fresh cells; and although at the latter stage of the process a few fat granules may be seen in the cell, they neither obscure nor destroy the nucleus. In the latter forms (degenerations), the cell becomes clogged with a material which destroys the generative power of the nucleus, and the cell dies without progeny; but in both the hyaline substance becomes fibrous.

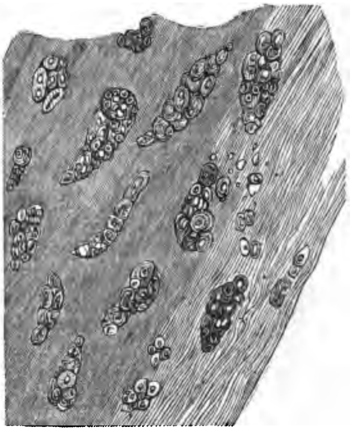

Fig. 6. Cartilage in a far advanced state of inflammation, the corpuscles much enlarged, containing many cells with two or more nuclei. To the left, the hyaline substance is finely striated; to the right, fibrous, mingled with round and fusiform nucleated cells. From tibia at knee, magnified 700 diameters.

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* Sir B. Brodie: Diseases of the Joints.

Now, although by the cases quoted from Mr. Arnott and other writers we find that absorption of cartilage may be extremely rapid, and although by the examinations of articular cartilage we find that the processes are those of excessive action, it may seem bold to state that this latter form of disease is inflammation. It is true, that throughout, no vessel, nor any signs of a vessel, can be seen in cartilage, but if we enter into a study of all organs of the body, we shall find that every tissue is without bloodvessels; the capillaries only skirt the organization, whatever it may be. Examine the grey portion of the brain, then the white, and it will be seen that the pieces of brain-matter between the enclosing meshes of vessel are much larger in the latter than in the former; than examine a piece of the dermis from the tip of the finger and another piece from about the loins, and again are the islets of tissue between the vascular streams larger in the latter than in the former; but shall it therefore be said that the grey matter of the brain is more capable of inflammation than the white, or the skin of the finger-tip more capable thereof than that of the loins? Take a wider example, and compare the vascularity of intestinal mucous membrane with that of bone. A piece of that mucous membrane is so richly supplied with blood, that it is of a brilliant red hue, and if it be injected, it appears simply a mass of bloodvessels. A piece of bone, on the other hand, is so sparingly supplied, that the islands between the vessels, whither no blood penetrates, are frequently half a line square, and even much larger in the longitudinal section; yet the mucous membrane is not more capable of inflammation than bone. Surely it would be very illogical to say that every vascular part, whether highly so or only slightly so, was equally capable of inflammation; but every non-vascular part (so called) was incapable of it. Surely, if this vascularization make the difference between capacity and incapacity, there would be degrees of capability from the part most supplied to that least supplied with blood; but we do not find this to be the case. Again, although in such a tissue as the brain the vessels lie very close together, yet there must be spaces, however small, wherein brain-matter lies and whither vessels do not penetrate, which are therefore non-vascular; and in cerebral inflammation it is these very spots, and not the vessels themselves, which are inflamed. Can any one presume to say that a non-vascular tissue of a certain size shall be capable of inflammation, but beyond that size incapable? In other words, can it be affirmed, that in order to render any issue capable of inflammation, it is necessary that blood should flow at a distance not exceeding a certain definite fraction of a line?

In endeavouring to explain the vascular phenomena of inflammation, recourse has been had to all sorts of theories; some have supposed that there is dilatation, others contraction of the capillaries, with increased or decreased rapidity of the blood stream; others have imagined an actual stasis. Soon it came to be shown that capillary vessels are incapable of either contraction or dilatation, and the place of action was shifted to the finer arteries and to the nerves which may be supposed to control them. Here again, however, is difficulty without end, for we find all sorts of vascular states, which have been called passive and active, venous and arterial congestion, which, however, are not inflammation.

I lately saw a man, under the care of my friend Dr. H. Salter, who had chronic palpitations of such rapidity, that his heart beat over two hundred in a minute. If rapid vascular action be the cause of inflammation, the man ought to have been inflamed all over, through and through. Moreover, how does it happen that parts supplied by the same branch or twig of artery shall be inflamed in one point, and not in another?

Let us, to solve these difficulties, again go back to the affirmation that inflammation is an alteration in the nutritive function, and let us consider the conditions of that function. It is evident that in the support of various tissues the bloodvessels are the mere passive carriers of nutriment—at least, no one, I imagine, would for an instant suppose that the nutritive function is forced upon a part by the blood; on the contrary, the tissues (all extra-vascular) draw from the blood pabulum as they want it. This demand of the tissue is so balanced with the capillary blood-supply, that the two functions go on without in any way disturbing one another; and if from any extraneous cause the blood-stream be temporarily either considerably quickened or retarded, we do not find that any immediate increase or decrease of the tissues results. On the other hand, when a part is undergoing great exertion or employment, therefore great nutritive activity, the blood-stream is immediately and instantaneously increased. This amounts to saying, that the supply of blood does not regulate the nutritive activity of a part, but that the nutritive activity regulates the supply of blood. If, then, inflammation be a disturbance in the nutritive activity, it begins where that function is situated in the tissue itself, not in the vessels, the changes in which are only secondary.

It seems, then, that inflammation begins in the substance of tissue which is extra-vascular, and no one can affirm that this non-vascular islet must be of a certain definite minuteness, in order to permit of inflammation, since we do not find that the tendency to that action is in any direct ratio with the minuteness of the intra-plexural parts. It seems rather probable that every organ which is capable of nutrition must also be capable of inflammation, and it therefore is impossible to affirm that cartilage, because there are no vessels in its substance, cannot assume that disorder of its usual nutritive action which constitutes inflammation.

But again, the sceptic may still say, "How can inflammation be present when its symptoms, heat, pain, redness, swelling, are absent?" The signs of a thing are not the thing itself, and are very often not even essentials, although, as far as our knowledge at present goes, they may be constant accompaniments. Thus, a potentate of India had always been accustomed to consider fluidity so essentially characteristic of water, that he either bastinadoed or bowstringed a gentleman who mentioned that he had seen that substance solid. The phenomena, which we have been accustomed to consider as signs of inflammation, are not signs of the disturbed nutritive action, which we find to be situated on the tissue, but of the vascular disorder produced thereby; and if it be admitted that inflammation may begin in the tissue, and that vascular disturbance is only a secondary action, it must also be admitted that inflammation can proceed to a certain distance without producing such symptoms of disorder. We have other signs of inflammation which are likely to be more infallible than mere hyperæmia, which is often present in non-inflamed parts under the name congestion. When we find evidences of superabundant action in the tissue, with effusion of lymph, hardening, softening, &c., we know that inflammation has been present, even if we do not find any vascular engorgement in its immediate neighbourhood. Now, this increased action and subsequent alteration of texture is found to take place in cartilage, the simplicity of whose structure and purity of whose action render such changes easier to follow, though its symptoms be not striking, because free from secondary actions, which being prominent, have been seized upon as invariable characteristics of inflammation. Therefore it appears to me that this increased action occurring in cartilage should serve as the type ot the inflammatory process in the simplest form, unmasked by any secondary action.

In the experiments which Dr. Redfern made upon the costal cartilages of dogs, he performed certain operations, which, in ordinary textures, would have produced inflammation; and the results were increased action of the cartilage cells, and hyperæmia of the neighbouring parts which supply that structure with nutriment. When a synovitis has lasted some time, the cartilages will in places be found ulcerated and fibrous; in some parts the free surface only will be affected, in others the deep only will be altered and detached from the bone, sometimes the two lesions occur together. The detachment of the cartilage from the bone takes place not by ulceration of the deep surface, but by separation of the articular lamella from the rest of the bone, and the ulceration on that deep surface takes place subsequently. Now in all these instances the microscope will present the same, or very nearly the same, appearances—the differences being only in degree. There will be great increase in the activity, in the number and in the nuclear contents of the cells, producing absorption of the hyaline substance, either slowly after its conversion into fibres, or more rapidly without such change; subsequently, before the bone be laid bare, before fibrification of the cartilage shall have reached its osseous surface, the articular lamella will have become detached—a process which can only take place by the disease having passed through the cartilage to the bone. If we saw through the bone of any joint, in which pretty active ulceration of cartilage is going on, we shall find close to its articular surface a deep red blush. This is the true hyperæmia of inflamed cartilage, which we miss in the substance of that structure itself. That the vessels thus injected belong more especially to the cartilage is evident from the fact, that bone is only supplied with such looped and dilated branches when close to articular cartilage; when in its over-action that structure requires more nutriment, the vessels become engorged, and pour forth more plasma than can pass through the tubular structure of the lamella; this material becomes either pus or false membrane; the action, in fact, spreads to the bone, which now becomes inflamed, the bony plate supporting the articular lamella is detached from the rest of the bone, and adheres to the cartilage, which it causes to feel rough and gritty, like sand-paper.

Thus, as said above, the action, begun in the synovial membrane, passes to the cartilage, thence to the bone; in the synovial membrane it was inflammatory, in the bone inflammatory; it cannot, in its passage through the cartilage, have changed its nature twice—that is, once from inflammation in the synovial membrane, to something else in the cartilage, and once from that something else, back again to inflammation in the bone. It is more consonant with reason to admit at once the inflammatory nature of the action in articular cartilage.

Ulceration, even of this active type, may go considerable lengths without producing any distinct symptoms; this is by no means an argument against its inflammatory nature; it was shown above that hyperæmia and hyperæsthesia are but secondary conditions of inflammation. When, therefore, the inflammation reaches a point to produce the hyperæmic condition above described, then certain symptoms—starting of the limb, "soreness of the bone," "gnawing of the bone,"* &c. &c., come on. These pains are obscure, and described variously by different patients; but they are not, as is generally supposed, symptomatic of commencing ulceration of cartilage, but of that action having already proceeded to a point when the bone is affected. Whether the attack have been more or less acute—i.e., the time since the commencement of the disease—is immaterial, for whenever in synovitis that particular set of pains comes on, we shall find the cartilages destroyed over a greater or smaller surface, throughout, or nearly throughout their entire thickness. I have some reason to believe, that when the articular lamella gives way, there is, at all events for a time, mitigation of these painful symptoms; but this is a point which requires more investigation, and the rarity of cases in which a strumous joint can be so early examined renders it very difficult to determine the fact by direct proof.

This view of the different diseases to which ulceration of cartilage is due, will explain why ulceration of cartilage is sometimes a painful disease; why at other times, we find, after death, erosions, which gave during life no symptoms of their presence. Such excavations are frequently seen in the dissecting-room or in the dead-house, and may, by proper examination, be distinguished as belonging to two classes—firstly, to degenerative disease; secondly, to an inflammatory disease. The former of these gives rise to no hyperæmia in the cancellous structure, and therefore to no symptoms whatever; the latter is of a slow inflammatory form, causes a slight, variable, and very chronic engorgement of the cancellous structure, and in all probability gives rise to those obscure pains and stiffness in the joints which are often accompaniments of old age.

To recapitulate shortly. The results of a careful examination of the anatomy, physiology, and pathology of articular cartilage lead to the following conclusions :—

1st. That although in the articular cartilage itself there are no vessels, there are situated immediately within the articular lamella a set of arteries destined to supply that cartilage with nutriment.

2ndly. That the articular lamella is composed of a finely tubular structure, which allows its nutrient fluid to find its way to the cartilage in finely divided streams.

3rdly. That the cartilage has no other source of nutriment.

4thly. That the diseases of cartilage must not be estimated or named by the alterations which the hyaline structure undergoes, but by those which take place in the cells.

5thly. That a certain number of these diseases are degenerative, but the larger number decrease or increase in the generative activity of the cells; the last of these attends all inflammatory diseases of the joints, and is itself inflammation.

6thly. That the hyperæmia of this inflammation is situated in the vessels immediately beneath the articular lamella, and it is this hyperæmia which gives rise to the symptoms usually supposed to be produced by ulcerating cartilage.

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* So described frequently by patients.

7thly. That simple degenerative diseases of the cartilage, as they produce no hyperæmia, produce no symptoms.

Art. IV.

Series of Clinical Cases (with Observations) illustrating the Views recently put forward by Dr. Brown-Séquard, as regards certain points connected with the Physiology of the Nervous System. By John W. Ogle, M.D. Oxon, F.R.C.P., Assistant-Physician to St. George's Hospital, Secretary to the Pathological Society of London.

I design by the relation of the following clinical cases, which, with one exception, are selected from the records of medical experience in St. George's Hospital, to assist in rendering human pathology instrumental in elucidating the truthfulness of some of the ingenious and profound generalizations based mainly on experiment, and brought forward of late years by Dr. Brown-Séquard.

The members of the medical profession in England are now fully conversant with, and fully appreciate, most of the special researches of that dexterous experimental physiologist respecting the blood, muscle, animal heat, and the nervous system in general; and therefore any formal enunciation of his varied deductions would here be quite superfluous and misplaced. I will consequently, by way of premise to the cases which I shall detail, merely content myself with bringing before the mind of the reader, in a manner as concise as the intentions of this communication permit, those conclusions from facts observed by Brown-Séquard connected with the minute organization of the nervous system, the certainty of which I purpose to illustrate by the cases which I shall adduce, and which are in direct opposition to the conclusions arrived at by other physiologists, French, German, or English.

These cases will, I think, tend most materially to corroborate the following deductions ascertained by Brown-Séquard:—

(*a*) That the posterior columns of the spinal cord do "not" form, as was thought by Longet and many other observers, the means for the entire (*en totalité*) conduction of sensitive impressions to the brain; but

(*b*) That these columns form a medium to a slight extent only for the passage of sensitive impressions; such conduction along the spinal cord chiefly occurring in the central or grey matter of the cord, into which part the fibres of the posterior sensitive roots of the nerves, by means of transverse, ascending, and chiefly descending fibres, find their way almost immediately after gaining the cord, *viâ* the posterior columns, the posterior grey cornua, and, in part, the lateral columns. And that if there be any fibres conducting sensitive impressions ascending from the trunk or limbs along the entire length of the cord, their number must be very inconsiderable.

(*c*) That the fibres conveying sensitive impressions to the brain do not at any rate decussate at a point higher up in the cerebro-spinal axis than the pons Varolii.

(*d*) That the decussation of fibres conveying sensitive impressions must be of necessity not only at a part below the level of the upper margin or central part of the pons Varolii, but even for the most part, if not entirely, below the medulla oblongata itself. That is to say, this crossing or interlacement of sensory nerves must take place in the spinal cord itself, and that not in any particular spot, but in *every* portion of the cord almost immediately after the entrance therein of the afferent fibres.

(*e*) That all the motor or efferent fibres decussate at a distinct point immediately below the pons Varolii—that is to say, at the anterior pyramids and the neighbouring parts, contrary to the views of Valentin, Cruveilhier, Longet, Foville, &c.

Having thus enumerated in a succinct manner the various physiological positions which I purpose to illustrate and strengthen in this pathological communication, I will, without further circumstance or digression, proceed to narrate my cases, the true interpretation of which at the time of their occurrence, and upon the old theories then commonly in acceptance, could obviously not have been apparent.

CASE I.—*Dislocation of some of the vertebræ, and projection of bone into the anterior surface of the spinal cord, the posterior columns remaining entire; perfect loss of sensibility and power of movement in the lower limbs.*

History.— P. S——, aged sixty-two, was brought into St. George's Hospital, having fallen a height of eleven feet, and received a scalp wound. He was for ten minutes stunned by the fall, but after admission walked into the ward, complaining only of pain at the neck. He had a slight cough. Nothing fresh was complained of until three days afterwards, when he felt numbness in the legs and tottered in walking. Sensibility of the skin to pinching and pricking was everywhere perfect. Seven days after the accident he had to such a degree lost power in the legs, that he scarcely was able to move them; still, sensibility of the skin remained as before. The bowels were constipated, and the catheter had to be used, the urine, too, becoming purulent. Stiffness also of the arms came on, and nine days after the accident he could with difficulty move them; they were also much flexed. All power also of voluntary movement below the diaphragm had disappeared. Eleven days after the accident the soft parts over the larger trochanter of the right femur began to slough, and two days later his cough was attended by dyspnœa. On the thirty-second day after the accident all sensibility of the skin, as well as all power of movement in the legs, was found to be lost: the respiration was chiefly abdominal. After some difficulty in swallowing, the patient died September 27th.

Post-mortem examination.—Dislocation forwards of the sixth cervical vertebra was found to exist, so that the body of the seventh vertebra projected for at least half an inch into the spinal canal behind. The dura mater opposite the sixth and seventh cervical vertebræ was thickened, and a slight amount of recently-formed soft fibrin existed at this spot between the bone and the theca vertebralis. The anterior part of the spinal cord at this part was "pressed on by the body of the seventh cervical vertebra, and was slightly softened," but there was no laceration of, or extravasation of blood into, its substance, nor was the cord more than usually vascular or otherwise affected. The lungs were congested, and the bronchial tubes filled with frothy serous fluid. The kidneys were also cysted and otherwise diseased, and the lining of their pelves, as well as that of the bladder, was highly inflamed and covered with soft fibrin. Abscess also of the prostate gland existed.

Remarks.—The most noteworthy point in this case is the fact that, along with loss of motility in the limbs, owing no doubt to injury of the anterior columns, there was loss of certain forms of sensibility of the skin, whilst *the posterior columns of the spinal cord were uninjured.* From the nature of the accident—viz., the projection of the seventh cervical vertebra to the distance of half an inch into the spinal canal, &c.—the anterior columns, and very possibly the middle grey matter also, must have been greatly injured. This would account of course for the loss of motility, and also, according to Brown-Séquard's views respecting the functions and uses of the central grey matter, for the loss of sensibility as to certain tactile impressions of the surface of the body. This latter symptom of anæsthesia, it will be remembered, came on at a later period than the affection of the voluntary motor power, and no doubt the grey centre of the cord, being at the onset less affected by the direct injury than the anterior columns which were more exposed to the influence of the accident, became secondarily involved in such softening as one might expect, and as was found to have occurred in the immediate vicinity of the directly injured part of the cord.

One or two points there are in the above case apart from the object of this paper, which, in connexion with the injury of the spinal cord, are deserving of a passing notice, such as the persistent flexing of the muscles of the arms. The congestion of the lungs found after death might well be considered to correspond with the loss of action of the chest-moving muscles (for the respiration was mainly effected by the diaphragm); but the question may plausibly be entertained, to what extent the accumulation of the fluid in the pericardium was fairly attributable to any paralysis of vasculo-motor influence, as respects the nerves accredited to that covering, owing to injury of the cervical part of the spinal cord.

CASE II.—*Carcinomatous disease of the dura-mater at the edge of the foramen magnum, encroaching greatly on the outer or white parts of the spinal cord: absence of any anæsthesia of the skin as to tactile impressions.*

A woman, aged forty-nine, when brought into St. George's Hospital, could neither walk, stand, nor feed herself without assistance. She could, however, move both of her legs when in bed, although slowly; and the left arm and leg could be much more easily moved than those on the right side. She was frequently subject to sensations of numbness over the entire body, but she never experienced actual pain or had any convulsive attacks; and there was complete absence of anæsthesia as regards pinching or pricking of any part of the integument, as was particularly noticed only six or seven days before death. It was stated that the patient originally had a "fit" of some kind or other, and that she had been for some time in a state of unconsciousness, and subsequently that she was found to have quite lost the power of movement on the *left* side of the body. Of this want of power she had partially recovered, but latterly the *right* side of the body had fallen into a state similar to that of the left. Whilst under observation within the hospital, she was unable on trying to draw up the legs in bed, to pull them up simultaneously, but first one and then the other slowly yielded to her efforts. As before said, there was no anæsthesia as regards pinching of the skin, and this was the case to the last; but strange sensations of numbness were complained of. Her power over the muscles became yet weaker, and the patient gradually sank and died, considerable dyspnœa and cough, with abundant secretion of mucus into the bronchial tubes, having occurred.

Post-mortem examination.—A vascular but very firm encephaloid tumour, of the size of a small walnut, was found attached to the dura-matter, connected with the right and anterior border of the foramen magnum. This tumour (see fig. 1) indented the cerebellum and encroached greatly on the foramen magnum and the parts which passed through; having, moreover, hooked around it the seventh and eighth pair of cranial nerves. A small mass of a similar character was also found attached to the dura-matter at the opposite (the anterior) border of the foramen magnum, and these two encephaloid growths interfered to such a degree with the aperture for the passage of the spinal cord, as to reduce it to a triangularly-shaped space, so small as barely to allow the tip of the little finger to enter.* Two other and like growths were found to be connected with the falx cerebri and dura-mater near the optic commissure. Some softening also there was of the dorsal region of the spinal cord, and considerable evidence of congestion of the lungs existed.

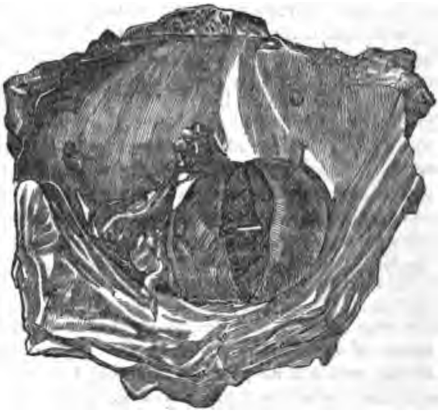

Fig. 1. This illustration exhibits a large rounded mass of carcinomatous material (of which a section has been made), in connexion with the dura-mater at the right and anterior part of the edge of the foramen magnum (seen from above), encroaching considerably upon the nervous substance passing through that opening. A similar but much smaller mass is seen to the left of the larger one.

Remarks.—In this case the chief point of interest is found in the fact that sensibility to pinching and pricking of all portions of the skin's surface remained undiminished, and this certainly to within a few days of death, whilst at the same time there existed so considerable an encroachment upon the external or superficial portions (the anterior and posterior parts, &c.) of the spinal cord as it passed through the aperture, that this organ did not exceed the tip of the little finger in magnitude at this spot. In such a case it could not be otherwise than that those columns, generally hitherto supposed to be the medium for the conveyance of sensibility, were very considerably pressed upon. These external parts of the cord receiving directly and immediately the pressure (in this instance slowly exercised), would to a considerable degree, as it were, shield the enclosed or grey central portions, which consequently would be less injured. Hence it came to pass, as we may from Brown-Séquard's theory conjecture, that the original sensibility of the skin persisted.

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* This specimen is now in the St. George's Hospital Pathological Museum, as Preparation No. 1 b, Sub-series vii., Series xxi.

The softening of the upper part of the dorsal region of the spinal cord came on, doubtless, during the last week of life, and with it, in all probability, corresponded the extreme dyspnœa, the cough, and the excessive outpouring of mucus into the bronchial tubes. Before quitting the consideration of this case, the peculiarity regarding the voluntary muscles must receive a passing notice; for although there is a distinct mention of impairment of their power, first on one side and then on the other, yet when the patient was within the hospital there was indeed not so much a deficiency of actual motor ability, as of promptness on the part of the muscles in replying to the mandates of the will.

CASE III.—*Fracture of the dorsal vertebræ and sternum. Softening of the anterior and central parts of the spinal cord; the posterior columns, entire. Loss of power in moving the lower limbs. Considerable anæsthesia as regards pinching and pricking of the surface (but incomplete) of the integument of the lower limbs.*

J. H., aged thirty-one, a carter, was thrown out of his cart backwards, and pitched on to the ground, alighting chiefly upon the lower part of his neck and shoulders. He was at the time quite sober, and was raised from the ground perfectly sensible. When brought home it was ascertained that he had almost, but not entirely, lost all voluntary power of moving the muscles of the lower limbs. On the day following, the bowels acted thrice from medicine without any loss of power over the sphincters of the rectum; the bladder was emptied naturally. Two days after the accident the respiration became oppressed, and large crepitations with gurgling were heard in the bronchial tubes; and the bladder could not be emptied without the aid of the catheter. Slight power of moving the lower limbs still existed as before, and the sensibility of the skin of these parts as to pinching or pricking was much blunted but not actually destroyed. Numbness of the legs was complained of. As to the arms, it was ascertained that he could voluntarily raise the right one to the head, but not so the left one, as its movement was restricted, although this really appeared to be contingent rather upon the external contusions than upon the loss of central nervous power. The respiration was entirely diaphragmatic, the abdomen being tense and tympanitic; and there was a sense of constriction around the body on a level with the umbilicus. On the third day after the accident all power of moving the lower limbs was found to be lost, and more numbness of the limbs was complained of, but still sensibility of the skin to such tactile impressions as were tested was found to be not annihilated. The bowels became constipated, the urine also still requiring the catheter for its removal. The patient remained in the same state until death, excepting that the bronchial tubes became more clogged up, and consequently the breathing more laborious, the phlegm being expectorated with the utmost difficulty. Prior to death, which occurred April 9th, the evacuations were several times passed involuntarily.

Post-mortem examination.—In addition to the extravasation of blood, &c., amongst the muscles of the neck and back, the following structural changes were met with. The veins of the spinal membranes were very distended with blood, and a slight amount of dark coagulated blood was seen extravasated between the laminæ of the vertebræ and the membranes, at a point corresponding to the bodies of the two last cervical and first dorsal vertebræ. The spinal cord itself, opposite to the effusion of blood just mentioned, was in parts very soft for the distance of about one inch, the softening not involving the whole cylinder of the cord, but being limited to its anterior half and to the grey matter in the middle of the cord, which also contained numerous minute ecchymosed spots. *The posterior columns of the spinal cord were quite entire.* A transverse fracture of the body of the first dorsal vertebra, but without displacement, existed, and the anterior and posterior ligaments of the neighbourhood were uninjured. There was also fracture of the sternum. The lungs were in part only congested, and in part quite hepatized.

Remarks.—In this case it must be noticed that integrity of the posterior columns—those (formerly) supposed conductors of sensation—was found after death, whilst during life the sensibility of the skin of the lower limbs was greatly impaired, whether as regards pinching or pricking of the surface. These symptoms accord well with the textural changes discovered after death—viz., the softening and the ecchymosis of the central grey, and of the posterior white parts of the spinal cord. The oppression of the breathing, the loss of thoracic respiration, along with the bronchial effusion and râles, and, finally, the pulmonary hepatization, may be considered as coinciding with the destruction of the upper portion of the dorsal division of the cord. Another point of interest not to be overlooked in the history of this case, is the distended and tympanitic state of the abdomen, and the feeling of constriction around the umbilical region—symptoms most probably referrible to a paralysed and yielding condition of certain portions of the muscles forming the parietes of the abdomen.

Case IV.—*Cyst of considerable dimensions in the right portion of the pons Varolii. Great loss of power of motion and of sensibility to pricking and pinching in the "left" arm and leg, and side of the trunk. Also numbness and diminished sensibility of the skin of the "right" side of the face and nose, and of the "right" temple, with increased vascularity of the conjunctiva of the "right" eye.*

The patient, a girl, aged eighteen, was brought into St. George's Hospital in the following condition. There was greatly impaired power of motion in the left leg, arm, and hand, and there was great numbness and loss of sensibility of the skin, as regards pinching and pricking, of the whole of the left side of the body, as high up as the middle of the neck. The condition of the right arm and leg was natural, both as regards power of motion and sensibility of the skin. Numbness also, and contactile anæsthesia of the integument of the right side of the face and nose and right temple, existed, along with very great vascularity of the conjunctiva of the right eye and much lachrymation. The right eyeball was drawn inwards, and could not be abducted, and the right pupil was "constricted," the left one being "dilated." The right eyelids could not be approximated, and some difficulty was found respecting the power of opening the mouth and of swallowing. Subsequently, vomiting and constipation came on, along with greater vascularity of the conjunctiva of the right eye, and great dulness of the cornea. Death was preceded by coma, with lividity of the face, immediately following what was termed by the nurse a "fainting fit."

Post-mortem examination.—A large cyst, of the capacity of a bantam's egg, was met with, situated in the right portion of the pons Varolii (fig. 2). The parietes of this cyst, which consisted of attenuated nervous structure, had given way at the thinnest part during the removal of the brain, owing to the firm adhesion which had formed between the cyst-wall and the dura-mater lining the base of the cranium; and the interior of the cyst was seen to have been occupied by a quantity of light coloured and glairy albuminous fluid, containing a number of white albuminous particles, being also lined by a thin film of albumino-fibrinous material. Its outer surface in one or two places had recent shreddy fibrinous material con-

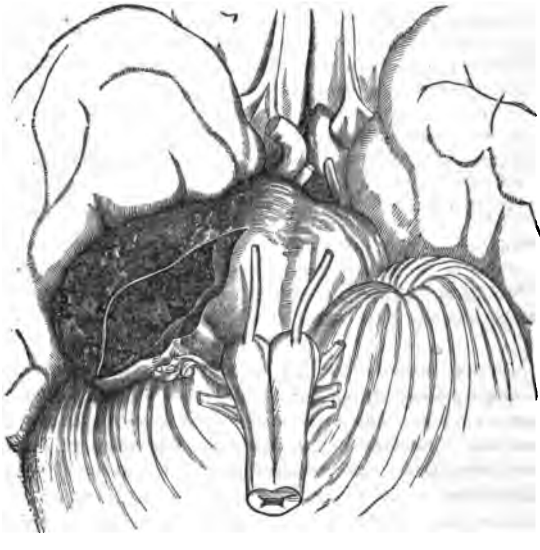

Fig. 2. This illustration exhibits a cyst in the substance of the right portion of the pons Varolii, which has been freely opened to show the interior.

nected with it.* By means of the cyst, the anterior part of the under surface of the right side of the cerebellum was much indented, the whole cerebellum having been, as it were, slightly twisted, and somewhat pushed over to the left side. The under surface of the middle cerebral lobe on the same side was also indented. Moreover, the cyst, owing to its position, interfered with the fourth, fifth, and seventh pair of cranial nerves on the right side. Excepting thickening of the arachnoid membrane around the cyst, and distension of the lateral cerebral ventricles with clear fluid, the other parts of the brain were healthy.

Remarks.—This cyst was no doubt the result of softening of some scrofulous deposit within the pons Varolii. Besides the symptoms clearly referrible to interference with the fourth, fifth, and seventh cranial nerves above mentioned, and which for my purpose require in this place no comment or further enumeration, this case shows decisively that the decussation of nerve-fibres (at least of the majority of them) devoted to the conduction of sensory impressions, does not at any rate take place, as some have held, at a point higher up in the cerebro-spinal axis than the pons Varolii; for here we have a loss of sensibility of the skin on the "left" side of the body (the same side on which power of motion was destroyed) produced by disease on the "right" side of the pons Varolii (the left side being unaffected). Had it been otherwise—that is, had the bulk of the sensory fibres coming from the left side of the body (that affected with contactile anæsthesia) passed through the pons Varolii prior to their crossing or decussation—then they must have penetrated the uninjured left portion of the pons Varolii, and in this way escaped all implication; and we should not have had any anæsthesia of the limbs on the left side of the body. In like manner, this case also shows that the nerve-fibres addicted to the conduction of voluntary power passing downwards from the brain to the right side of the body, intercross at a point below the lower margin of the pons Varolii.

CASE V.—*Extravasation of blood on one side of the median line of the pons Varolii. Complete loss of power of movement in the muscles of the right arm and leg, and also of sensibility, as regards pinching and pricking, of the integument on the same side of the body.*

This case, which I venture to quote from the 'Transactions of the Pathological Society,' was brought before the notice of that Society† by my late friend, Mr. W. Barlow, formerly resident officer at the Westminster Hospital. It was that of a woman, aged thirty-four, who, after falling down suddenly, was found to have lost all power of motion in the right arm and leg, and also to have anæsthesia, as regards the impressions of pricking or pinching, of the paralysed limbs. There was, moreover, paralysis of the muscles of the left side of the face, the left eyelids not being capable of closure, and the mouth being greatly drawn to the right. Reflex action of the affected limbs could easily be excited. Consciousness was unimpaired, and even intelligence remained until within a few hours of death, which occurred five days after the accident.

Post-mortem examination.—A coagulum of blood as large as a filbert, which had ruptured into the fourth ventricle, was found so situated in the pons Varolii that a section of this organ in the mesial line passed almost, "*but not quite*," through the centre of the effused blood. The brain and cerebellum were otherwise natural.

Remarks.—Here I would premise, that if the description of this case by Mr. Barlow had been somewhat more precise and fuller of detail, it would have been, for the purpose to which I am applying it, more available; but it is nevertheless most useful. The post-mortem record certainly implies that a blood-clot was located more on one side than the other of the pons Varolii, but fails to state on which side it preponderated. The probabilities are, and I think it must, from the symptoms, be assumed, that the blood-clot was situated more on the *left* side of the median line than on the right, and that it involved the left part of the pons more especially—along, no doubt, with a considerable portion of the right part of this ogan. Thus we have loss of power of motion, and loss of a certain form of sensibility, on the right side especially, of the body; although, considering that the lesion was not confined to one side of the pons, there may have been, in addition, a certain degree of, or even very considerable interference with, the power of motion or sensibility on the opposite side. This latter might have been comparatively so insignificant that the right side attracted by far the most attention.

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* This preparation is now in the St. George's Hospital Pathological Museum, as Preparation No. 11 a, Sub-series iv., Series xx.

† Transactions of the Pathological Society of London, 1852–3, p. 28.

The assumption just entertained as to the left side of the pons Varolii being chiefly the seat of the effused blood, becomes materially fortified, and the gap in the documentary evidence greatly filled up, by the fact of the existence of facial paralysis (so termed) on the "*left*" side, and absence of it on the right side. The blood-clot was no doubt altogether out of the way of the source of the portio dura of the seventh pair on the right side, but was evidently placed so much on the opposite (the "*left*" side) as to involve the portio dura on that side. Had the clot been quite central, and not more on one side than the other, we should either have had *no facial paralysis* at all, or have had it on the two sides to an equal degree. Again, had it been quite central, we should doubtless have had an equal degree of loss of voluntary power and sensibility on both the sides of the body.

CASE VI.—*Large masses of scrofulous deposit in the right portion of the pons Varolii and medulla oblongata; general loss of muscular power; great numbness, and "coldness" of the left arm and hand.*

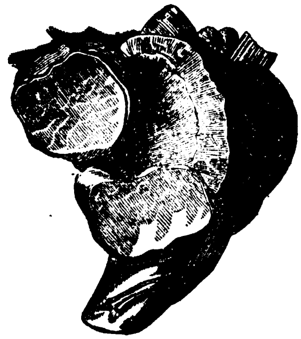

Fig. 3. The illustration exhibits masses of scrofulous deposit in the right portions of the medulla oblongata and pons Varolii, partly dislodged from their natural position.

The patient, a girl, aged eighteen years, was brought into St. George's Hospital with headache, and great numbness and "COLDNESS" of the left arm and hand, which she had experienced for about a year. There was also a peculiar oscillating movement of both eyeballs, but especially of the right one. The patient for two or three years had been subject to double vision. General loss of voluntary muscular power and semi-stupor, with a difficulty in swallowing and articulating her words, preceded death.

Post-mortem examination.—Two large rounded masses of firm scrofulous material were found occupying, one the centre and posterior part of the right portion of the medulla oblongata, and projecting into the fourth cerebral ventricle, the other the upper and right portion of the pons Varolii (fig. 3).* The posterior parts of the crura cerebri were also softened, and one or two small scrofulous deposits were met with in the posterior parts of the cerebral hemispheres.

Remarks.—The particulars of immediate interest in this case, as regards the question for which I here introduce it, are the following:—

(*a*) The conjunction of the disease of the "right" part of the pons Varolii and medulla oblongata with the disorder of sensibility on the "left" (the opposite) side of the body.

(*b*) The fact that sensibility on one side was affected, whilst the scrofulous deposit was situated so low down as it proved to be on the opposite (the right) part of the medulla oblongata, demonstrating that the decussation of such up-going or centripetal nerve-fibres as form the medium of communication for sensory impressions, decussate, to a great extent at least, at some part or parts below the medulla oblongata.

(*c*) The complaint of "coldness" of the arm and hand (of long duration) on the side *opposite* to the lesion of the pons Varolii, &c. This point is of great interest in connexion with the subject of the conduction of reflex phenomena (as regards blood-vessels) in the pons Varolii—a subject treated of by Dr. Brown-Séquard at p. 525 of vol. i. of his 'Journal de la Physiologie.'

CASE VII.—*Laceration of the cervical part of the spinal cord, the left side being mainly affected, in connexion with dislocation of the vertebræ; contactile sensibility of the skin interfered with, chiefly on the opposite (the right) part of the body.*

History.—The case was that of a man, aged twenty-eight, who was brought into St.

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* This specimen is now in the St. George's Hospital Pathological Museum, as Preparation No. 10 a, Sub-series iv. Series xx.

George's Hospital in a state of collapse, but conscious, after a fall and blow. At the first he was quite able to move his legs, but not his arms, and two hours subsequently he lost all power over both his legs. At a later period the lower part of the body and the legs, as well as the lower part of the right arm (as high up as the elbow), were found to have completely lost sensibility to the impressions of touch or pinching; the upper part of the right arm still, however, perfectly retaining sensibility to these tactile impressions. The legs and arms continued immovable, and the triceps muscle of the left arm became affected by repeated spasms. Priapism also existed. The patient died seventy-two hours after the injury.

Post-mortem examination.—Dislocation of the fourth and fifth cervical vertebræ was found. There was considerable laceration of the corresponding portion of the spinal cord, the left part of its substance being chiefly implicated in the injury, and the central parts being much more affected than the outer ones. The parts injured were greatly softened, and infiltrated with effused blood. The anterior root also of one of the nerves near the mutilated part of the spinal cord was torn off, with the exception of a few of its lower fibres.

Remarks.—The above case seems to illustrate the fact that afferent sensory fibres actually decussate in the spinal cord, and that also in various parts. This is exemplified by the circumstance that whilst we have injury chiefly to the "left side" of the cord at the lower part of the cervical region, we have the contactile sensibility of the skin mainly affected on the "right" (the opposite) side, the whole of the right arm below the elbow being totally deprived of this form of sensibility. If decussation of such sensory fibres as pertain to the lower parts of the arms had not existed at some part below the injury of the spinal cord, this injury, so much the more extensive as it was on the left side of the cord, would of course have been followed by greater loss of sensibility on the same or corresponding side of the body.

Such are the clinical cases, obtained, with one exception, from the experience supplied by St. George's Hospital, which I would adduce as bearing upon several important propositions newly advanced by Brown-Séquard touching the functions of the spinal cord, and indicative especially of the anatomical routes along which communication is established between the central nervous masses and the peripheric portions of the frame.

I might have proffered other cases also from the same source, but as none appeared to afford such manifest illustrations, or to be so free from subordinate phenomena calculated perhaps in the minds of some to cloud or complicate the main subject which those clinical cases are intended to elucidate, I have desisted from any multiplication of their number. Had all the instances of disease or injury of the cerebral and spinal centres been at the time of their occurrence carefully examined with reference to the special views promulgated by Brown-Séquard, there can, I think, be no doubt that the pathological experience of a field so wide as that which a hospital like St. George's presents (whether furnished by facts accumulated under the "curatorship" of so many accurate and scientific observers as were my predecessors, or during the period—one of between six and seven years—in which the charge of the pathological department fell to my lot), would have afforded a vastly additional number of cases which would have proved highly available for such an occasion as the present one.

But it is universally felt that in matters of scientific research rich and important details may be brought together indicating most praiseworthy diligence and to a considerable extent philosophical precision, and nevertheless they may be deficient when called into requisition in aid of any special general question, and this merely from the want of the existence of some paramount intention or guiding purpose in the mind of the investigator at the moment of observation. The mind's eye not being illumined from any particular source, the experimenter or observer very frequently not only fails to a certain extent in constructing or giving unity and coherence to the edifice for which each individual fact might be most fitted, but also records the phenomena presented to his notice in such a way that when employed by future artificers, they are found to be just wanting in that single element or characteristic which alone is required to render them fully useful for his specific purpose. This must, I imagine, have been found to be the case with all who in any department of the intellect have set themselves with a particular or newly-acquired insight to utilize disjointed material, whether amassed by themselves on any previous occasion, or by others; and in like manner I have found it to obtain with regard to the varied subject-matter which our hospital has at its disposal (the valuable aggregate of a period now extending over nearly twenty years),* in reference to Dr. Brown-Séquard's theories on the nervous system.

As their consideration may in a measure explain past deficiencies, and also prove in some degree serviceable for future guidance, I will here venture to enumerate some of the particulars which, had they been in past years attended to by observers in our profession in the registration of clinical histories and in the record of post-mortem appearances, would have greatly heightened the intrinsic value of such recorded facts as are to be met with in many of our medical publications.

These particulars naturally arrange themselves under the separate headings of Clinical or Life Histories and Post-mortem Pathological Appearances.

As regards the Life Histories, in the first place, an oversight with respect to the following items may frequently be noticed as having occurred—an oversight, which, let it be remembered, was in many cases positively quite inevitable at the time, owing to defect in our then existing physiological knowledge, resting upon which alone as a basis, pathological statements or speculations can possess any true or constructive philosophical character.

Firstly. The frequent observations as respects any diminution or loss of power recorded baldly as "paralysis," without any qualification whatever—that is to say, without any approximative statement as to the degree of deficiency of muscular power, or as to the method of its access, whether gradual or rapid; whether the paralysis followed a so-called "fit" or not, and if so, whether it was attended by pain at the onset (i.e., at the exact period when the supposed lesion of the nervous structures took place). Again, it is noticeable that "hemiplegia" is often stated to have occurred, whilst very frequently, indeed, no mention is made as to the existence or absence of any degree of facial paralysis, or of divergence in the movements or alteration in the form and appearance of the tongue; or if such mention does exist, there is not infrequently a total want of allusion to the side of the face or tongue affected. Still further, in connexion with this subject, there is, judging from the mention of certain collateral symptoms, a frequent want of diagnosis between the opposite states of "*paralysis*" and "*spasm*" of the facial muscles (two conditions which it is important to notice, may pass into each other, and which if only slightly marked, may without difficulty be mistaken for each other).

Secondly. We often find reference made to diminution, or entire loss of power of voluntary movement, along with evident and total omission as to whether or not the condition of the muscular or various forms of tactile cutaneous sensibility were in any manner implicated.

Thirdly. There is often a mention of diminution or loss of cutaneous sensibility, but no accompanying particularization of the special form of anæsthesia which exists—whether, that is to say, it was an interference with the perception of mere contactile impressions, or an insensibility to the various other cutaneous impressions, as of differences in temperature, of pinching, pricking, &c.

Fourthly. Great numbers of clinical records evidently display considerable research into the question of anæsthesia in one form or another; but there is obvious neglect of attendant observation (and this also to a great extent) as respects "hyperæsthesia" or the exaltation of the various forms of sensibility.

Fifthly. We have in very numerous instances an entire want of evidence, and in many a deficiency, at least, of evidence, as to the temperature of the skin or mucous membrane of the affected part of the body (the thermometer being in only few instances resorted to). In many cases the subjective sensations of the patients are all that is alluded to.

Sixthly. We frequently have observations, obviously rigid and complete, as to all needful particulars respective of the side or portion of the body mainly affected and therefore pre-eminently attracting the attention of the observer, whereas there has been at the same time a total neglect of any mention of the "opposite side" as to the existence of any affection of sensibility or voluntary power of motion (although, of course, such may have been present to a very subordinate extent).

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* This appears to be a fitting opportunity for making known the fact that we are originally indebted to the energy and practical industry of my friend Professor Hewett (whom as curator at a long interval I had the honour of succeeding), for the superior arrangements at St. George's Hospital which we possess as respects the systematic recording of post-mortem observations accompanied by their clinical life-symptoms. The excellency of the plan to which I allude, will be, I trust, more widely made known ere long, as we are about to print and publish the Catalogue of the Hospital Pathological Collection, revised and remodelled by Mr. Gray and myself, under the sanction of the Medical School Committee.

Seventhly. We often find vague mention made of such symptoms as "strabismus," "distortion of the eyes," &c., but no indication as to which form of squinting existed, or even as to which eye was affected, and we are in consequence utterly ignorant as to what sets of muscles or what nerves have been implicated. Also the pupils are oftentimes spoken of as being "dilated" or "contracted," but it is manifest that sufficient care has not been taken to observe whether the pupils harmonized with each other or not, or whether they deviated from their natural condition as to size or form; or whether they were constant or variable in size. Again, with regard to the special senses, or the faculties of deglutition or swallowing, symptoms are very often quite unmentioned, or when at all alluded to, the statement is often omitted as to the particular side on which the sight or smell, or taste, or hearing, &c., was subjected to interference.*

Eighthly. Constant omissions exist as regards the powers of reflex nervous action enjoyed by various affected parts of the body; or if they at all attracted attention, their presence or abeyance have only been studied in the case of the soles of the feet. Again, touching the determination of the degree of excito-motory power enjoyed, very often, perhaps most frequently, this has only been effected by means of tickling the skin (the use of heat or cold, or galvanic stimuli, being not at all resorted to).

Ninthly. Symptoms precise and extensive enough, are often noted, either wholly without reference to date, or without relative dates of their occurrence as regards the exact time of death, so that the reader is often at a loss to conjecture with any degree of accuracy as to the connexion between such and such a symptom on the one hand, and the morbid lesion observed after death on the other hand.

Of this kind are the numerous omissions as regards Clinical histories, especially as concerns injuries and diseases of the nervous system, often encountered in our attempts to connect varied and isolated cases in support of any dominant view or theory—omissions which obviously quite prevent that accuracy of diagnosis which it is so desirable to possess, and which is at the present time to a very great degree attainable, considering the multifarious results of recent experimental and pathological researches.

I will now proceed briefly to delineate some of the defects frequently experienced in our attempts to systematize and, so to say, co-ordinate the Post-mortem Pathological appearances detailed in many records. They may be conveniently disposed as follows:

Firstly In post-mortem statements as to affections of the brain and spinal cord, such phrases as the following are pretty constantly met with: the cord or brain "softened," "harder than usual," "lacerated," "containing extravasated blood," "ecchymosed," "discoloured," and the like; and this very frequently unattended by any mention as to which precise portion of the brain or spinal cord was affected, or to what extent the given lesion extended. Omissions on these latter points are especially disastrous (considering the important interests of diagnosis or physiology), in the case of disease or injury of the pons Varolii or medulla oblongata, in which parts the disposition of nerve-fibres allotted to different actions is more complex than in the spinal cord proper.

Secondly. Pathological observers have too often rested satisfied with the examination of one only of the large central nervous organs; at one time the spinal cord alone having been examined, whilst at another time, and this of course much more frequently, the brain has been subject to scrutiny, to the neglect of the spinal cord. The nerves, and especially the origin and the roots of the spinal nerves, have frequently altogether escaped examination; and at times, only that part itself of the spinal cord which was previously supposed to have been affected, has been submitted to examination, the remaining portion of the cord, and especially the "chorda equina," having been quite overlooked.

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* Dr. Brown-Séquard shews that when deglutition is impaired, indicating an affection of the pharynx, it may be diagnostic of the exact part of the nervous system interfered with, for in cases of alteration of the pons Varolii, this symptom is observed to exist *un*accompanied by loss of speech; whilst, if the latter co-exists, the lesion is probably situated in the medulla oblongata or the "*vagi*" nerves.

Thirdly. Clinical observation, with a view to the establishment of physiological questions, has been, as a rule, more addressed to instances of "primary disease" of the nervous centres; whereas, perhaps, more information (especially after the early effects of shock, loss of blood, injury by contusion of muscles, &c. have quite passed away*) relative to the healthy functions of certain parts of the nervous centres, would have been elicited by the study of cases of "surgical injuries" of those textures, or of such rapid diseases of the spinal or cranial bones as affect those subjacent nervous textures in a secondary manner as by pressure. This latter kind of cases is particularly serviceable in the study of the physiology and properties of the nervous centres, inasmuch as we have therein a probability of a quicker fatality, and in consequence less chance of such extensive structural changes taking place as would go far to complicate and obscure the phenomena chiefly regarded; whether such textural alterations be indeed pathological, having occurred before death, or are essentially of post-mortem origin, and attributable in the main to chemical decomposition, which will be the more complete in proportion to ante-mortem pathological disintegration.

In drawing to a close the above enumeration of omissions noticeable in many recorded cases of injury and disease, bearing upon the invaluable propositions advanced of late years in the field of our studies of the nervous system, I am anxious emphatically to repeat, that many of the shortenings of observation which I have commented upon are such as were in past years quite inevitable by reason of the uninformed state of our pathology and physiology, as regards the multifarious points connected with the nervous system. Not a few of them have been the natural consequence of the absence of any pervading and regulating theory, in support and under the influence of which, observation should have been conducted.

Convincing, but yet fully capable of more extended proof, especially by means of clinical cases sedulously and minutely observed, as are the demonstrations of Dr. Brown-Séquard in regard to the interesting physiological questions, of which the foregoing cases are illustrative, it is to me a subject of regret that these cases which I have just cited are so scanty in number. I was anxious, and thought I should be able in some way to show why so large a mass of material as that at my disposal has, comparatively speaking, yielded so little product, and that was my reason for bringing forward the various points of omission to which I have alluded, as occurring to one's mind in supervising clinical records in various quarters.

I also felt assured that in our future investigations connected with disease and injury of the nervous system, greater care and precision of observation will be called for than hitherto we have been in the habit of bestowing in the matter.

For this reason, therefore, having the hope and expectation that from their consideration a few practical suggestions may occur to the reader's mind for future use and guidance in the prosecution of researches connected with the multiform and too often embarrassing lesions of the nervous system, I have taken this opportunity of noticing such defects in our methods of examination as rise into prominence on reflection upon the ends which in such examination we must ever keep in view.

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* I may here opportunely draw attention to the precaution (forcibly pointed out by Dr. Brown-Séquard as being so imperative) not to be misled in our estimate of the effects of injury to nervous structures by the results almost always attendant on the division and laceration of powerful muscles, entailed in the act of obtaining access to central nervous structures, lesion of which would obviously give the appearance at first sight of nerve-paralysis. He of course alludes to experiments on the lower animals, but the hint may be taken also in respect of injuries in man, as it no doubt often happens that injury (such as laceration and contusion) of muscles from extensive accidents involving the spinal region, produces a semblance of serious lesion of some of the spinal nerves or even of the spinal cord itself.

PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON MICROLOGY.

By John W. Ogle, M.D., F.R.C.P.

Assistant Physician to St. George's Hospital, and Honorary Secretary to the Pathological Society.

Part I.—Physiological Micrology.

EPITHELIAL SYSTEM.

On the Structure of Cylindrical and Ciliated Epithelium. By Dr. N. Friedrich, of Heidelberg.[*]—The author first alludes to the observations of Kölliker and Funke upon the perpendicular markings supposed to be porous canals at the broad end of intestinal epithelium, and thought to serve the purpose of resorption of fat. He then adverts to some former observations of his own on the epithelium of the bile-ducts of the fœtus, which left it doubtful whether the perpendicular markings were broadly-striped coverings of the cells or adherent cilia, and speaks of having met with cylindrical epithelium of the gall-bladder and ducts in the adult as well as in the infant, which often possessed striped terminal edges, as also of the stripings of the cell-covering in ciliated epithelium of the bronchi in man and the ox, and in the ventricles of the human brain.

In the ventricles of the brain the epithelium was seen by the author to have the following striking characteristics. The cilia were seen through the homogeneous edges of the cells to pass directly into the cells more or less deeply, in some cases only just passing within, in others passing to the nuclei, and in others, but rarely, quite down to the base of the cell. In these cases each single cilium appeared to correspond to a striping of the border, and each line traversing the cells appeared to be a downward projection of a marking on the cell-cover. In many cases the cilia were adherent, so that only the striping of the border, with its continuation into the cell, could be seen; but for the most part the latter were absent, and only a simple cylindrical cell with a striped cover existed. Sometimes fat-drops were seen in or upon the threads as they pass through the cells, or at the termination of the cilia within the cell. In some cases also the bile-tinged epithelium-cells of the gall bladder showed similar stripings, but less clearly.

With regard to these observations, the author acknowledges that they are not absolutely and entirely new, as Valentin[†] has spoken of normal ciliated epithelial cells, in which the cilia sank deeply into them; and Bühlmann,[‡] Donders,[§] &c., have seen cells under various conditions in the midst of which the cilia have existed, or rows of fat and granules corresponding to the striping of the bright border. The fact of the discovery of these ciliary threads in epithelial cells of bronchi, and the ependyma of cerebral ventricles as well as in the intestines and gall-ducts, quite removes the supposition that their presence is connected with the metamorphosis of fat elements.

The striping of the cell-cover is connected by the author with the functions of resorption in general, as in this way we have an arrangement of lines forming a regular system of exceedingly fine capillary tubules from the tips of the cilia to the base of the cells. The author then speculates upon the connexion between the lower end of the epithelium of the empendyma with the projections of the subjacent areolar-tissue cells, and on the probability of the areolar-tissue corpuscles being the commencement of the lymphatics; and refers to the probability of the contents of the cerebral ventricles being taken up by the termination of the ciliary threads, and carried directly into the lymph stream, the ciliary movement subserving the motion in the tubes and the onward progress of new molecular matter.

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[*] Virchow's Archiv, Band v. Hefte 5, 6, p. 585.

[†] See article Flimmerbewegung im Handwörterbuch der Physiologie, Band i. s. 500.

[‡] Virchow's Archiv, Band xi. s. 576.

[§] Moleschott's untersuch., Band ii., 1857, s. 115.

The author then draws an analogy between epithelial cells and the areolar-tissue corpuscles (justifiable in certain places at least), looking upon the former as being in fact a modified instance of the latter, only destitute of any intervening substance.

He proceeds to describe a species of epithelial cell which he had met with, which led him to believe that in some cases more than a single cell was placed on a common stalk communicating below by the union of their processes. Similar cells had been seen by Heidenhain in the cylindrical epithelium of the intestines of the rabbit.*

The author concludes by giving the details of two cases of chronic inflammation about the brain of children, in which alterations in the epithelium were met with, and from which he is inclined to suppose that the more obvious existence of the lines seen traversing the cells is only a species of hypertrophy allied to the indistinct hypertrophy of the general ependyma of the ventricle.

MUSCULAR SYSTEM.

On the Minute Structure of Muscular Fibre. By Professor Amici.—The author in his communication† describes the muscular fibre of the fly as follows. In the centre, and passing through it longitudinally, there exists a canal filled with spherical or oval vesicles containing very fine granules. This canal is surrounded by a kind of sheath, consisting of a series of flat rings placed one above the other at a small distance, and united by numerous longitudinal threads. Close upon these threads a soft cellular tissue exists, and external to this another sheath, formed of rings united by threads. Lastly, surrounding the entire fibre we have a thin transparent wrinkled membrane.

The author then describes the process of manipulation resorted to by him, and the appearances presented during the examination of such a muscular fibre. The cross stripes correspond to the profile of the flat rings of the double sheath, and consist each of three flat layers forming the thickness of a single ring; the dotting of the transverse stripes is produced by the middle one of these three layers which is punctate, whilst the two others are more transparent. The dotting is probably the result of the insertion of the longitudinal threads which bind one ring to another. Each fibre has its tendinous termination, to which it is united by the numerous diverging fibrils into which the tendon divides, and which are fixed to the free convexity of the last ring of the fibre. In the wing of the fly, whilst one end of the muscular fibre is so terminated by a single tendon, the other is attached by means of the diverging fibrils directly into the opaque cartilage, no single tendon intervening. Sometimes a tendinous pedicle is seen, having a bundle of fibres at its free extremity resembling a thistle flower; and very often instead of the three longitudinal bands ordinarily seen along each fibre caused by the central canal and the cellular layers, one sees only the central one. At times there is an appearance of five bands, owing to a second layer of cellular tissue near to the circumference of the muscular fibre. Unlike other fibres, those of the flaccid chest-muscles may easily be separated, and are found also to contain a number of globules or vesicles generally disposed in layers. The fibres of these muscles are very firm and cylindrical.

Speaking of the voluntary muscles of higher animals, he comes to the conclusion that they are composed, not of varicose cords, nor of aggregated particles, nor of spiral threads, but of cylindrical tubules divided transversely by a diaphragm, just as the fibres of the fly are, by rings.

He then alludes to the representation of muscular fibre in Mr. Quekett's treatise on the microscope, and assents entirely to the correctness of the appearances there depicted, but objects to the statement made in connexion, that the transverse lines of the fibril give the appearance as if it were composed of a linear row of more or less long or quadratic cells containing a central dark substance, which extends to the side of the cell, and prevents it being transparent. He contends that there are no cells containing the dark substance, but that the difference of light and dark portions depends in part on the difference in power of refraction of light possessed by the alternate segments of the fibrils. He then goes on to observe on the muscular fibres of the wasp, bee, and other insects. His opinion is that the contractile part of the fibre is that enclosed between the cross stripes, which during contraction or elongation remains of the same thickness.

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* See Moleschott's Untersuch, Band iv. Heft 3, s. 264, 1858.

† Il Tempo Giornale Ital. di Medicina, Chirurgia, &c., vol. ii. p. 328, quoted in Virchow's Archiv, Band xvi. Hefte 3, 4, p. 414.

OSSEOUS AND CARTILAGINOUS SYSTEM.

Histological character of bone produced by the Transplantation of Periosteum. By Dr. Ollier.*—This observer has given the result of his experiments upon the artificial production of bones by the transplantation of periosteum, and on the regeneration of bone after resections and complete removal. These experiments, of such vast interest to physiology and surgery, were performed upon rabbits of various sizes and under various hygienic conditions, in whom he grafted portions of the periosteum into parts outside the limits of normal ossification and under the influence of vessels strange to such ossification; and he found that whenever portions of this membrane have been transplanted, exudations capable of ossification have been produced. At the end of a certain period the formation of true bone was the result, demonstrating that the periosteum is not only a mere limiting membranous envelope, and that a tissue may preserve its property and functions, although removed from the influence of all naturally surrounding parts. The author exemplifies especially the origin and mode of development of new bone, showing that if it proceeds from the periosteum it is not the result of transformation of its fibrous layers, first into cartilage and then into bone. The important element of this membrane engaged in the process is a layer of blastema on the inner surface, so delicate that in scraping the periosteum with a scalpel it is difficult to obtain any portions of it visible without the microscope; and this blastema appears as a rule to become penetrated by calcareous salts between the fourth and twelfth days (a period corresponding to the formation of the first osseous cavities), but if this process fails to occur in that period, the new bone remains in part fibrous, and the development is slow and incomplete. Insufficient nourishment, suppuration of the periosteum, and other conditions, injuriously affect the process. Without digressing further, we will here state the three kinds of experiments which the author followed in his prosecution of the question. They were as follows—

1stly. Those in which the portion of periosteum used was still left in more or less connexion with the bone, and was grafted into the midst of muscles or under the skin, but continuing to receive vessels from the bone.

2ndly. Those in which the pedicle of the portion of periosteum was divided three, four, or five days after transplantation, so as to interrupt all continuity with the bone.

3rdly. Those in which the piece of periosteum completely detached from the bone and rest of periosteum was at once transplanted into neighbouring or distant parts.

The new bone formed in any of the above ways, varying in size according to that of the transplanted periosteum (in one case a bone almost as large as the tibia being produced), is found at its periphery to possess a regular layer of compact osseous tissue, and to be covered by its own periosteum. It is hollowed in the interior by medullary spaces, which terminate by uniting into a relatively large cavity, and which are formed by the rarification of the bone-tissue and production of small cavities, whose walls finally give way. The osseous corpuscles, as observed in delicate sections under the microscope, are seen at first to be irregularly disposed, but in the compact tissue they are arranged in layers sufficiently distinct around the vascular canals; but the regularity which in natural bone is observed around the Haversian canals is here wanting, as far as hitherto has been observed. The Haversian canals are generally parallel to the axis of the bone, but their arrangement is not perfectly intelligible in some respects. The medullary spaces are full of a soft, reddish, vascular substance, like fœtal medulla, and is found to contain:—(*a*) Free nuclei (the médulocelles of Robin), and small medullary cellules with a round nucleus; (*b*) Plates with many nuclei, generally infiltrated with fat and granulations, and containing from three to eight nuclei analogous to free nuclei (the myéloplaques of Robin; (*c*) Fatty matter; (*d*) Some fibro-plastic elements and some fibrils of connective tissue; (*e*) bloodvessels. There very often is to be observed a peculiar longitudinal groove running all along one of the surfaces of the new bone, owing to the deficient union of the parallel borders of the portion of periosteum; a similar line is also to be observed on the surface of bone reproduced after sub-periosteal resections.

The author then describes at length the method of development of bones obtained by the transplantation of periosteum, of which the following is a condensed statement:—At the very commencement an effusion of lymph takes place, at first serous and then more consistent, which infiltrates the portion of periosteum and neighbouring tissues. The periosteum soon becomes swelled, and its capillaries filled with blood, and on its inner surface an exudation is formed, which is distinguished from the above effusion by its greater consistence and by its constant increase whilst the other one decreases.

At the end of four or five days an accumulation of firm, transparent, or slightly greyish material occurs within the periosteum (for its edges are then united so as to form an envelope for the blastema). This material is chondroid rather than cartilaginous. About the seventh or eighth day the calcareous matter begins to be deposited, a process not necessarily preceded by the formation of veritable cartilage, although sometimes we find a substance hard, elastic, and with the external characters of the latter. When once commenced the ossification advances quickly, beginning at the centre and passing to the periphery. The above mentioned blastema is found by the microscope to be composed of a large number of free nuclei and cellules, analogous to those found in embryonic tissues, embedded in an amorphous, more or less granular substance. A few fusiform cellules or very fine fibrils, are also met with; and moreover cellules with a single nucleus like the small cellules of the medulla, and large regular cellules with numerous nuclei like the multi-nucleated plates of the same tissue. The blastema is more abundant in proportion to the growth of the animal. Under the transplanted periosteum this layer of blastema continues to be the germ and point of departure of new bone, the various elements being formed in succession, and the intermediate substance becoming fibroid, calcareous granulations being deposited, and ossification accomplished.

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* Brown-Séquard's Journal de Physiologie, p. 14, Jan. 1859.

In those cases in which cartilage *has* been found, the cellules and cavities differed from those of normal cartilage in form and grouping. The author does not appear able to account for the existence of cartilage in some cases, and its absence in others. He proceeds to detail experiments showing that neither the blood-vessels nor external layers of periosteum suffice to produce bone; a layer of blastema of embryonic cellules being necessary. These embryonic elements are seen intimately mixed at certain points with the cellular-tissue and elastic fibres composing the deep layer of periosteum.

He concludes by speaking at length of the reproduction of various kinds of bone and joints after re-section, and shows that after the removal of articular extremities of two contiguous bones, the articulation is capable of regeneration if the capsule and ligaments be left continuous with the periosteum of the re-sected bone. As a means of diminishing the risk of suppurative inflammation of bone after amputation, and of forming the union of the stump, he suggests that the end of the bone should be covered, and the medullary cavity closed up, by a piece of periosteum.

On Ossification in Cartilages. By H. Müller.—The author,[*] speaking of the so-called ossification by substitution, and not of that of secondary bone, periosteal deposits, clavical, cranial bones, &c., arrives at the following conclusions respecting the order of the phenomena of its development. 1st. That the cellules of cartilage are disposed in regular groups. 2ndly. That the fundamental substance is encrusted with calcareous salts. 3rdly. This latter is resorbed in the construction of the medullary spaces. 4thly. These increase, encroach upon the cavities of the cartilage, whose calcified walls disappear. 5thly. At the same time that the medullary spaces are so formed, the true osseous substance is deposited. 6thly. This substance, at first a soft delicate stratum, becomes hardened, and finally encrusted with calcareous matter. In this new layer many cellules are seen, of a stellate form, from the commencement. 7thly. The stellate cavities are owing to the thickening of the fundamental substance around the cellules, and not to the production of canaliculated pores in the stratified layers which exist around the calcified walls of the persistent cavities of the cartilage. 8thly. The stellated cellules of the osseous tissue are to be considered as the offshoots of the cartilage cells which, being liberated by the opening or disappearance of their capsules, are multiplied in the interior of the medullary spaces forming the fœtal medulla, of which a part becomes cellulo-osseous, the other part remaining in the state of cellules of the medulla. Probably this fundamental layer is secreted by the stellate cells. The new bone is the seat of an active movement of composition and decomposition, destroying the old parts and producing new ones. 9thly. In the interior of a cartilage mass the osseous substance first appears in the canals of the cartilage. They become filled with an osteogenic layer supplied with stellated cells, at first soft, then hard, and finally encrusted with calcareous matter (as in the short bones, vertebræ, tarsal bones, &c.). 10thly. In the long bones the osseous substance is first deposited at the periphery, beneath the perichondrium, whilst the interior is transformed into fœtal medulla.

GLANDULAR SYSTEM.

Glandular Structures in the Conjunctiva. By Dr. W. Manz.[†]—the author, following Professor Meissner, who had found structures similar to the sweat-glands in the conjunctiva of the calf and ox, has pursued the investigation to a greater length. In those animals, in the neighbourhood of the under and inner part of the edge of the cornea, are to be found a number of pouches, chiefly observing a linear direction (from six to eight in each eye), terminating in roundish glandular bodies connected with the conjunctiva and sub-conjunctival areolar tissue, and having excretory ducts opening free into the conjunctival cavity. These ducts for the most part penetrate the connective tissue, and observe partly a direct and partly a more or less convoluted course, but are less regularly spiral than the ducts of sweat-glands. Their diameter in the calf is about 0·02‴ to 0·025‴ and something greater in the ox. The rounded glandular terminating bodies are composed of convolutions and loopings of the pouch, inside a special transparent and lacerable capsule. The gland bodies, which are about 0·1‴ to 0·2‴ in diameter, contain a quantity of finely granular material; here and there a delicate epithelium was apparent. The walls of the pouch are so thin that often a double contour could not be seen, and near to the outlet the canal dilated in a fusiform or globular manner. Very often one or two blood-vessels accompany the ducts to their outlet.

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[*] See Brown-Séquard's Journal de la Physiologie, Oct. 1858, p. 810, as quoted from Kölliker's Zeitschrift für wissenschaft. Zoologie, vol. ix. part 2, 1858.

[†] Henle und Pfeuffer's Zeitschrift, Band v. Hefte 2, 3, p. 121.

The author met with similar glands in the conjunctiva of the goat, but did not meet with them in the case of the dog, cat, or rabbit; whilst in the pig he found glands, but of quite a different construction to those above described, and chiefly on the inner side of the cornea. Lying amidst concentrically arranged areolar-tissue fibres under the conjunctiva, bright roundish vesicles may frequently be seen by the naked eye, varying from 0·03‴ to 0·1‴ diameter. These are found to possess openings of an oval shape, surrounded by a rim, and very often slightly curved radiating lines are to be seen passing outwards to the edge of the rim. In the immediate neighbourhood of the cornea the glands are disposed in two or three rows. The vesicles and the surrounding trabecular work contain pigment. The glands contain roundish cells, some lying in regular order like glandular epithelium, and nuclei of various sizes, with a quantity of finely granular material. No fat drops were visible. The part of the conjunctiva occupied by these glands is generally separated from the other part by a vessel, which often forms more or less of a circle around the cornea, and from which fine twigs are given off into the glandular framework, and stronger branches in an outward direction.

In the human eye a fibrous texture was found finer and more delicate than that in the pig, enclosing rounded shining places of various sizes, the largest being 0·05‴ to 0·09‴ in diameter, and of very variable shapes, filled with bright round cells containing nuclei, which in the central part are less crowded and without order, whilst at the circumference they are fewer, the outermost layer of cells being perpendicular to the boundaries of the space. After describing the microscopic appearance of perpendicular sections of the cornea, the author goes on to speak of a peculiar appearance near the margin of the cornea, where fibrous material passes from the so-called membrane of Bowman at even distances, forming partitions in the epithelial layer, filled with round cells and covered above by the epithelium of the cornea. Many of these compartments have lateral outbulgings, and occasionally the fibrous structure is coloured by pigment. The parietes of the compartments in the epithelium are not unlike the papillæ of the skin in form, in their being invested with epithelium, and in their possessing vessels.

Lymphatic Vessels of the Liver.—Dr. Beale* gives some drawings of the minute branches of the lymphatics of the liver of an ox. He had succeeded in tracing branches of lymphatics into the smallest portal canals, and finds that the vessels and ducts are surrounded by a network of lymphatics.

On the Matrix of the Kidney.—The fibrous appearance which has been by some authorities considered as being caused by a distinct structure (first described by Goodsir as the fibrous matrix of the kidney) is considered by Dr. Lionel Beale† as being for the most part the result of collapse of the capillary bloodvessels, and partly caused by the sections of uriniferous tubes. In specimens of kidneys which have been carefully injected with transparent injection, so as to distend the bloodvessels, no such appearance was observable; and after very many experiments, Dr. Beale was quite unable to make out any structure of a fibrous character between the tubes of the kidney and capillary vessels. He considers that the appearance above alluded to is merely owing to the shrinking and crumbling which necessarily occur when uninjected specimens are washed in water.

THE BLOOD.

Remarkable Effects produced by adding Sherry Wine to the Blood.—Dr. William Addison describes these at length,* as follows: At first there is no disturbance in the liquor sanguinis, or plasma, owing to the addition; but afterwards the fluid is seen by the microscope to contain multitudes of molecular particles, which, as Dr. Addison thinks, have come out of the red corpuscles. The corpuscles not only threw off these molecules, but also long threads or tails are projected by them into the fluid. Sometimes as many as five of these tails are seen issuing out of, and remaining attached to, a single corpuscle. They all terminate in a knob at the extremity, and wave about in a very extraordinary manner. Many of them grow thicker: from being at first a delicate filament, they swell out to a considerable thickness, and then breaking away from the corpuscle, they continue a kind of wriggling movement in the fluid. Others remain attached to the corpuscles, and attain a very great length. At the same time, from the numerous molecules issuing from the corpuscles, the liquor sanguinis becomes troubled or disordered, as just described. Dr. Addison describes the corpuscles as undergoing various internal changes before the appearance of the tails, and he remarks that on the addition of the wine to the blood all disposition in the corpuscles to adhere in rolls is removed. Sherry wine alone will produce all the effects described; but the best manner of repeating the experiment is as follows:—Dissolve two grains of common table salt and one grain of carbonate of soda in half an ounce of water. Take a slip of glass and receive on it a "very small" drop of blood; then place, by means of a pipette, a small drop of the saline solution close to, but not touching the blood, and add to this double the quantity of sherry wine. Let fall a thin piece of glass upon the fluids, and they will mingle in various proportions. To observe the effect of the fluids upon the corpuscles of the blood, the edges of the mixture, and not the middle of it, must be looked at. The fullest effects take place in half an hour. Dr. Addison appeals to this experiment to show that the corpuscles of blood very probably during life throw off mórbid matters into the liquor sanguinis, and thus become a source of disorder or distemperature to the fluids of the blood. He argues that symptoms of fever arise from disorder of the corpuscles. Miasms in the air, he says, affect the corpuscles of the blood; a contagious virus is generated, and this is excreted from the corpuscles into the fluid. Thus he seeks to account for the sequence between fever and inflammation: fever appears when the corpuscles of the blood are diseased, inflammation when the fluid or plasma is disordered.

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* Archives of Medicine, No. 2, p. 118.

† Ibid., part ii. p. 225.

On the Action of Salts upon the Red Corpuscles of the Blood whilst in Circulation. By Br. Botkin, of Moscow.†—The mesentery of the frog was found most convenient for watching the influence of these agents, partly owing to the want of pigment and partly by reason of the superficial bifurcation of the bloodvessels. A drop of solution of chloride of sodium (15 per cent.) being added, a change in the circulation is remarked previous to any narrowing of the calibre of arteries and veins. The interspaces between the single blood-corpuscles disappear, the corpuscles being interrupted in movement and irregularly round. In some of the smallest vessels a complete plugging up occurs, whilst in neighbouring larger ones the circulation is obviously accelerated. In a few minutes the smallest vessels plugged up begin to show a movement which extends to them from the vessels still retaining blood-movement, so that they gradually become freed from their plug of corpuscles, and in about half an hour completely resume their usual condition. If, after the formation of such a plug, one covers the preparation with water, the process of plugging is immediately arrested; and on again adding some of the saline solution the above-described changes in the bloodvessels extend to all the capillaries in the field of observation, and the capillaries of larger diameter become stopped up. The changes become observable in the veins and arteries, and in the last also an evident pulsation.

After some hours, on being left to itself, the circulation becomes re-established (firstly in the large, and then in the small vessels), but not to the original rapidity.

A stasis so produced can easily be dissipated by the addition of water; but if some of the saline solution be added in its place, the circulation in the arteries immediately ceases, probably owing to interruption of the communication between the arteries and veins by means of arrest in the capillaries. At the same time a starting movement begins in the veins, by which at each systole the blood-corpuscles move from the periphery to the centre, and at the commencement of the diastole recede in the opposite direction. Finally, this starting movement in the veins passes into an unbroken stream from the centre to the periphery, at first being very rapid, and then becoming slower and slower, and altogether ceases. The veins and capillaries appear to be filled with blood, whilst in the arteries the movement continues.

Hæmin Crystals.—L. Büchner and G. Simon contribute a rather lengthy paper upon these crystals, and their importance in a medico-legal point of view.‡ Alluding to the discovery by Teichmann in 1858, of the production of rhombic, coloured crystals in dried blood which had been subject to the action of acetic acid, a fact so highly serviceable in determining between blood-stains and other marks upon articles of clothing, wood, iron, &c. &c., the author passes on to give a precise description of the external appearance of these crystals.

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* Gulstonian Lectures on Fever and Inflammation, Royal College of Physicians, 1859: published in the British and Foreign Medical Journal for April, May, and June, 1859.

† Virchow's Archiv. Band xv. Hefte 1, 2, p. 178.

‡ Ibid., p. 50.

We are merely able to give some of his remarks on the substances with which these rhombic-shaped microscopical crystals may be confounded. Indigo, on the addition of acetic acid, gives crystals resembling hæmin crystals, as Virchow had remarked, but their clear blue colour distinguishes them. The colouring matter of sandal-wood, madder, "red ink," seedlac, and dragon's blood (and of the three first preparations, as well that with as that without chloride of sodium) contained microscopical crystals which, to the unpractised eye, might give rise to mistake. Their irregular form, at one time needle-shaped at another quadratic, their obscure outline, their colour, were sufficient to prevent any confusion. The "red ink," treated with chloride of zinc and alum, gave rhombic-shaped crystals, but they were colourless, and only here and there had a pale rose-red colour, which even the addition of water dispelled. The murexid, however, presented greater difficulty, as it yielded crystals, with or without the addition of acetic acid, which in form and colour were very like hæmin crystals. The difference was, however, established by the fact that the murexid fluid evaporated with acetic acid is of a bright brick-red colour, whereas the fluid of blood so treated is of a dingy brown-red colour. On the addition of water, the murexid evaporated with acetic acid passes into a purple-red colour; on the addition of hydrochloric acid it becomes colourless; and on addition of alkali, blue; whilst hæmin crystals are insoluble in the first fluid, and become dark-green in alkali. In a mixture of murexid and blood with acetic acid we have produced a colour less bright red than pure murexid, and brighter than blood. Water and hydrochloric acid dissolve out the murexid, leaving the hæmin crystals unchanged.

PART II.—PATHOLOGICAL MICROLOGY.

TUMOURS, MORBID GROWTHS, ETC.

Circumscribed Connective-tissue Tumour of the Liver in a Child four weeks old. By Professor LUSCHKA.*—This tumour is totally distinct from the fibroid knots of the syphilitic liver. The child was prematurely born, and from the first had a very pale-yellow skin and a prominent vein-marked abdomen. Twelve days after birth hæmorrhage from the umbilical vein came on, and returned several times; and there was vomiting of blood, with œdema of the lower part of the body. On post-mortem examination, a great want of blood was found in every organ, and the spleen and mesenteric glands were very large. The liver was of a greenish yellow colour and large, and contained the tumour at its under surface, so situated as to press on the vena cava, and almost entirely close the ductus venosus. It was round and flesh-like, and here and there contained liver substance and blood-filled vessels. On section, it was seen to be composed of two kinds of material, quite distinct from each other. The inner one, which was a kind of a nucleus of the size of a walnut, was very dry and friable, and in colour very like a "corpus luteum." It was not separated from other parts by any even boundary line, but projected more or less into it. The basis of the yellow mass proved to be a molecular detritus containing carbonates, and free fat in small drops, yellow pigment molecules and rhombic hæmatin crystals, with cell groups of altered liver cells, and occasional small roundish nucleated cells and spindle-shaped corpuscles. The outer part of the tumour was greyish in colour, and almost of a homogeneous look. Here and there existed meshwork, which only could be considered as transverse sections of enlarged liver vesicles. The predominant element was a fibrillated connective tissue, arranged mainly in broad stripes, containing roundish cell-nuclei placed in a linear way, and containing one or two nucleus-corpuscles, very visible by aid of acetic acid. The connective tissue was found often to contain dark contoured nuclei, with nucleus-corpuscles and an investing substance, drawn out at the extremities into a spindle shape. One or other elongated termination was divided or split into fibre-bundles. No formation of a cell-membrane surrounding the nucleus existed, but simply a surrounding of the cell nuclei by an intervening material, having the property of out-growing in various directions and splitting into the finest fibres. The tumour probably in the first instance originated in extravasation of blood into the parenchyma of the liver during fœtal life; there was no ground whatever for attributing it to syphilis.

On the Combination of Enchondroma and Carcinoma.—Dr. Lotzbech, of Tübingen, after quoting several instances of such examples,† from Baring, Wardrop, Schaffner, and Paget, describes a case which occurred in a man, aged fifty-nine. The tumour was in connexion with the upper lip, and was of slow growth for eight years, but for the last two years had grown very rapidly. In this case, the cartilage bodies were in many cases seen to be broken up, owing apparently to continuous formation of nuclei; the nuclei became liberated, and so underwent growth and development external to the enclosing membrane. By softening of the cartilage substance these structures became free, and subsequently metamorphosed. In this way many of the mother cells of the cancerous mass seemed to have had origin, whilst others may have arisen from a simple change of pre-existing cells. The writer considers that this endogenous "proliferation" of the contents of cartilage cells of a primary enchondroma, along with liberation of the multiplying nuclei, which undergo changes in connexion with blood-vessels, constituted the carcinoma. He concludes by observing the analogy between the cell elements of cartilage and those of areolar tissue, in which the above changes are so frequently observed, even in a pathological point of view; also by noticing how pathological productions (as, for example, enchondroma) may undergo further changes, such as are observed in healthy tissue.

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* Virchow's Archiv, Band xv. Hefte 1, 2, p. 168.

† Virchow's Archiv, Sept. 1858, p. 394.

New Bone-formation in a Fibro-fatty Tumour of Enormous Weight. By Dr. BECK, of Freiburg.—The entire growth was removed from the right thigh of a woman, aged forty-four, and had been forming seven years. It was found to be made up of a number of smaller ones; and on section a quantity of fluid escaped, containing a large number of fat drops; and in many places collections of blood and fibrin had formed, owing to laceration of the vessels and the tension of the tissue. The colour of a sectional surface varied from greyish red and yellow to a waxen colour; occasionally darker coloured parts existed. The various subordinate tumours were bound together by dense areolar tissue. On microscopical examination, the substance of the growth was found to be composed of broad, firm, and finely-fibred structure, in which were contained closely packed fibres, very like those formed directly out of fibrin; and between the fibres existed partly transparent cellules, and in part complete fat cells and fat drops. On addition of acetic and sulphuric acid and sulphuric ether it became evident that the cells contained fat and elaine. In the yellow or yellowish-white part the stroma was not so dense, and the fibres were more like those of areolar tissue, the fat cells of large size, and the whole was very like the structure of lipoma. In some places a kind of detritus existed, the fibres being destroyed, and only fat cells and drops visible. On making a section of the under part of the tumour a hard mass was met with, found to consist of a large osteoid growth weighing no less than five pounds and a half, and of the size of a child's head. This was surrounded by a thick layer of connective tissue, and situated beneath the aponeurosis, having no connexion with the bone, from which it was separated by the muscles. On section of the bony growth parts were found in which much spongy tissue existed, in the cavities of which were fat and albuminous matter; otherwise it consisted of compact bone tissue, having on its surface laminated deposits of new bone. Bone cells and canals were visible, and on addition of sulphuric acid the fibrous matrix was seen which formed the stroma of the soft part of the general tumour. It would appear as if the oldest part of the fibro-fatty tumour had become ossified, and the finely-fibred stroma converted into bone substance, and the elementary vesicles and fat cells into bone cells. The author concludes with allusion to the classification of fatty tumours which he has adopted in his work on the histology, &c., of pseudo-plasmata.

GLANDULAR SYSTEM, LYMPHATIC AND SECRETING.

Two Cases of Multiple Hypertrophy of Lymphatic Glands. By C. A. Wunderlich.*—The descriptions of these cases are given at great length, one being in the person of a man, aged twenty-two, the other of a woman, aged thirty-two. In the first case, the mucous membrane, where visible, was anæmic, and the various glands of the neck, armpits, groins, &c., were enlarged. Slight dulness existed under the clavicles, and there were cough, œdema, dyspnœa, excessive expectoration, and rapid pulse. The blood, both before and after death, was examined by the microscope, but no alteration as to the relative number of the white and red blood-corpuscles was found.

On post-mortem examination, other lymphatic glands were found very enlarged, of which some had attained the size of a hen's egg, and were variously shaped by pressure, &c. These glands were very vascular, and on section presented a bacon-like consistence; they were of a yellowish-red colour, and contained but little juice, but here and there presented dryish, greyish yellow-coloured spots, ranging in size up to that of a hazel-nut. Although very large, and closely surrounding the large vessels, they did not compress them. The mediastinal glands were so enlarged that the lungs and heart were much pressed backwards and sideways by them, but the large vessels were not encroached upon. Vomicæ were found in the lungs. A microscopical examination of the enlarged glands showed the juice of the large knots to contain a small round or polygonal cell, with homogeneous or granular contents, of the size of $\frac{1}{200}$th‴, with small rounded nuclei and nucleoli, and also a tolerable number of cells, with proportionately large nuclei. These were contained in a fluid, which underwent fibrillation on the addition of acetic acid. A small number also of middle-sized cells existed, with two nuclei, or with long nuclei and two nucleoli. The dry yellowish spots contained the same elements, but had undergone atrophy and degeneration. The stroma showed alveoli, varying in size up to $\frac{1}{20}$th‴, round or polygonal in shape, and surrounded by homogeneous or finely-fibred connective tissue, containing but few vessels or nuclei, the smallest alveoli consisting of tissue formed of a single or double row of spindle or star-shaped connective-tissue corpuscles, which in the dryest parts was wavy.

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* Archiv für Physiolog. Heilkunde, Band ii., 1858, p. 123.

In the second case, along with enlargement of the lymphatic glands in every part of the body, there was an enlargement of the spleen felt during life, and great emaciation, pallor, and pain in the head, which at one time was attended with much fever. On microscopical examination, the enlarged glands were seen to contain a large number of roundish or irregular bodies, as also amorphous, at one time striped, at another flake-like masses, the latter of which became more transparent on addition of acetic acid. The spleen was more than double its ordinary size, and was strewn with a yellowish-red deposit, corresponding histologically with the substance of the swollen glands.

The above cases are considered by the author as being neither carcinomatous nor scrofulous in character. The first case he looked upon as simply one of hypertrophy, but of the exact nature of the second he is more doubtful, inasmuch as the partly coagulated fibrin infiltrated through the texture of the organ points to a general constitutional taint.

In both these cases it is of interest to remark that no leukhæmia was observed.

Fatty Degeneration of the Sweat Glands. By Rudolph Virchow.*—This observer, considering the excessive perspiration in certain diseases—as in phthisis, for instance—might be dependent on some change or degeneration of the sweat glands, was led to examine them, especially where seated on the fore part of the chest. The result was that a highly fatty and degenerated state of the epithelium of these glands wa sfound, sometimes with enlargement of the entire gland and extension of the gland-pouch (investment). The fat, however, never reached any vêry large amount, such, for instance, as in the kidney. The excretory duct, for the most part, was free from this affection. In many instances the results of this process appeaaed to be a progressive atrophy of the glands, as is the case with many phthisical people.

THE BLOOD.

On some Modifications of Structure presented by Red Blood-globules in the Adult, in certain morbid Conditions. By Dr. C. Robin.†—The author mentions six altered states which are brought about in the globules of blood which has become extravasated:—

1. When such globules exist infiltrated amongst tissues or in a clot, or suspended in the liquid of a cyst or other closed cavity, a certain number are found which have become spherical or slightly angular, having lost their central depression. Their tint and contour are generally more decided than natural.

2. Some, whether lighter or darker than in the normal condition, are remarkable for granulations contained. These are generally discoïd, and void of any central depression, but some are spherical. The granulations vary from three to five in number, and when few, are situated at the periphery of the disc. When as many as five or six, they form a more or less complete circle. They strongly refract the light, and possess a bright yellowish or red centre, dissolving in acetic and sulphuric acid along with the rest of the globules, also liquifying in potash and ammonia; and after a period of from twelve to sixteen hours, also dissolving in plain water.

3. Globules exist which are quite colourless, having lost all their hæmatosine. These retain their form, but are rather small and delicate, and generally are mixed with the varieties before mentioned, often containing here and there spherical granulations with clear and dark outline; the outline being in some cases as it were double, that is, circumscribed by two circular parallel lines, and this is specially so in the globules which have lost colour but not become angular, and which exist in a small number. These pale coloured globules are less influenced by the action of water, and are smaller than others.

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* Virchow's Archiv, March, 1858, p. 288.

† Brown-Séquard's Journal de la Physiologie, p. 290. April, 1858.

4. In numerous cysts, especially those found often in the thyroid gland, varicose veins, vesiculæ seminales of old subjects, and in most apoplectic clots, blood globules are met with in a state of accumulation, forming ovoid, angular, and elongated masses, and attaining the size of one-tenth of a millimetre. They are always without a depression, and slightly distended, being paler than usual, and of a reddish-brown colour. Sometimes all contour is lost, and they form a more or less homogeneous mass. At times the masses are surrounded by amorphous substance.

5. When blood globules have remained for a long time in the liquid of various kinds of cysts, they assume a reddish-brown or even chocolate colour.

6. Certain blood globules which have been for a long time in acid urine, or are vomited up in diseases of the stomach, lose their colour, become pale at their centre, the central depression looking very large and transparent, whilst the periphery is very decided. They are less amenable to the influence of water and other reägents than when in the natural state.

HALF-YEARLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, AND HYGIENE.

By Benjamin W. Richardson, M.D., L.R.C.P.

Physician to the Royal Infirmary for Diseases of the Chest, and Lecturer on Physiology and Hygienic Medicine at the Grosvenor-place Medical School.

I. Toxicology.

Poisoning by Arsenic.—In a case ("The People *v.* James Stephens") recently tried in New York City, for the murder of a wife by her husband, the jury, after long consultation, brought in a verdict of "guilty." The facts of this case are of great moment. The wife of the prisoner, a robust woman weighing one hundred and sixty pounds, and forty-six years of age, was in good health until about three weeks before her death, when she complained of general indisposition and of a sensation of heat in the "chest." A physician called in at that time discovered no indication of disease sufficient to make treatment necessary; he told her she would be well in two or three days. The symptoms, however, advanced, the sense of burning being located at the epigastrium, and vomiting soon setting in. The vomiting always followed the ingestion of food or liquid after a short interval, five to fifteen minutes. A second physician was now summoned, who found the patient suffering from symptoms of severe gastritis. He prescribed as a mixture, nitrate of potassa in five-grain doses, in combination with two grains of Dover's powder. He also ordered quinine in pills in one grain dose, together with three grains of ox-gall. A large blister was applied over the stomach, and the blistered surface was dressed with an opiate ointment. These remedies were of little avail. The vomiting continued; the matter vomited was at first yellow, but gradually became green, and towards the close of the case became dark, bordering on brown, and containing spots of blood and a thick, ropy mucus, which could be lifted out of the vessel on a stick. Whatever was swallowed by the patient was rejected; nevertheless, she craved for drinks, and took a variety of fluids. During the last week of her life nervous symptoms manifested themselves. The extremities were cold, though the face was flushed; there was a hesitation in answering questions; partial anæsthesia of the hands, slight impairment of voluntary motive power and convulsive tossings of the arms. The expression of the face was languid and anxious, with a peculiar sharpness of the eye. The legs and feet were œdematous, the lips swollen; the urine was scanty, high coloured, and irritating to the urethra. Diarrhœa did not set in until thirty hours before death; the evacuations being then dark coloured and offensive. The respiration, which had before been hurried, became laboured; stupor, interrupted but once by a wild scream, set in; and the scene closed with complete collapse. In regard to the stupor, it is to be borne in mind that opium and brandy were given on the last day by the husband. One witness stated that probably three ounces of laudanum and a pint of brandy were given within a space of ten hours on the last day. Mrs. Stephens died, and was buried in the Greenwood Cemetery on the 23rd of September, 1857, no suspicion being expressed that the death was from poison. On September 24th, 1858, the body was exhumed and taken to Bellevue Hospital for official inspection. Dr. James Wood and Professor Doremus conducted the leading scientific inquiries. The body was in a remarkable state of preservation. All the viscera were well preserved except the brain, which was much decomposed; the muscles retained their redness. The mucous membrane of the stomach was hard, much harder than natural, and its veins were large, as if congested. The contents of the stomach were unusually small, and resembled coffee-grounds; the body gave out a very peculiar odour, not of decomposition. The small intestines, also well preserved, were nearly empty, but lined with a thin layer of yellowish pasty material; the large intestines were slightly reddened in parts of the colon and rectum, and contained a small quantity of a brown pasty substance. The analysis of the body was conducted by Professor Doremus, whose careful labour and conscientiousness is a lesson which deserves diffusion this side the Atlantic. He made no guesses as to quantity of poison found; he used no tests until he was assured that they were free of poison; while he even considered the prisoner as a man who, before trial, deserved to have the benefit of every scientific doubt, and against whom positive evidence should be freed from exaggeration, colouring, and invention. His anxiety for justice rather than victory led Professor Doremus to examine the whole body of the deceased, in order to present to the jury weighable amounts of any poison found. It resulted from the inquiry, that arsenic was pretty generally distributed through the organism. A quantitative analysis of the heart, a portion of the lungs, liver, and kidneys, with the small and large intestines, the spleen, pancreas, omentum, bladder, and uterus, weighing altogether 7 lbs. 3 oz., gave as a result 0·185 grain, nearly the fifth of a grain of arsenious acid. The defence at the trial seemed to have urged the old hypothesis of arsenic as a normal constituent of the body; it also suggested that the symptoms might be caused by natural disease, but admitted that there were no cases known to sustain the theory. The verdict went against the prisoner.—*The American Medical Monthly*, May, 1859.

[Granting the above to have been a case of arsenical poisoning, as some of the symptoms, especially those affecting the nervous system, would seem to indicate, the absence of the ordinary appearances of lesion caused by arsenic is not a little remarkable. The doubt naturally suggested is, always assuming the case to have been one of poison, that the long interment of the deceased—twelve months and a day—must have modified the appearances which would have been recognisable in a recent autopsy.]

Poisoning by Arsenic.—Another curious case, known as the King case, has been tried at Toronto, and has caused considerable excitement in the district. The poisoner was a medical man; the poisoned was his wife. The deceased was in good health up to the 18th of October, 1858. She was then seized with violent internal pains, burning sensation in the throat, and retching, which, with other symptoms of poisoning, continued until the 3rd of November, when death occurred. There was, however, throughout, no purging. After death the stomach was found engorged, in an early stage of inflammation. The intestines were coloured and the rectum was coloured, an effect also attributed to early inflammation. The peritoneum was dark over its entire surface. The lower part of the right lung was slightly congested; the liver was hard; the uterus contained a fœtus of from four to five months. It was proved in evidence that Dr. King had repeatedly administered to the deceased a white powder, which she said was "fiery tasted." This administration caused vomiting of dark-greenish matter. The severe pain was felt only during the vomiting. On analysis, Professor Croft found no less than eleven grains of arsenic in the stomach and its contents, and smaller quantities in the liver. On the side of the defence various questions were raised. The lady had fallen from a buggy nine weeks before her illness, and this accident was noticed as accounting for the symptoms. The occurrence of the vomiting as incident to the pregnant condition was contested. The absence of symptoms indicating disturbance of the bowels was dwelt on as opposed to the known effects of arsenic. It was argued that the poison found might have been introduced into the stomach after death. It was urged that the patient had taken arsenic in homœopatic globules, and that this might account for the presence of the poison. The jury found the man guilty.—*Toronto Weekly Globe*, April 8th, 1859.

[As yet we are not aware that any scientific history of this case has been recorded. Dr. Croft, from his evidence, clearly formed an opinion that this was a case of *acute* arsenical poisoning, and that the poison could not have been taken into the stomach long before death. In this opinion we entirely concur. From the facts that there were no morbid appearances of natural origin to account for death; that the prisoner had poison; that he was seen to mix and administer white powder; and that poison was found in abundant quantity in the contents of the stomach and in the tissues; the chain of evidence against the accused was complete. He died confessing himself guilty.]

Acute Poisoning by Phosphorus.—Dr. Lewinsky records a case of poisoning by phosphorus, in which the symptoms and pathology were most peculiar. A girl, twenty-two years old, and healthy, was brought to Dr. Lewinsky on the 19th of November, 1858. She stated that on that and on the evening of the previous day she had taken a portion of phosphorus scraped off from a small packet of lucifer-matches, for the purpose of self-destruction. Soon after taking the phosphorus, she felt a sharp burning pain in the abdomen, followed by vomiting. Her sister observed a luminous appearance of the ejected matter during the act of vomiting. When admitted under Dr. Lewinsky, the patient was suffering from vomiting and diarrhœa, but no smell of phosphorus was perceptible in the excretions; the vomited matters were of a dirty-grey colour, and mixed with mucous flakes. The abdomen was somewhat swollen, and was sensitive on pressure. The tongue was white and moist; the pulse normal; the mind clear. Analysis of the evacuations by Dr. Folwarezny indicated bilious substance and phosphoric acid. Magnesia was given internally, and portions of ice; ice was also applied externally to the stomach. Vomiting, alternating with hiccup, continued unceasingly on this and the two following days, but the diarrhœa ceased on the second day, and the mind continued undisturbed. Sleep was interrupted. There was no fever. On the third day there were signs of jaundice; the urine was scanty and of dark colour, and the pupils were widely dilated and reacted feebly to light. On the next day (the fourth) the jaundiced appearance of the face was much increased, collapse and great restlessness, with extreme thirst and weak and quick pulse, were present, but the vomiting had abated, a small quantity of blood only being thrown up. In the evening, these symptoms were intensified, and great sleepiness presented itself. In the night there were convulsions, and the consciousness was impaired. At the same time, a little clear red blood was vomited. The death of the invalid occurred on the sixth day after the taking of the poison.

The post-mortem appearances presented evidence of extensive changes. The brain structure was free of blood, but the ventricles contained a drachm of serum. The throat cavity contained a bloody frothy mucus, and the same extended into the bronchial tubes. The right lung was fixed by infiltrated plastic matter; the left lung, together with the costal pleura, was covered with flaky exudation; the visceral pleura was suffused in large spread patches. In the pericardium there was half an ounce of serum; the heart was ecchymosed at its base and was contracted, its muscular structure being of fawn colour. In the cavities there was fluid blood with a little loose coagulum of fibrin. The liver was large and very fatty. The gall-bladder contained a little bile mucus. The spleen was hard and friable. The stomach was filled with gas and with a blackish-brown fluid; its mucous coat, raised from beneath, was covered with a thick mucus streaked with dark brown lines. The intestine and even the small intestine contained a blackish-brown thin frothy fluid. The kidneys were large, pale, fawny, and fatty. The bladder was contracted and empty. The ovaries were small, charged with follicles, and contained extravasated ova. The uterus was large, and its cavity was lined with a bloody mucus.

A chemical examination of the stomach and its contents, by Dr. Schauenstein, showed no indication of phosphorus.

In commenting on this case, Dr. Lewinsky remarks that in this, as in a similar case recorded by Dr. Nitsche, the symptoms and pathology show much more evidence of an influence exerted on the stomach and intestines than is commonly found in instances of phosphorous poisoning. He infers that this difference may have arisen from the transformation of the phosphorus into phosphoric acid, or from its combination with organic substances, and from its rapid absorption, in one or other of these combinations, into the blood. He further suggests that the peculiar appearances in the liver and kidney could scarcely be set down to the effects of the phosphorus. These must have been of a chronic nature, although the deceased was reported healthy. Lastly, he opines that magnesia as an antidote for phosphorus is of no effect, inasmuch as the local effect of the poison on the stomach is too rapid for the remedy to be effectively applied.—*Zeitschrift der k. k. Gesellschaft. der Aerzte zu Wien*, Dec. 27th, 1858.

On Poisoning with Sulphocyanide of Potassium.—Dr. Setschenow, in a paper on this subject, supplies the following inferences as to the action of the sulphocyanide on animal bodies; in his experiments he used frogs as the animals to be operated on, except in one instance, when a rabbit was selected.

(*a*) The sulphocyanide introduced into the stomach acts as a poison; causes decrease of the muscular irritability, and destroys life.

(*b*) The sensibility of the skin in poisoning by sulphocyanide, either by introduction of the poison into the stomach by the mouth, or by injection of it under the skin, decreases more quickly than the motive power of the animal.

(*c*) Brought into contact, in solution, with muscular structure, it does not produce direct paralysis of muscle.

(*d*) The symptoms induced by this salt when introduced into the body, prove that the action of the poison is primarily exerted on the brain, then upon the spinal cord, and through these nerve centres on the organs of locomotion and sensation.

(*e*) When the heart has ceased to beat, its action may be excited for a time by external irritation; but this action or irritation ceases much sooner in the heart than in the voluntary muscles in cases where the poison has been inserted under the skin. On the contrary, in cases where the poison was introduced by the mouth, the heart, even to the period of three hours after the administration, and when the animal was reactionless (*reactionslos*), may be excited to contraction. The author therefore infers that there is a difference between the two modes of poisoning. In most instances the heart ceases to act during the diastole, and is left full of blood.

In one instance noted by Dr. Setschenow, he injected a portion of concentrated solution of the sulphocyanide into the external jugular vein of a rabbit. Death was almost momentary, with convulsions. A quarter of an hour after the operation the heart was found rigid, and the muscles of the neck and of the hinder extremities were also found rigid. In this example it was admitted by the experimentalist that by the direct action of the poison on the muscles, their paralysis and death result. In this respect, therefore, Bernard's view is admitted as correct; but Setschenow qualifies the admission by trying to prove that there is a difference between absence of irritability of muscular fibre and actual death.—*Archiv fur path. Anat. und Phys.*, von R. Virchow, Sept. 1858.

Poisoning by Cyanide of Potassium.—Dr. A. Schauenstein, in a communication on poisoning by cyanide of potassium, gives an account of five cases of death by the cyanide. The author, who is a judicial chemist, comments upon the great increase of deaths by suicide through the agency of this poison. Thus, in Vienna, from 1851 to 1856, only two poisonings were noted, one of which was doubtful; while from August, 1857, to December, 1858, no less than five cases came under the personal observation of the author. In proportion to this increase of deaths from the cyanide there was a corresponding decrease of deaths from arsenic.

Dr. Schauenstein relates at length three of the cases observed, and in brief the pathology of the two others. In all cases the death seems to have been sudden. In one case, in a young girl, strong tetanic spasms came on directly after the poison had been taken, and death took place in less than an hour. In the second case occurring in a young man, death took place almost instantly, and with no striking symptoms. The third case was similar; no note of the symptoms in the remaining two cases is given, but Dr. Schauenstein observes, that in several of the cases death took place suddenly, as in apoplexy.

In all the cases a post-mortem examination was conducted, but the appearances observed are considered by the author as offering nothing very characteristic. They were:

(*a.*) The brain containing more or less blood.

(*b.*) The blood in the cavities of the heart dark, and of thick consistency.

(*c.*) The condition of the stomach various. In one case the mucous surface presented no particular colouring. In the case where life was prolonged nearly an hour the mucous membrane was slightly red, but offered no other extraordinary appearance. In another case, the death being very sudden, the mucous membrane was of a dark red colour, swollen, and in places covered with numerous bloody points; the contents of the stomach were also of blood-red colour; the two remaining cases of the five presented similar appearances in a less degree.

(*d.*) The smell of prussic acid in the stomach was very evident in four of the cases. But in one case, on account of the quantity of undigested food in the stomach, the smell remained hidden entirely.

(*e.*) The reaction of the contents of the stomach was strongly alkaline, and in every case chemical search proved without doubt the presence of prussic acid; but constantly formic acid was also found, showing that prussic acid in the stomach is transformed into formic in many cases.

[This latter fact, one of great interest, was originally pointed out by Dr. Schauenstein in the 'Wochenbl. der Zeitschrift der k. k. Ges. der Aerzte,' No. 3, 1857.]

Dr. Schauenstein, in commenting on the cases, opines that there are no true and distinguishing pathological indications by which the effects of the poison can be safely pronounced. He further observes that the chemical detection may become equally difficult in instances where, from the body having been dead several days, or having undergone a rapid decomposition, the poison has been decomposed.—*Zeitschrift der k. k. Gesellschaft. der Aerzte zu Wien*, No. 1, Jan. 8, 1859.

Researches on some New Varieties of Woorara.—Messrs. Hammond and Mitchell have recently made some very valuable inquiries on two varieties of arrow poison, named "corroval," and "vao," or "bao." These poisons, while apparently allied to the variety of woorara which has been experimented on by European physiologists, differ materially in respect of physiological action from the better known substance. "Corroval," when in lumps, is of brownish black colour. Reduced to a fine powder it becomes a tawny yellow. Its taste is intensely bitter and persistent. Both water and alcohol extract the active principle. The aqueous solution mixed with blood does not retard coagulation, nor alter the shape of the blood discs more than any other blood fluid of similar density. It is not poisonous to plants. The extract yields an alkaloid which dissolves in alcohol, ether, and chloroform; and forms salts with nitric, hydrochloric, and sulphuric acids. The alkaloid is not crystallizable, nor are its salts. The authors propose to name the alkaloid "corrovalia."

The writers deduce the following conclusions from their experiments: 1st, Corroval differs essentially from any variety of woorara hitherto described, both in its chemical constitution and physiological effects; 2ndly, It acts primarily upon the heart through the medium of the blood, producing an arrest of the action of that organ; 3rdly, The annihilation of the voluntary and reflex movements is a secondary result of its action, depending primarily upon the discontinuance of the function of the heart; 4thly, It acts upon the nerves from the periphery to the centre, and abolishes both the sensory and motor functions; 5thly, It destroys muscular irritability; 6thly, It paralyses the sympathetic nerve, this being one of its primary effects; 7thly, It is absorbed both from the intestinal canal and skin of frogs; 8thly, Its poisonous qualities are due to an alkaloid hitherto undescribed.

Vao or Bao, the second specimen of woorara described by Hammond and Mitchell, is a dark brown extract, perfectly dry and hard, and partially soluble in water and alcohol. On experiment an alkaloid was obtainable, having all the qualities of corrovalia. Physiologically, the vao poison seems identical in action with corroval, vao being merely a weaker variety. The point of most interest in relation to these two poisons is that, contrary to the known action of the poison known by the same name in this country, these put out life by a primary effect on the circulation. Our readers will remember how, in the interesting experiments of Brodie, Waterton, Sibson, and others, animals poisoned with woorara were kept alive for hours by artificial respiration, and recovered at last on the elimination of the poison, the heart remaining true to its function throughout. The new varieties of woorara (*Corroval* and *Vao*) exert the opposite influence. The heart stops first and respiration continues, while the blood is not rendered permanently fluid. Hammond and Mitchell opine that there is no antidote for this variety of poison; they consider that both poisons are of vegetable origin, but that no form of strychnia is present in either.—*American Journal of Medical Sciences, July*, 1859.

Action of Strychnine and Nicotine.—In two papers, published in the 'Proceedings of the Royal Irish Academy,' November 29th, 1856, and June 28th, 1858, the Rev. Professor Haughton draws attention to the opposite character of the specific actions of strychnine and nicotine on the muscular system. From the well-known facts that strychnine produces tetanic contraction of the muscles, whilst nicotine is a most powerful agent, whether administered in the form of tobacco-smoke or of infusion of the leaves, in relaxing muscular action, Mr. Haughton was led to believe that these poisons might be used as antidotes to each other, and with a view of testing the truth of this conjecture he made a series of experiments. These experiments were made by introducing frogs into separate solutions of nicotine and strychnine, and into similar solutions mixed together. He arrived at the conclusion that nicotine has the action of retarding, and in certain cases of completely counteracting, the effects of strychnine. In one experiment a frog had lived for forty-seven minutes in a mixture of two solutions, of which one would have destroyed life in four minutes; the other would have produced paralysis in one minute, and destroyed life in twenty-three minutes; and yet, in the mixture, the animal lived forty-seven minutes, and afterwards for twenty-four hours. In another experiment the frog, immersed in a similar mixture of the poisons for ten minutes, ultimately recovered, the effect of the strychnine being completely obviated by the action of the nicotine. Since the announcement of Mr. Haughton's results, Dr. O'Reilly, of St. Louis, Missouri, acting upon the suggestion, has successfully treated a case of poisoning by strychnine by the administration of infusion of tobacco leaves. The patient had taken six grains of strychnia; an emetic had been administered which had produced vomiting, but notwithstanding, the usual symptoms were fully developed. Dr. O'Reilly writes: "One hour and fifteen minutes after he had taken the poison we gave him the first dose of the tobacco infusion, which he swallowed with difficulty. We continued it in table-spoonful doses at intervals of five minutes, until he had taken half the quantity infused, before we had noticed a favourable change. Then the muscles became relaxed, the spasms less severe, and the intervals between them longer; and so conscious was the patient of relief, that he constantly called for the tobacco juice when he found the paroxysm approaching. This encouraged us to persevere with the infusion, prolonging the intervals between each dose as the frequency of the spasms abated; until finally, after twelve hours, they disappeared, leaving him in a state of fearful nervous prostration, from which he recovered in a few days under a careful tonic treatment. The whole quantity of tobacco used in making the infusion was one ounce and two drachms.

Poisonous Effects of Lead.—Dr. Lauder Lindsay, in a paper 'On the Action of Hard Waters upon Lead,' read before the Chemical section of the British Association, Leeds Meeting, September 24th, 1858, arrives at conclusions with reference to the action of waters on lead somewhat different from those which have been hitherto generally held. From observation, experiment, and inquiry he deduces the following results: 1. That certain pure or soft waters *do not* act upon lead. 2. That certain impure or hard waters, in some cases containing abundance of the very salts which are generally regarded as most protective or preservative, *do act* upon lead. 3. That the rationale of the action in these anomalous or exceptional cases is very imperfectly understood. 4. That experimentation on the small scale, and for short periods, is most fallacious, and frequently dangerous, in regard to the conclusions thence to be drawn. 5. That water may, under certain circumstances and to certain extents, contain lead without necessarily being possessed of appreciable poisonous action on the human system. 6. That water contaminated with lead may deleteriously affect certain members or individuals only of a community, family, or household. 7. That the use of water so contaminated is the obscure cause of many anomalous colicky and paralytic affections. From this paper we select the following points of interest:

"The use of leaden covers to cisterns is fraught with great danger, and fortunately is comparatively seldom resorted to by plumbers. If cisterns are covered at all, probably the safest material is wood. However impure or hard a water is, however saturated with sulphates and carbonates, and however free from plumbeous impregnation, the moisture which results from its evaporation, and which condenses on the leaden cover, is pure or distilled water. Hydrated oxide and carbonate are produced in the ordinary way; these gradually encrust the lead, and by and by scales or fragments fall off and drop into the subnatant water, in which they are partly dissolved, partly held in a state of suspension.

"Time is a most important element or condition in the action of water on lead. The mere flowing of water through leaden pipes, and the standing of water for a long period in leaden cisterns, are two very opposite conditions. Accordingly it is found that the same water which will cause no perceptible change on lead from short contact, will produce a copious deposit of oxide and carbonate on standing for some weeks or months.

"An equally important element in the action is, perhaps, the extent of surface exposed. It not unfrequently happens that experiments on the small scale give erroneous, and consequently dangerous, results, merely because the surface exposed was too small and the time allowed too short."

Dr. Lindsay admits the effect of galvanic action in influencing the corroding power of water on lead, but he thinks too much has been attributed to it by some writers on the subject. He says: "Galvanic action in connexion with the corrosion of lead seems to be a most hopeful field of research. It is generally called into requisition to explain all the anomalous causes of the action of hard waters on lead. But this is making too much of it. I have no doubt that it exerts a powerful influence in many cases, but it has yet to be distinctly shown in what cases, how, and under what circumstances it operates."

From an experimental examination of various waters—viz., of well waters decidedly hard (i.e., containing carbonates, sulphates, and chlorides abundant, lime and magnesia also abundant), of spring-waters, mostly hard (presenting similar chemical characters); of surface or drainage water; of river water decidedly soft (containing the same saline impurities but in smaller quantities); and of rain waters, very soft (containing only traces of the above salts), Dr. Lindsay found that they all possessed some action on lead; though only in the cases of the rain waters was this action apparently dangerous in amount or degree.

SUMMARY.

Poisoning by Hydrochloric Acid.—An instance of poisoning by hydrochloric acid is recorded in the 'Lancet' for July 16th, 1859. The patient was a feeble woman, aged sixty-three. She took half a fluid ounce of concentrated hydrochloric acid for the purpose of suicide. The symptoms as noticed by Dr. Cayley, of King's College, were vomiting, coldness of skin, whitening and abrasion of the mucous surface of the lips, tongue, and fauces; rapid and feeble pulse, and burning pain in the throat and stomach. The fauces became swollen, and vomiting of matter containing blood and shreds of membrane was frequent. She retained consciousness to the last. She died eighteen hours after taking the poison. There was great rigor mortis: the lungs were engorged; the heart was healthy, but all its cavities were filled with black clots; the liver was enlarged; the gall bladder distended. The abrasion and distension of mucous surface was confined chiefly to the mouth and fauces. The stomach was comparatively little affected.

Abortion produced by Phosphorus.—The 'Journal de Chimie,' and 'American Medical

Monthly' (June, 1859), refer to a communication made by the Abbé Mignot to the effect that pregnant women inhaling the vapour of phosphorus are extremely liable to abort, and that in localities where the manufacture of matches engages a large number of workpeople, the women profit by the circumstance alluded to to rid themselves of the products of conception. The same author states that men working in phosphorus are subject to extreme excitation of the generative functions. Chevallier has the subject now under examination.

Symptoms of Poisoning by Potato Berries.—Mr. Morris, of Merford, records, in the 'British Medical Journal' for September 3rd, an instance of death in a young lady of fourteen, from eating the berries of the potato plant. The symptoms are thus described: "She was tossing to and fro in bed; the skin was bedewed with a cold clammy perspiration, and was of a rather livid colour; respiration was hurried; the pulse was exceedingly quick and feeble; the teeth for the most part were closed, and she was constantly spitting through the closed teeth a viscid frothy phlegm. She was speechless. She would, after repeated asking, put out her tongue, which was covered with a dark brown moist fur. She occasionally asked for drink, and begged her mother not to leave her. The eyes for the most part were open, and the pupils were not much dilated. The expression was anxious. She occasionally slumbered for a few minutes, and again was restless." Death took place on the second day.

Arsenic, Fallacy of Sources of, in Dead Bodies.—Dr. Lois records the fact of his having made examinations of various specimens of brass. He discovered arsenic in ten specimens of brass, and in some specimens in large quantities. He considers this point as of importance to the medical jurist, inasmuch as various brazen ornaments—such as medals and rosaries, which are laid so frequently in coffins—may by undergoing change in the presence of the products of decomposition, supply to the remains the poison which a chemist may be instructed to search for in the exhumations of persons who have been thought to die from poison.—*Oesterr. Zeitschrift. f. pract. Heilk.*, No. xlix., 1859; and *Vierteljahrsschrift f. prakt. Heilk.*, No. xvi., 1859.

Poisoning by Sulphate of Zinc.—Dr. Ogle gives the record of a case of this kind in the 'Lancet' for August 27th, 1859. The poison was taken for the purpose of suicide, and seems to have been swallowed in frequent doses for a week. On the evening before death the patient was very tremulous and apparently delirious, and shortly before going to bed he was violently sick after taking brandy and water. He was found in his bed in the morning with his throat cut, and died whilst being conveyed to St. George's Hospital. The mucous surface from the mouth to the stomach was slightly congested, and its surface was thickened in patches and of a greyish-white colour. The tongue was pale and shrivelled. The stomach was contracted, and contained about an ounce of whey-like fluid; the whole of its inner surface was of a nearly uniform dirty-grey colour, the mucous membrane being very greatly thickened, condensed, and indurated, and altogether having a singular appearance, very similar to that of a piece of tripe. The duodenum had the same appearance in a less degree. The colon and rectum were contracted, but otherwise were healthy. There was general congestion of the visceral organs, and a fluid, dark condition of blood. On analysis, sulphate of zinc was readily detected.

II. Miscellanea.

Case of Rape committed on a Child.—Dr. D. M'Kinlay relates the particulars of a case of rape committed by an adult on the person of a child of less than seven years of age. The injuries produced on the person of the child were remarkably extensive. They are thus described by Dr. M'Kinlay: "At the upper part of the cleft of her buttocks, behind and above the anus, the skin was besmeared with dried blood. In her private parts the vagina was lacerated in various directions. One laceration extended from the lower part of the vagina downwards, dividing the recto-vaginal septum and perineum on the left side of the raphe, down to the verge of the anus, and laying bare the rectum. Another laceration, having divided the upper part of the vagina, passed upwards along the left nympha, and terminated near the root of the clitoris. The vagina was also lacerated laterally, so that for an inch and three-quarters inwards there appeared an open lacerated cavity or pouch rather than the semblance of a continuous canal.

"In the cavity produced by the laceration there was some feculent matter. This was found to have come through a lacerated opening in the coats of the rectum. Three-fourths of an inch inwards from the verge of the anus there was a lacerated opening in the rectum of nearly an inch in length, through which the fæces escaped into the lacerated cavity before described. This opening had evidently been produced at the same time with the other parts of the laceration, and was not owing to any subsequent sloughing of the parts."

On the clothes of both the accused and the little girl stains were found in which, by the microscope, blood-globules, ephithelial scales, and spermatozoa, were detected. In the course of the treatment of the case it was found necessary to divide that portion of the recto-vaginal septum which extended between the verge of the anus and the laceration. It appears that the child gradually recovered, although for some time the action of the sphincter was not established.—*Glasgow Medical Journal*, July, 1859, p. 135.

Blood Crystals, and their Judicial Medical Importance.—Büchner and Simon, in a valuable paper on blood-crystals, give as characteristic of these the subjoined particulars. They usually form rhomboidal tables, but seldom rhomboidal columns. When somewhat imperfect, they take the form of a shuttle. Their colour changes from yellow to black, but most frequently they are of dirty red-brown. Their size is various; they generally lie in groups together, and often lie crossways, form the X figure, or they take the stellate form. Forms of needles, rods, or grains are of little value in a forensic point of view. The crystals are insoluble in water, alcohol, acetic acid, phosphoric acid, and hydrochloric acid. They dissolve with difficulty in ammonia, in dilute sulphuric acid, and in dilute nitric acid; but are easily soluble in potash water, English sulphuric acid, and in the vapour of nitric acid. In chlorine water they lose their colour, and look as if corroded. For the demonstration of the crystals, a very small quantity of blood, or of fluids tinged with blood, is required, but an excess of concentrated acetic acid is necessary. The character, age, or impurity of blood, in no way hinders the crystallization. To obtain crystals, a drop of fluid blood is to be mixed with a small excess of concentrated acetic acid, and evaporation is slowly made over a spirit lamp; in the dried matter remaining the crystals will be recognised by the microscope. In cases where the blood which has to be examined has been robbed of its salts by washing, rain, or moisture, as may happen, crystallization will not occur by the simple process given above. Under these conditions it is necessary to add a few grains of common salt, when the capability of crystallization is restored. It is, perhaps, after all, best in forensic inquiries to use the salt on every occasion. A source of fallacy may enter inquiries of this nature, for it has been found that madder, red ink, dragon's blood, and some other substances, will also form crystals. Crystals thus formed may be distinguished by their want of colour, irregular form, solubility in water, and different chemical reactions. Murexide offers the greatest difficulty in determination, since it forms crystals similar to the hæmatin crystals, both in colour and form. Solution of murexide gives, however, with acetic acid, a residue after evaporation of a clear brick-red colour. This residue, covered with water, dissolves, giving a purple-red colour. Addition of hydrochloric acid destroys the colour, and potash causes dissolution with a blue colour. Blood-crystals, on the other hand, are insoluble in the first, and are dissolved in potash with a green colour.

Professor Bryk, of Cracow, has also an original article on the same subject, in which is supplied information similar to that embodied in the paper of Büchner and Simon. In some minor points he differs from those authors; in other points he is more elaborate, and he introduces certain new facts of value. He recommends the acetic acid plan as the best for producing the crystals, but thinks the addition of salt superfluous, and that the presence of salt crystals is apt to deceive. In cases where blood cannot be scraped off an object—i.e., where the blood is imbedded in a soft substance, the spots, as Bryk directs, should be steeped in distilled water; the blood-colouring stuff sinks in streaks and flakes, and gives to the fluid a brown-red or brown-greenish colouring. The maceration is perfected in from ten to twenty-four hours. The solution, protected from the air by a glass cover, is evaporated to dryness in the sand-bath, great care being taken that the mass does not become carbonized. When quite dry the blood is treated with acetic acid for crystallization. This author states that spots of blood left by fleas and bugs yield no crystals, and that if purulent matter be mixed with blood there is a granular pigment formed, but no characteristic crystalline substance. Blood spots left on wood are retained differently. If the wood be smooth and hard, the blood retains its power of crystallization; if the wood be soft and the blood soak deeply into it, one can obtain for the first six or eight days, by maceration, a solution of blood-colouring stuff which will yield the crystals. After six to eight weeks, especially in soft kinds of wood, the crystal formation no longer is possible, owing probably to the formation of a tannin albuminate, which is insoluble. On iron without rust, dried blood retains its capability of crystallization; it loses it, however, if rust form at the same spot. Blood left on clay or chalk retains also the power of crystallization, unless it is thinly laid on and has been long exposed to the influences of the weather. In instances where linen substances spotted with blood have long macerated in water, so that the blood-colouring stuff is in a great measure extracted, Bryk recommends that the linen be dried and then moistened with acetic acid; on adding further a solution of potassa, a green colour is given to the threads of the fabric.—*Vierteljahrsschrift f. d. prakt. Heilk.*, *Jahrgang* 1859; Virchow's *Archiv*, Band xv.; and *Wien Wochenschrift*, 1858, Nos. 42, 45.

The Smethurst, Case.—The necessary limitation of space prevents us from noticing in the briefest terms upwards of forty other cases of forensic interest which have appeared since our last report in the British and Foreign Journals. We must, however, for the sake of our Continental readers, say a word respecting the famous Smethurst case, which in this country has for some months been the leading medical topic. This case was one in which poison was suspected; but as there was no evidence of poisoning as the cause of death, save in the imagination of men who have gifts in that direction, we could not logically put the case in our toxicological section. The essence of the case is that a woman, forty-three years of age, and having premonitory indications of abdominal disease, became pregnant. That upon the pregnancy there resulted vomiting, an aggravation of the intestinal disturbance and dysentery. In great measure from starvation (owing to the inability of the stomach to retain food), to which must be added the exhaustion produced by the dysenteric disease, death was the result. *Facilis descensus averni:* the medical philosophers who attended the case, not appreciating the meaning of the previous history of the woman, ignoring altogether the fact of pregnancy, and mistaking the symptoms which peculiarly mark starvation, came to the hypothesis that the case was one of slow irritant poisoning, Nevertheless, they took no pains for many days—not indeed until death was at the door—either to remove the poisoner, to look for poison, or to give an antidote. On the contrary, they prescribed for the case as one of ordinary disease almost to the end. When, granting that the case had been a case of poison, it was too late to do anything, the supposed poisoner was arrested, the antidote was supplied, and the poison was searched for. Suffice it to add, that no poison was found in the possession of the prisoner; none *satisfactorily* in the body of the patient after her death; that the post-mortem appearances were admitted to be dysentery; and that the fact of the existence of pregnancy was revealed by the knife. In spite of all, the vague hypothesis was adhered to. A man was tried for his life, and a jury, overpowered by the dogmatic, and, we may add, stereotyped statement of the scientific witnesses for the prosecution; to wit, that death could only be attributed to irritant poisoning—condemned the prisoner to the scaffold. Since, the condemnation the country has risen up against it, and the Secretary of State for the Home Department, than whom a more logical mind is not in this realm, has granted a reprieve from death. The nation is thus happily relieved from a national sin, and English science rescued from indelible disgrace.

Another remarkable case of suspected poisoning by arsenic, antimony, or both, has happened in Germany. A woman died, as it was supposed from poison. Portions of her body were examined by two chemists, who found, or thought they found, *arsenic*, but no antimony. Other portions were examined by two other chemists, who found, or thought they found, *antimony*, but no arsenic. The whole evidence was then submitted to the distinguished Fresenius, who in an elaborate report, conspicuous for its clearness and science, showed that there was no chemical evidence whatever of death from poison.

III. HYGIENE.

Building of Hospitals.—Dr. Mackenna, in a paper on the Hygienic Requirements of Hospitals, gives the following advice to his fellow-colonists: "As it is not likely that we shall in this colony require another large hospital, I shall pass on to enumerate the advantages that smaller ones ought to secure. For a population of eight or even ten thousand, I think one of fifty beds should answer. The site, aspect, and drainage should be the best it is possible to procure; the ventilation should be, as it could easily be, made perfect; the wards should hold six or eight beds; the ceilings should be eighteen or twenty feet in height—as I am convinced that the higher a ward is the better, and that no extension in other directions can compensate for it. The impure air should be drawn gradually off by two or more of Dr. Chowne's syphon tubes of large size, the exterior opening of which should be controlled by a cover and vane to prevent downward drafts in hot winds. The ventilation carried on by the aid of louvres and other permanent openings in the roof is liable to the objection that the hot winds force their way downwards, and instantly expel the cooler air, to the great danger of delicate patients. This can be remedied by changing them to a common tube controlled as above. A cold chamber could, and indeed should, be made in the underground, which by a simple arrangement of evaporating surfaces would procure for one or two, or more chambers above, a supply of air several degrees colder and moister than the outside temperature by which many an otherwise fatal disease might be effectually checked. No kitchen or other office which could in the slightest degree tend by its ascending odours to taint the air of the wards or passages, should be tolerated, and every care should be taken to keep this story thoroughly ventilated. The passages and wards should be flagged with glazed tiles set in cement, which do not retain the moisture after being washed, nor imbibe impurities, and the walls and ceilings should be coated with a glaze of silicated paint. A permanent verandah should face those aspects much exposed to the sun, and the windows should be protected by kuskus tatties. No hospital should have more than a ground and upper story, or be without a pair of wards apart from the house for noisy or unruly patients. Lifts and Arnott stoves (for occasional use in winter) are indispensable in every hospital, and the wards should communicate with each other and with the offices by speaking-tubes. Every mechanical improvement in baths, water-closets, and especially those for the removal of patients before and after operations, accidents, &c. &c., should be provided."

Effects of Swill Milk.—A report on milk, by Dr. Percy, read at the Academy of Medicine, New York, introduces us to what is called swill milk, or milk yielded by cows fed on swill, the said cows consuming in the daily swill potations not less than a gallon of vinegar per day. The milk yielded has a strong acid reaction, is deficient in butter and sugar, and, according to the observation of Dr. Percy, is quite insufficient for the purposes of life. It is also said to be wanting in a peculiar phosphoric organic compound which has been found in butter, and is described by Gobley, Treacy, and others. The effect of this milk on children fed with it is thus described: "It is not found that this milk, as given to children, actually in all or many cases sickens them at the time it is given; but the child, though inordinately voracious, is starved and poisoned by slow degrees. The nervous system becomes irritated beyond endurance, the vitality is undermined, and the child dies of marasmus, bowel complaints, cholera morbus, dropsy upon the brain, or kindred diseases."

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.

Fellow of the Royal College of Physicians, Physician to and Lecturer on Materia Medica at St. Mary's Hospital.

I. *Some Remarks on the Epidemic of Diphtheria* (angine couenneuse) *of* 1857 *and* 1858. By Dr. Bouillon-Lagrange. (Gazette Hebdomadaire, Nos. 23, 25, 27, 28.)

The author's paper is based upon an observation of 73 cases of diphtheria, 51 of which were cured, 22 having been fatal. His general conclusions are, that we are still far from being acquainted with the real nature of the disease. He entirely differs from the views of Bretonneau, who considers that under all circumstances the diphtheritic germ is sown locally, like that of syphilis, forgetting, as the author justly observes, that wherever the virus may be deposited, at least with very rare exceptions, the throat, the tonsils, or the nasal fossæ are the parts in which the diphtheria is first manifested. Dr. Bouillon-Lagrange admits the possibility of contagion, but denies the necessity of this element in the propagation of the disease. He holds that we are nearer the truth in regarding the disease as one primarily constitutional, manifesting itself on the mucous membrane, in the same way as the eruptive fevers are characterized by a rash. This view, he adds, may be less attractive than the one accounting for the propagation by local contact, because it deprives us of the excuse for employing heroic remedies. Pathological anatomy, chemistry, the microscope, have not, according to our author, thrown much light upon the study of diphtheria; the first has only confirmed what clinical observation had previously taught regarding the state of internal organs; it has also demonstrated the liquid state of the blood and the internal congestions which result from this condition, and the asphyxia produced mechanically by the false membranes in the air-passages. Dr. Bouillon-Lagrange urges the propriety of paying more attention to the lymphatic system, which he believes to play an important part in the disease. He concludes by calling upon chemists and microscopists to study the state of the blood at different stages of diphtheria, which "may be the sole means of obtaining a knowledge of the morbid state which certainly precedes the primary modification of this fluid, before the profound alteration unavoidably entailed by the enormous deposition of fibrine upon the mucous membranes."

II. *Diphtheritis: the Epidemic Malignant Sore-throat prevailing in Albany, New York.* (American Medical Monthly, December, 1858.)

It appears that our American *confrères* are also surprised by the appearance of diphtherite, an epidemic of which prevailed in Albany, a town about one hundred and fifty miles north of New York, during the end of last year. We gather from the brief summary of a meeting of practitioners in that town, that a large number of the inhabitants were attacked. One physician had had forty cases, of which four were fatal, "the larynx, trachea, and bronchia were found lined with a diphtheritic exudation, readily separated from the mucous membrane, which was found entire, except upon the tonsils, which were excavated, enlarged, and much congested. The secretions were copious, offensive, and very acrid. In one fatal case gangrene of the uvula had taken place." During the prevalence of diphtheria, scarlet fever occurred very rarely. Gargles containing chlorates of potash and soda or vinegar, the mineral acids, and tonics internally, constituted the prevailing treatment. At the time of the meeting, in November, 1858, the disease appeared to be on the decline.

III. 1. *On the Presence of Strongylus Gigas in the Urinary Organs of Man.* By Dr. J. Lehocq. (Archives Générales, June, 1859.)

2. *On the Development of the Larvæ of the Lucilia Homini-vorax in the Pharynx.* By Dr. Coquerel. (Ibid.)

The first of these papers gives an interesting account of a very clever fraud perpetrated for a series of years by a young woman upon a large number of medical men, who believed that she was in the habit of passing strongyli from her urethra. Suspicion, however, was aroused, and at last Dr. Lehocq, by the aid of M. Robin, succeeded in determining that all her symptoms were the result of her own manipulation, and that on the last occasion at least, when she was supposed to have passed a strongylus, the parasite proved to be portions of a pigeon's intestine. The case deserves a place in the memorabilia of medical experience.

The second paper gives another instance of the development of a peculiar parasite in the human body, which Dr. Coquerel brought under the notice of the profession some time ago.* It occurs in Cayenne, and results from the deposition within certain cavities of the human body of the eggs of an insect hitherto unknown, a species of diptera, called by Dr. Coquerel, Lucilia homini-vorax. In the cases formerly detailed, the frontal sinuses and the nasal fossæ were the parts affected. In the present instance, gangrene of the pharynx and of the nasal fossæ, and death, resulted from the development of the larvæ of the lucilia in these parts. By keeping the larvæ and watching their transformations, the additional proof has been obtained of the correctness of the opinion adopted concerning them.

IV. *Exophthalmia as a Symptom of Disease.* By C. E. Fleming, M.D. (Charleston Medical Journal, January, 1859.)

Numerous observers have drawn attention to the fact that in anæmic individuals a peculiar prominence of the eyeball takes place, unaccompanied by any palpable disorganization of the structures in or about the globe. It has been attributed to a deposition of fat, or to varicose veins behind the eyeball, to atony of the recti muscles, to enlargement of the globe from diminished tension of the sclerotic.

Dr. Fleming has investigated six cases that have fallen under his notice, which presented the following symptoms: anæmia was first established to a greater or less degree; functional derangement of the heart ensued, entailing organic change, especially dilatation. The thyroid gland next enlarged; and, lastly, the eyes began to protrude, and well marked exophthalmia, or pro-ptosis, as it is also termed, was ultimately established. In all his cases the patient had previously suffered from rheumatism, upon which the author lays great stress, inasmuch as he regards the rheumatism as the real origin of the whole chain of phenomena. Dr. Fleming holds that the protrusion of the eye is due to the loss of tonicity in the muscles; they become weak and unable to perform their functions properly, one of which he maintains is, that when they contract together, they draw the globe back upon the cushion of fat in the posterior part of the orbit, so as to diminish the antero-posterior diameter. Dr. Fleming quotes two cases of strabismus in support of his view. In one the internal rectus was divided; the eye then turned outwards by the action of the external rectus and was protruded quite perceptibly; in the other the internal rectus was first divided, which caused slight protrusion, and when, in order to rectify the outward squint that followed, the external rectus was also divided, a very considerable protrusion of the eye occurred. In order still further to illustrate the subject, Dr. Fleming made some experiments upon dogs. On dividing the internal and external recti alone, he found that the eye respectively turned inwards or outwards with scarcely any perceptible prominence, but that when all the muscles of the eyeball except the external rectus were divided, the eye almost immediately shot forwards, being also rolled outwards by the undivided muscle.

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* See British and Foreign Medico-Chirurgical Review, Oct. 1858.

The author considers that in these operations and experiments an analogy exists with the state of exophthalmia of anæmia, but we are disposed to think that the free movement and parallelism of the eyes which exist in the morbid condition under consideration contradict such an assumption, and that Dr. Fleming's explanation is therefore incomplete.*

The author advocates the usual medicinal and hygienic treatment adapted for anæmia as suitable to anæmic exophthalmia, but he does not state that he has actually succeeded in restoring the eye to the normal condition and appearance by such means.

V. *Practical Communications.* By Dr. Franz Innhauser. (Zeitschr. der k. k. Gesellsch. der Aerzte, No. 51.)

From these communications we extract the following case of variola occurring three times in the same individual:—In 1857, a maidservant, aged forty-five, who was attending a patient of Dr. Innhauser's labouring under genuine variola, had been repeatedly vaccinated in childhood without any result. When twelve years old she was infected by genuine variola, leaving large, deep, confluent cicatrices. In 1855 she was again attacked with variola, in consequence of nursing a man labouring under true variola. It was severe, and the cicatrices were readily distinguishable from the marks left by the first seizure. Three weeks after the recovery of her mistress in 1857, she was attacked a third time with variola, numerous pustules formed, but no cicatrices.

VI. *On a Musical Bruit, not yet described, heard at the Middle and Lower Part of the Sternum in a Man affected with Cirrhosis of the Liver.* By L. Lemaire. (L'Union Médicale, Jan., 1859.)

The following case occurred in the clinical wards of M. Bouillaud:

Joseph Venn, aged forty-three, admitted into the Charité, Sept. 6th, 1858, had always enjoyed good health, with the exception of slight bronchitis seven years ago; until two months before admission, his abdomen was seen to enlarge; loss of appetite and diarrhœa followed, with thirst and loss of strength. On admission the abdomen was swollen, tympanitic above, fluctuating at the inferior part; the dulness of the liver was slightly increased, that of the spleen considerably. The tongue moist; the pulse eighty, regular; the heat of the skin normal. The heart occupied its normal limits; the valvular sounds entirely normal (by an evident misprint they are stated to have been *tout à fait anormaux*), without any murmur. Over the middle and inferior part of the sternum, over the right cavities of the heart, a musical sound was heard, which so closely resembled a sibilant râle as at once to suggest the presence of bronchitis, but on examining carefully the different parts of the thorax, the vesicular murmur throughout was found soft, and unaccompanied by any kind of râle. The bruit was continuous, and not insochronous with the beats of the heart. It was increased at each inspiration, and it increased in intensity as respiration became accelerated. The bruit continued when respiration was arrested. A strong continuous blowing murmur was heard in the vessels in the neck. The bruit continued on the 8th, on the 9th it changed to genuine *bruit de rouet*, which was heard in the præcordia, and which increased in intensity the nearer one approached to the xiphoid process; it varied in character, but was persistent, even when respiration was arrested. The bruit became feebler as death approached, which occurred on the 24th. Bouillaud declined offering an opinion as to the cause of the sound. The post-mortem threw but little light on the subject; the heart and chief arteries were perfectly healthy, nor was there any marked abnormity in the lungs. The liver was in a state of advanced cirrhosis, but the fossa of the vena cava was almost obliterated, and the vein seemed to have been compressed between the edge of the liver and the spine. It is possible, according to the author's opinion, that this compression was the cause of the abnormal sound; but it was by no means proved that this compression really existed.

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* Mr. Dixon (On Diseases of the Eye, second edition, p. 318) observes in reference to this question, that atony of the recti muscles might produce a certain amount of prominence, but it is hardly compatible with such free motion as usually exists; and a shortening of the *levatores palpebrarum*, such as would account for much of the seeming prominence, would hardly allow of complete and easy closure of the eyelids. An interesting historical and critical article on the subject of exophthalmia, may be found in the 'Gazette Hebdomadaire de Médecine,' April 8th, 1859, in which we find the proposal of Dr. Hirsch supported that the affection should be called the malady of Basedow, who was the first to write on the subject.

VII. *On the Foramen Ovale of the Adult.* By Dr. H. Wallmann. (Vierteljahrsschrift für die Practische Heilkunde, xvi. Jahrgang, 1859.)

After a short inquiry into the illustrations afforded by comparative anatomy regarding the foramen ovales, the author states that he has examined the permeability of the fossa ovalis in 300 autopsies of persons of all ages, and that in 130 of these he found it open. Dr. Klob, who recently read a paper on the same subject at Bonn, found the foramen ovale patulous in 224 out of 500 subjects, a proportion that corresponds closely to that of Dr. Wallmann. These numbers, it is to be observed, give a higher ratio than Bizot's,* who found the foramen ovale open in 44 out of 155 cases not affected with cyanosis, or about 28 per cent. Of the 130 cases of Dr. Wallmann, 9 were under twenty years of age, all the rest were adults ranging up to ninety-three years, the majority (or 95), however, being under forty; 105 were men, 25 females. The patients died of the most various diseases—we merely note the relative frequency and nature of the cardiac complaints—hypertrophy of the left side of the heart, 14 times; dilatation of the right side, 9 times; insufficiency of the aortic valves, 4 times; insufficiency of the mitral valves, once; vegetations on the mitral valves, twice; pericarditis, 4 times; pericardial adhesions, 5 times; atheroma, 15 times. The only point suggested by the author in reference to cyanosis that we need advert to, is that the cyanosis occasionally occurring shortly before death, may be favoured by the presence of a patulous foramen ovale.

VIII. *New Researches into the Nature of Hysterical Convulsions.* By Dr. Briquet, Physician to the Charité, &c. (Archives Générales, June, 1859.)

The author's object in the present paper is to prove:—1. That hysteria consists of a series of allied acts, and that the various phenomena associated with it, however contradictory and incongruous they may appear, depend upon certain simple pathological laws. 2. That in the great majority of instances one is able to distinguish clearly between such attacks and those of epilepsy and eclampsia. 3. That we possess means sufficiently powerful to arrest them when we choose. Dr. Briquet analyses 254 cases, in which he was able to obtain accurate details, and finds that in 48 cases only the first attack occurred without the presence of an appreciable exciting cause, or could be regarded as resulting from a progressive increase of the hysteria; that in 206 cases it was brought on by an exciting cause, and that these causes were as follows: emotions, 54; fright, 47; disappointment, 30; painful menstruation, 23; illtreatment, 16; witnessing convulsions, 9; venæsection, 8; anger, 4; convalescence from serious illness, 4; magnetisation, 2; and with equal frequency, coïtus, application of the speculum, cauterization of a chancre in the vagina, extraction of a tooth, a fracture, an attack of intermittent fever, and erysipelas of the face.†

Dr. Briquet combats the Hippocratic doctrine that hysteria is essentially of uterine origin. He finds that in all hysterical convulsions pain appears to emanate from some point of the body; and that out of 221 patients whom he examined minutely on this point, only 2 appeared to indicate the uterus as the *point de départ*, whilst in 165 the epigastric region was the seat of diseased sensation, in 29 the head, in the others the neck, the extremities, or some part of the trunk. After analysing the various symptoms of a complete attack of hysterical convulsions, he concludes that it is nothing more than a manifestation of those acts by which the emotions and painful sensations are made palpable; and that the convulsions are a secondary effect resulting from the pain, and their character varies according to the susceptibility, habits, age, constitution, or the passions of the patient. "Hysterical convulsions are nothing but the unregulated involuntary repetition of the complex movements which may be executed during the ordinary acts of life; the mental disturbance is merely a repetition of the moral impressions or of the ideas which have influenced the brain in its lucid condition; it is a reminiscence or reproduction analogous to what we see in dreams." All this has nothing to do with the uterus. The distinction between the convulsions of eclampsia or epilepsy and those of hysteria consists, according to our author, in the limitation of the movements of the former, and in their presenting no analogy with the ordinary acts of life, which is so characteristically the case in hysteria. The other distinctions between the two forms of convulsive action are gone into by Dr. Briquet, but need not detain us. His treatment of the hysterical attack consists in the administration of chloroform by inhalation, so as to produce sleep—a plan which he has adopted for twelve years with almost uniform success, the effect being to arrest the convulsions; neither coma, somnolence, nor dangerous syncope resulting from the proceedure. He also recommends the employment of topical applications of chloroform to those parts of the surface that are painful during the free intervals, by which means the pain itself and the attacks it originates may be arrested.

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* Jones and Sieveking's Pathological Anatomy, p. 336.

† It is to be observed that the numbers do not tally with the author's totals.

IX. *On the Sudden Increase of the White Blood-Corpuscles during the last stage of Cachectic Diseases.* By Dr. Gubler. (L'Union Médicale, July 2, 1859.)

The author relates two cases in which, after the proportion of white and red corpuscles had been observed during a cachectic malady to remain normal for a long time, the number of the former suddenly increased to such an extent as to constitute well-marked leucocythæmia.

The first was a man, aged twenty-one, of scrofulous habit, who six months before admission to the Hôpital Beaujon, under Dr. Gubler, was attacked, in July, 1858, with intermittent fever, which would not yield to quina. He was then treated with *karapa*, a substance that has been proposed as a substitute for quina. The spleen being enlarged, the blood was examined microscopically on the 17th and 20th of March, 1859, when there was no increase of white corpuscles, about a dozen being counted in the field; on the 24th pneumonia of the right lung supervened, and there was now an increase of from thirty to forty in the field; on the 25th, from fifty to sixty were counted. Death ensued from pneumonia on March 27th. The post-mortem confirmed the diagnosis of pneumonia and hypertrophy of the spleen.

The second case was that of a young man, aged eighteen, admitted into the hospital with Bright's disease, April 9th, 1859. He remained for a week, and the blood examined at this time was normal as regards the proportion of white and red corpuscles. He returned in a state of general anasarca a week later, and when the blood was examined on May 13th, the red corpuscles were found to be ill-formed, not forming the ordinary rouleaux; and of the white from fifty to fifty-five were counted in the field, instead of the twelve or fifteen previously seen. The latter had undergone a still further increase on May 13th, and there was then noticed a number of amorphous masses of a white colour and irregular shape, apparently consisting of coagulated fibrin. Death ensued on May 14th. The post-mortem showed the kidneys to be granular and contracted, the liver and heart enlarged, and the spleen atrophied and flabby.

X. *A Case of Progressive Muscular Atrophy.* By Dr. Rodet. (L'Union Médicale, March 3, 1859.)

Although the case related by Dr. Rodet was of a much more acute character than those commonly classed under the name of progressive muscular atrophy, and although the diagnosis was not verified by the galvanic test, it is sufficiently interesting in the present state of our knowledge to deserve notice. A gentleman, aged fifty-six, came under Dr. Rodet's care for a hard chancre, for which he was subjected to mercurial treatment on February 9th, 1856. He soon complained of unusual weakness and dull pains in the legs, about the calves and articulations; but the mercurial treatment was continued, with sundry intermissions. On July 25th, the fingers of the right hand were observed to be feeble and incapable of being completely closed; the thenar and hypothenar eminences had much diminished; the prominence between the forefinger and thumb, when these were closed, had disappeared. The muscles of the forearm were much diminished in size, as well as the biceps and triceps. The right calf and thigh were also atrophied, and the foot of this side was thinner than its fellow. All the affected muscles were the seat of fibrillar movements, that might be compared to feeble electric discharges, the intensity of which in each muscle appeared to be in the direct ratio of the rapidity with which the atrophy had occurred. The patient, who had weighed 115 kilogrammes (about 250lbs.) before his illness, now weighed only 100 (about 225lbs.). The muscular degeneration advanced. In the meantime secondary symptoms made their appearance, and iodide of potassium was commenced from August 29th, together with quinine, and frictions of the atrophied parts with a stimulating liniment. The iodide of potassium was increased up to fifteen grains for a dose, and then gradually reduced. A gradual improvement of all the symptoms took place; the cramps and pains ceased, the strength improved, and on January 29th, 1857, the report states that the patient could walk better and write; that the members of the right side were almost equal to those of the left, and that the thenar and hypothenar eminences had almost recovered their size; the fibrillar movements no longer existed, the head was well, sleep was restored, and the patient had gained two kilogrammes in weight. On June 18th, no atrophy remained; the strength was the same on both sides of the body, and no syphilitic symptom had returned. His weight was then 105 kilogrammes (about 236lbs.), or three kilogrammes more than at the previous report. The recovery was permanent. We may well congratulate Dr. Rodet on his success.

XI. *On Diabetes.* By W. Griesinger. (Archiv für Physiologische Heilkunde, Jahrg. 1859. Erster Heft.)

In an elaborate article on Diabetes, Professor Griesinger inquires into the pathology and treatment of this disease. We extract some of his remarks in reference to the theory that the liver is the organ mainly at fault, which has been specially revived since Bernard's discoveries regarding the glycogenic function of that organ. The author denies the correctness of Andral's observation, that in diabetes there is post-mortem evidence of over-activity in the glycogenic function. In none of his cases was there any enlargement of the liver detected during life, and only in one of five cadaveric inspections was the organ found to be somewhat hypertrophied. In this case it was very convex, with a sharp edge, eleven inches and a half in the longest diameter, six inches in the transverse diameter, and the weight, with the gall-bladder, was sixty-three ounces; on the surface were a few asteroid injections; in the interior, many small, pale, dirty-yellow spots; the tissue generally was exsanguine, friable, and granular, and the gall-bladder contained much brown bile. A careful examination in the other four cases showed no anomaly of the liver. A comparison of sixty-four cadaveric inspections confirmed the author's observation that diabetes mellitus is not characterized by any peculiar morbid condition of the liver. Dr. Griesinger does not appear to have noticed any microscopic changes in the hepatic tissue, and the evidence he quotes with reference to the increase or diminution of the oily contents of the hepatic cells is contradictory; thus Frerichs states that the absence of oil is characteristic, while the reverse is affirmed by Beale; Forster, again, maintaining the tissue to be perfectly normal. It certainly is remarkable that if the liver is the organ mainly at fault in diabetes, we so rarely meet with it directly indicative of hepatic disease. Among 225 cases analysed by the author, there were only two that exhibited hepatic symptoms, both having been preceded by icterus immediately before the appearance of saccharine urine. As an apology for pathology, Dr. Griesinger quotes some apparently contradictory statements by physiologists who have specially worked at the subject, and which it may not be uninteresting to reproduce:

"Exclusively animal diet does not diminish the quantity of sugar in the liver; exclusively amylaceous diet does not increase it." (Bernard.)

"With a mixed diet (partly amylaceous) there is much more sugar in the liver than with an exclusively animal diet." (Stockvis.)

"After an exclusively animal diet there is more sugar in the liver than after vegetable diet." (Figuier.)

"The liver of graminivora contains much more sugar than that of carnivora." (Bernard.)

"There is no material difference in the amount of sugar contained in the liver of graminivorous and carnivorous animals." (Poiseuille and Lefort, 1858.)

A careful examination of the evidence regarding the influence of treatment leads the author to the conclusion that, with the exception of alkalies, none of the remedies commonly used exert any directly curative effect. Nor did the exhibition of the carbonated alkalies do more than reduce the quantity of sugar secreted; they did not arrest the secretion or cure the disease. The details of these therapeutic observations are of much interest, and deserve a careful study. Still, though drugs can do little, the author is willing to admit that by avoiding injurious influences, by a suitable hygienic regimen, and by a generally tonic proceeding, much may be done to improve the patient's condition and to prolong life.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On the Operation for Artificial Anus in Children.* (Bulletin de l'Académie, tome xxiv. pp. 423 and 445.)

A short discussion upon this subject, of some interest, recently took place at the Academy of Medicine. It originated in a communication from M. Rochard, of Brest, who, referring to a former debate, during which some members of the Academy had expressed doubts whether the subjects of operation for artificial anus ever attained the adult age, now adduced five instances in which this had been observed. The reason why M. Rochard has been able to record so great a number of cases seems to be, that since the successful operation by

Duret, in 1793, great numbers of children suffering from congenital deficiency of anus have been brought to Brest. The first case referred to by M. Rochard is the above case operated upon by Duret, the subject dying in 1836 only. 2nd. A woman, operated upon by Serand, in 1813, is still alive and robust, suffering but little inconvenience from the artificial anus. 3rd. A lady operated upon in 1816 is still living at Brest, the mother of four children, and in the perfect enjoyment of life. 4th. A woman, who died at the age of thirty; and 5th, a lad, who died at fourteen, both of causes independent of the infirmity. All these operations were performed by Littré's method. In all the cases there was eversion of the lower end of the intestine. The tumour was only in part reducible, but it was insensible to the touch, and the mucous membrane covering it, in spite of exposure to the air and to contact with the bandages, &c., never became inflamed. M. Robert, who reported upon this paper, agrees with its author in recommending that whenever in a case of imperforate anus no fluctuation can be detected in the ano-perineal region, no operative procedure should be attempted in this direction, but recourse immediately had to forming an artificial anus in the groin. Malgaigne, Velpeau, and others joined in the discussion on the subject, but for this we must refer to the original account.

II. *On Traumatic Lesions of Nerves.* By Professor Larrey. (Moniteur des Hôpitaux, 1859, No. 30.)

In his clinical lectures, M. H. Larrey indicates that wounds of the nerves may give rise to three orders of phenomena. 1. Traumatic paralysis of motion and sensation, whether complete or incomplete, and which may disappear at the end of a certain time, or may remain persistent for an indefinite period, atrophy of the parts being then one of the results. 2. Neuralgias, whether immediate or consecutive, intermittent or continuous, these frequently being exceedingly obstinate. 3. In some rare cases, a true neuropathy is produced—i.e., general nervous accidents of various forms, among which are sometimes observed convulsions of a completely epileptiform character. M. Larrey, relying upon the incontestable proposition that traumatism produces very marked nervous phenomena, which are evidently influenced by the lesion or the cicatrix, does not hesitate admitting a *traumatic epilepsy*. He quotes two cases. A soldier having had the lower extremity of the radius fractured by a ball, was seized, some months after the complete healing of the wound, with well-marked epileptic paroxysms, a distinct aura proceeding from the thumb and the cicatrix. In another case, a soldier, aged twenty-six, of robust constitution, was struck in the haunch by a shell, and hospital gangrene following, the wound required five months to heal, leaving a large, irregular cicatrix, attended with but little pain. In this man, well-marked epilepsy became developed about a twelvemonth after the accident, although prior to this he had not manifested the slightest disposition to it.

III. *On Acute Inflammation of the Membrana Tympani.* By Dr. Kramer. (Gazette Médicale, 1859, No. 18.)

Among 6840 detailed cases of disease of the ear in the possession of Dr. Kramer, inflammation of the membrana tympani occurred in 1687, but it manifested itself in the acute form only in 177. Still, the affection is not so rare as these numbers would seem to indicate, for in many cases it is unperceived by the patients, and in others misunderstood by the practitioner. In the 177 cases it existed on both sides in 13, and in 164 on only one side. It never disappeared on one side to arise on the other, and never occurred successively in the two ears. In 151 cases there was more or less severe pain, this symptom being completely absent in only 16. In 38 only the pain gave rise to febrile reaction. The membrane was perforated in 40 cases, in 34 instances of the 151 accompanied by pain, and in 6 of the 16 painless cases. Noises in the ears were observed in 113 of the patients.

Symptoms.—The affection usually comes on suddenly, and especially at night, after the operation of some local cause, the first symptom being more or less severe pain, which seldom much diminishes until a sanguinolent or serous discharge takes place from the meatus; the secretion of cerumen entirely ceasing until after the inflammation has been relieved. The discharge after awhile becomes creamy or whitish, containing brilliant flakes or even firm false membranes. It seldom becomes purulent and fetid, unless in old cases. Noises in the ear are a very early accompaniment; but neither in intensity, timbre, nor duration have they any relation to this particular inflammation, which always persists longer than they do. The sense of hearing becomes very soon remarkably influenced. Usually the meatus is found healthy; while the membrane presents some shade of redness over more or less of its surface, having lost its brilliancy and concavity. It may indeed be the seat of obvious tumefaction, projecting into the meatus, giving it sometimes the appearance of a polypous excrescence. It is not rare for the membrane to become perforated within the first twenty-four hours, a pin's head aperture being usually situated below.

Left to itself, acute inflammation only terminates favourably in some of the slightest cases; for even after the pain and discharge may have ceased, examination will show that the diseased state of the membrane continues, and the deafness is unrelieved. The inflammation may spread, according to its intensity, to various parts and depths of the cavity of the tympanum, and in very bad cases even to the brain or its membranes. As a general rule, the inflammation passes into the chronic stage. The prognosis of the affection is in general favourable. The diseased part is very accessible to sight by means of the speculum, and for the application of remedies. Suitable treatment may be attended with success, even in cases that have been neglected for weeks. The prognosis is in general, however, much more serious when the inflammation arises during the course of an acute exanthem; and that chiefly because the disease is overlooked until great destruction of the ear has taken place. In scrofula, syphilis, or arthritis, it is also very unmanageable.

Treatment.—Above all things, the membrane must be protected from the impressions of cold and of sonorous vibrations. Cold water must never be employed in cleansing it, cotton wool should be introduced into the meatus, and the patient must be maintained in complete quietude, the room or even the bed being kept when there are severe pains and fever. In all cases the diet must be mild, and cooling purgatives should be administered. When, however, the brain or its membranes are menaced, more active purgation by means of calomel is indicated. It is doubtful whether general bleeding is of utility, even in these cases, and most certainly it is not so in those which have not this complication. The employment of calomel in divided doses until slight salivation is produced, as recommended by Wilde and Toynbee, is also too energetic a procedure in simple cases.

In the *local* treatment we must first see that all abnormal matters be removed from the meatus. In the case of foreign bodies this must be done by the employment of a large syringe; but when purulent secretions have to be expelled, tepid injections by a syringe of vulcanized caoutchouc effect this object with most gentleness. When the pain is great, the canal should be filled with tepid olive oil, and closed with a little plug of wool. These instillations, repeated several times a day, according to the severity of the pain, are attended with a most marked amelioration; the pain, which had lasted for days and nights, diminishing or ceasing at the end of a few hours, enabling the patient to obtain sleep, of which he had long been deprived. When this effect does not result, one or two applications of leeches should be made in front of the tragus or to the mastoid process, allowing the bites to bleed, and applying over the ear emollient cataplasms as hot as they can be borne, and to which some henbane leaves have been added. Irritation is also to be excited beneath the mastoid process by means of an ointment composed of four parts of tartar emetic, four of lard, and half a part of croton oil. These means succeed better than blisters and fomentations.

IV. *On Ulcerations of the Trachea produced by the Canula after Tracheotomy in Croup.* By M. Henri Roger. (Archives Générales, August, 1859, p. 199.)

The following are the conclusions of the author:—Among the consecutive accidents of tracheotomy practised for croup, ulceration of the trachea produced by the canula is of rather frequent occurrence, especially during certain epidemics. Thus, during the first quarter of 1859, 13 instances were observed among 63 cases of tracheotomy, at the Hôpital des Enfants. The ulceration almost always occurs at the anterior part of the trachea, on a level with the lower end of the canula, and is produced by the friction which this produced. Generally there is but one ulceration, although several may form under epidemic influence. Usually oval, and limited to the point of contact, it may in some subjects occupy almost all the circumference of the trachea. The following pathological conditions which may coexist, are mentioned in the order of the frequency of their occurrence: ulceration or diphtheritis of the wound of the neck, double bronchio-pneumonia, tracheitis and bronchitis, suppuration of the surrounding cellular tissue, and multiple spontaneous ulceration of the air-passages. The first symptom which leads to the supposition of tracheal ulceration is a bad condition of the external wound. A black colour of the canula, the fetidity of the breath and of the sputa that pass through the canula, sometimes sanguinolent expectoration, and, in some children, pain at the front of the neck and dysphagia, are the signs which enable us to establish the diagnosis. The condition of the system in croup, and the acute state of the inflammation of the air-passages, must operate in aid of the pressure of the canula, for these ulcerations are as rare after tracheotomy performed for chronic affections of the larynx, as they are frequent, especially in certain epidemics, after the operation instituted for croup. The prognosis is unfavourable, as the ulceration adds to the danger of the case, especially when it goes on to perforation. Still these ulcers do not present so dangerous a feature in themselves as do the ulceration or gangrene of the external wound, which is almost always concomitant, and especially the secondary diphtheritis of other parts of the air-passages. The preventive treatment consists in employing a smaller canula, directed slightly obliquely backwards, so as not to press against the mucous membrane, while by rendering the body of this moveable, as in the canula invented by Luer, it is enabled to follow the movements of the trachea. At as early a period as possible after the operation the canula should be temporarily removed for at least a few instants.

V. *Suggestions of Improvements in Tracheotomy.* By Professor BRAINARD. (American Journal of Medical Science, July, 1839, p. 291.)

1. *Hæmorrhage.*—In order to prevent this, Dr. Brainard proceeds in the following manner: "Having incised the skin and fascia by successive and careful incisions, I press the sterno-hyoid and sterno-thyroid muscles to each side with the fingers, and thus expose the thyroid body. This effected, I pass under the isthmus a carved director or an aneurismal needle. This is followed by a common suture needle, which may be passed with the blunt end foremost, armed with two very strong ligatures. A ligature is then tied very firmly on each side, and the isthmus of the thyroid is divided between them. A little dissection with a blunt instrument divides the trachea to the required extent, and an opening can be made without danger of a drop of blood being drawn into it. The ligatures which have thus been secured serve the purpose of fixing the trachea, if desirable, and they may be tied behind the neck, so as to raise it forward and keep the wound open. I never open the trachea until the hæmorrhage is stopped, and a large surface of it has been quite denuded."

2. *Keeping the Opening in the Trachea pervious without resorting to a Tube.*—"The objections to a tube are twofold—1st, When the operation is performed for the extraction of a foreign body, it prevents its exit; and it is desirable to leave the opening in such a state that the foreign substance may escape whenever it becomes loosened from its situation in the bronchia. 2ndly, In tracheotomy for croup, the prolonged sojourn of the tube has been considered by the most eminent surgeons as a cause of the pneumonias which so frequently are the cause of death. The necessity of using a tube I avoid by the following means: Having denuded the trachea, insert a small suture needle, armed with a ligature, beneath two of its rings. Withdraw the needle, and drawing gently upon the thread, make a semicircular incision on one side, so as to form a valve, readily opened by drawing upon the thread. The opening thus formed can be kept patent, or be allowed to close at will. This is a matter, perhaps, of much greater consequence than might be supposed without reflection. Most surgeons have found their operations for tracheotomy less successful than they had reason *à priori* to expect, and this has been attributed to the direct entrance of cold air into the lungs."

VI. *Case of Ununited Fracture treated by Drilling and Wiring.* By Professor SANDHORN. (American Journal of Medical Science, July, 1859, pp. 101–6.)

This case, one of fracture of the bones of the forearm, occurring in a man thirty-three years of age, came under the author's notice a year after the accident. A seton had been passed on two occasions, and the ends of the bones had been drilled, but no consolidation had been produced. The limb was entirely powerless, not being strong enough to sustain its own weight, while the ends of the fractured bones could be readily felt as the arm bent by the weight of the hand. On the 23rd of December the radius was operated upon. The connecting ligament was cut through, and carefully dissected from the end of each fragment. The fracture was found to be oblique, the lower fragment being separated from the upper by about half an inch. The wedge-shaped end of each fragment was then bored through with a small gimlet at the distance of three-quarters of an inch from the end, and through these holes a stout silver wire (formed by twisting four threads of Dr. Simm's silver wire closely together) was passed, and the ends twisted with strong clasp forceps, until the broken ends were held firmly together in the loop. The twisted ends of the wire projected an inch or more from the middle of the wound. The patient went on without any remarkable occurrence, neither the pain nor swelling of the limb being excessive. From the 20th of January the wire was twisted for its detachment by means of forceps on alternate days—a procedure giving rise to considerable pain—until February 1st, when the small neck of bone that held it giving way, it was removed.

The limb, which had hitherto been kept in a splint, was now surrounded with a bandage formed of mucilage and powdered chalk. Union had evidently taken place, though it had not become firm; and the patient, believing that the fracture of the ulna was also consolidating, declined having it treated. Disappointed in the expectation, he returned March 16th. Although the arm had become quite strong, and the use of the thumb and the first two fingers was recovered, the condition of the false joint of the ulna remained unchanged. The same operation was now performed on it, and by April 1st a large callus had become thrown out around the wire, and union was evidently taking place. By the 20th the consolidation was perfect, and pronation and supination were possessed to a considerable degree. The report comes down to May 21st, when it is stated that the use of the patient's arm was daily improving under the influence of exercise about the farm.

Dr. Sandhorn observes: "The difference in the time of union in the two fractures will have been noticed. In the radius there were no signs of union on the thirtieth day after the operation. In the ulna it was quite evident as early as the sixteenth day, and apparently firm in twenty-eight days. In the radius the wire was so firmly held in the bones as to be removed with great difficulty, and after repeated trials of the twisting process. In the ulna, the wire was removed without force after once twisting. Undoubtedly, one reason why the second fracture united so readily was the immobility secured by the previously united radius. But still another cause, I think, may be found in the fact that in the last operation, besides the holes bored for the reception of the wire, I made several other perforations with the gimlet in each fragment—a feature in the operation which I should be careful not to omit in another case."

VII. *A Rare Form of Fracture of the Lower Jaw, treated by Suture of the Fragments.* By Dr. Kinloch. (American Journal of Medical Sciences, July, 1859, p. 67.)

A man, aged fifty, of weakly habit, and with health injured by dissipation, was admitted into the hospital at Charleston, on June 28th, 1858, with a compound fracture of the jaw on the right side, just in front of the anterior border of the masseter muscle. "All the molar teeth were absent, as a result of age, and the alveolar processes about the seat of the injury had undergone absorption. *The line of fracture divided the bone obliquely through its thickness, the obliquity being at the expense of the external plate of the small or posterior fragment, and of the internal plate of the large or anterior fragment.* The displacement was singular and marked. The small fragment projected inwards and slightly upwards into the cavity of the mouth. The large fragment rode the small one, having retreated downwards and backwards, and its extremity, which was somewhat pointed, could be felt externally under the integument."

After trying in vain various modes of adjustment in succession, and union not having taken place, the author resolved to pass sutures through the fragments. On the 30th of August "a semilunar incision, about two inches long, was made upon the side of the face, the middle of the incision reaching under the base of the jaw. With Brainard's smallest-sized drill a perforation was made through each fragment, the drill being entered on the outside, close to the base of the bone, and about one-eighth of an inch from the rough extremity of each fragment, and made to traverse the bony tissue and the mucous membrane covering it within the buccal cavity. The drill was afterwards thrust between the fragments and turned about, so as to slightly lacerate the intermediate connecting tissue. A stout silver wire was then passed through the perforations in the bone, from without inwards through the posterior fragment, and in the contrary direction through the anterior one; and their ends were tightly twisted together, so as to bring the fragments into secure apposition."

By the 26th of September good consolidation was effected, and the suture, which had occasioned but little suppuration, was untwisted and removed. On the 15th of October the patient left the hospital, with the fistulous opening healed and a good use of the jaw. Dr. Kinloch regards this variety of oblique fracture of the jaw as very rare, inasmuch that Malgaigne (the only writer noticing it) is able to refer to only two instances. In this case not only the position of the fragments, but the absence of the molar teeth, rendered the several forms of apparatus inapplicable.

VIII. *On the Non-reducibility of Fractures of the Long Bones.* By M. Gosselin. (Gazette Hebdomadaire, Nos. 9 and 11, 1859.)

M. Gosselin observes that so generally do the books lay down the law that all fractures are reducible, that a surgeon is at first surprised when he discovers the fallacy of the statement.

The following is a brief summary of the author's views on the subject:—

1. Even in cases in which there is no displacement, and there can be neither altered direction nor shortening, there may still be deformity, produced by a persistent increase of the size of the limb, at the level of, and to a certain distance above and below, the fracture. This deformity is of no great importance as long as the hypertrophied bone is not painful; but M. Gosselin has met with cases in which pain persisted for years, and kept the patients from their occupations for a far longer period than is usually the case after fracture of the leg. He cites two of these cases.

2. *Muscular atrophy* is another cause of consecutive deformity, and sometimes of irremediable diminution in the strength of the limb. Nothing is more common than muscular atrophy after fracture, both in relation to the fractured segment of the limb, and to the segments above and below this. In almost every case a notable diminution of the whole of the limb is to be observed, except in the instance of fracture of the clavicle, which does not seem usually to be followed by muscular atrophy of the limb. The *cause* of this atrophy has been attributed by some to the compression exerted by apparatus, and by others to the prolonged immovability of the limb. M. Gosselin is disposed to search for the explanation in the diverted nutrition of the parts consequent upon the reparative process of the fracture. At all events, he is of opinion that the atrophy does not depend upon causes from the operation of which a surgeon can shield his patients. Thus far, any means he has tried to remove this condition—as electricity, shampooing, &c.—have been of little avail; but this may arise from the patients not deeming the amount of inconvenience they suffer sufficient to induce them to undergo a prolonged treatment.

The displacements consequent on a fracture offer some varieties:—(1) The displacement, according to the direction of the limb, can usually be very well reduced, and it is in relation to it that the intervention of art is usually of utility. Still, the author refers to two cases of fracture of both bones of the leg, in which, in spite of every care, an angular displacement occurred; while certain cases of fracture of the fibula, whatever apparatus may be employed, and whatever care taken, are followed by a little abduction of the foot and slight elevation of its external border. (2) Displacements according to circumference are rarely irreducible. Still this is the case with a considerable number of fractures of the neck of the femur, with permanent penetration of the upper into the lower fragment, rotation outwards not being corrigible by other than imprudent attempts. M. Gosselin has likewise met with three cases of fracture of the leg in which irreducible displacement in the circumference has occurred. (3) Of the displacements which take place according to thickness, some are corrected easily and are not reproduced, others are reproduced again, until prevented by diffused pressure; and others, again, are irreducible, do what we will. In several instances the author has been unable, in fracture of the leg, to place, even with the aid of chloroform, an upper or lower fragment which projected beyond the other. This he attributes to the indentation of the fragments, the teeth not fitting into each other during the efforts at reduction, except as a mere matter of chance. (4) In considering the displacement according to length, besides the part played by muscular action, account has not been taken of the considerable crushing of the bone which results from the reciprocal pressure of the fragments. Here there will be shortening of the bone with impossibility of restoring it, the shortening even becoming augmented by subsequent absorption.

4. *Fractures near Joints.*—The frequency of fractures near joints, and the great liability to them of subjects aged more than fifty years, has been long known; but M. Vollemier, by introducing the term *penetration* in relation to fractures of the lower end of the radius, MM. Hervey de Chegoin and Robert, by demonstrating such penetration in fractures of the neck of the femur, and M. Trélet, by calling attention to intracondylian fractures of the lower end of the femur, have given quite a new impulse to the study of this description of fractures. But still there are wanting a generalization of these new facts and clinical deductions. In fact, these various fractures resemble each other in their mechanism and their lesions. (1) The fracture of the extremity of a long bone may take the transverse direction, and be unaccompanied by any crushing of the spongy tissue. This is the only fracture, indeed, recognised prior to Vollemier's investigations, but it is the most rare. (2) More frequently one of the fragments becomes so forced into the substance of the other that the penetration remains permanent, the spongy substance of the penetrated fragment being completely crushed. If the two fragments be separated, an accidental cavity will be seen to be hollowed out by the penetrating one, the latter usually presenting an irregular or toothed surface, which enters into such cavity. This variety is especially met with at the cervix femoris, and at the lower end of the radius. (3) In other cases, one of the fragments presents the depression and crushing of the spongy tissue, but the other is not lodged in this depression, and is removed from it some millimetres in front or behind—penetration having in fact taken place at the time of the accident, but not being maintained. This disposition is especially met with in the radius, and is more rare than the preceding or subsequent variety. (4) One of the frag-

ments, usually the shorter, may be comminutively fractured—the penetration being more forcible and deeper than in the preceding cases. This variety is observed in stelliform fracture of the radius, in fracture of the neck of the femur when the great trochanter is fractured at the same time, and in fracture of the lower end of the femur, when there are at the same time intra- and supra-condylian fracture. To these cases a proper clinical import has not been given, and fractures are described just as formerly. But how are we to reduce fractures when their fragments are so solidly penetrated and ingrained as to be scarcely separable, even after death; or when one of the fragments has become shortened by crushing or by comminution? It is evident that surgery can do nothing here, and that the limb must remain enlarged and shortened, and that the action of the joint must be impaired.

5. *Therapeutical conclusions.*—It is not the author's object to deter from attempts at reduction of fractures. These, he admits, must be made, and in case of failure repeated. But when complete adaptation cannot be thus obtained, and the failure is explicable on one of the grounds mentioned, attempts should not be multiplied, or complicated and expensive apparatus resorted to. In the author's opinion, a careful and attentive surgeon may obtain with the most simple appliances all possible results. The consecutive deformities or imperfections may be inevitable; and it is an illusion to suppose that in all cases they may be completely prevented.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., Lond.

Physician to the Royal Maternity Charity, Assistant Obstetric Physician to the London Hospital, &c.

I. Physiology of the Unimpregnated Female.

Appearances of the Yearly Ripening of Ova in Woman. By Dr. Mattei. (Gaz. des Hôpitaux, No. 22, 1859.)

Dr. Mattei regards the theory of the monthly maturation of ova concurrently with menstruation as erroneous, and believes that for each ovary only one annual ripening takes place. The months of January, February, March, and April are especially favourable for this maturation. The appearances of this condition come on at times very gently, at others very painfully. The general appearances are alterations of the voice, sleeplessness, at times neuralgias, prostration, vomiting, frequently palpitation, cough, hoarseness, without material change in the breasts. As local symptoms there are, sensation of weight or pain in the abdomen, from the sacrum to the thighs, and especially pains in that side of the pelvis on which the lymphatic glands are swollen and tender; there is also heat and excitement of the external genitals. The menstruation is disturbed; it is seldom rendered more profuse, most frequently more scanty, coming on earlier, and attended with nausea. At times leucorrhœa, diarrhœa, dysuria, sympathetic symptoms in the breasts; the excitation of the ovarian region causes pains, nausea, even hysterical cramps; hematocele, peritonitis, and phlegmon may occur. According to the individual, these symptoms may last for four, twelve, or twenty-eight days, and disappear altogether, or pass into symptoms of pregnancy or false conception. The interval between the ripening of the ova in the two ovaries is variable. The minimum observed by the author was four days, the maximum, five months. Dr. Mattei further says that this yearly ripening mostly ceases at the same epoch as the germination of plants and the rut of animals.

II. Pathology of the Unimpregnated Female..

On the Extirpation of Interstitial Fibroid Tumours of the Uterus. By B. Langenbeck. (Deutsche Klinik, 1. 1859.

Langenbeck refers to the little success attending the medicinal treatment of uterine fibroids; and distinguishing them into sub-mucous, sub-peritoneal, and interstitial, says that the sub-peritoneal are not favourable for removal by operation, on account of the danger of peritonitis. Including three cases of removal by the author, there are twelve cases in which the operation has been performed; in seven, cure ensued. The following is a condensation of Langenbeck's cases:

1. T——, aged thirty-five, mother of two children, had suffered much the last two years from metrorrhagia. After three days' pains a tumour of the size of a child's head had descended into the vagina, and at last beyond this. It was taken for the head. The pains weakened, and the patient collapsed. Langenbeck being called, made an incision through the entire tumour exposed beyond the vulva, and there appeared under the layers of the os uteri a yellowish-white fibrous mass, which could be separated easily, partly with the fingers, partly with the scissors. Hemorrhage slight. The dead child was immediately extracted. The patient died in the following night.

2. E——, aged thirty-seven, but suffered from profuse metrorrhagia. In November, 1856, Langenbeck found her very anæmic. The os uteri was high up, open, with a swelling the size of a hen's egg in the posterior wall. The anterior wall was thinned, the posterior one thickened. After attempts had been made during eight days to expand the os, extirpation of the firmly-fibrous tumour was carried out. The bleeding was inconsiderable. Two months later normal menstruation returned. Recovery was permanent.

3. S——, aged thirty-five; after two normal labours and one abortion, metrorrhagia set in, and produced a high degree of anæmia and weakness. Langenbeck found a tumour the size of the fist in the fundus uteri; the hinder lip of the os uteri was doubled in size; this enlargement extended into the posterior cervical wall. The anterior lip was thinned, the orifice open; and through this the finger felt a swelling the size of a goose's egg, projecting an inch and a half into the rectum. On March 24th, 1858, extirpation was undertaken. The operation was difficult and of long duration, because the tumour was nowhere distinctly bounded, and was very resisting. The hæmorrhage was not considerable. Free suppuration from the uterus ensued; and in August, 1858, the cure was established.

III. PHYSIOLOGY OF PREGNANCY.

1. *On the Causes of the Spiral Direction of the Umbilical Vessels, and the Convolutions of the Cord in the Human Fœtus.* By JOHN SIMPSON. (Edin. Med. Jour., July, 1857.)
2. *On the Transmigration of the Ovum as a Cause of Tubal Gestation.* By Professor KUSSMAUL. (Verhandl. d. Naturl. Med. Ver. zu Heidelberg, Band iv. p. 102, 1858; and Schmidt's Jahrb. No. 5, 1859.)

Mr. John Simpson gives a very interesting physiological account of the forces producing the spiral direction of the umbilical vessels. Our limits impose upon us the necessity of devoting the greater portion of our space to extracts from the more inaccessible foreign journals, and constantly compel us to pass over or to notice with brevity the contributions of our brethren at home. So in this instance we must forbear to trace the train of facts and arguments advanced by the author. The spiral or twist of the umbilical cord is known to be most frequently from left to right. Velpeau considered this to be caused by rotations imparted to the fœtus in the liquor amnii by the movements of its limbs. Schroeder van der Kolk supposes that the greater pressure of the blood in the arteries than in the vein causes a recoil, which reacting on the pubes of the floating embryo, causes it to turn on one side or the other according as they are placed on the left or right side of the umbilical vein in the annulus umbilicalis. The author adduces evidence to show that the direction of the twist is dependent on the structure and distribution of the arterial system of the fœtus, and the action of the heart upon the fluid within its tubes. Until after the eighth or tenth week there is no twist in the cord. At and after this period it will be found that the aorta is parallel with the spinal column in its left side in the dorsal region; then tending, in the lower part of its course towards the mesial line, to divide into the right and left iliac vessels on the bodies of the lumbar vertebræ, thus presenting a curve whose concavity looks forward and to the right. From this crossing of the aorta from the left side of the spine to near the middle line of the body, it is manifest that the *right iliac division has in a considerable degree the appearance and direction of the main trunk*, whilst the left presents more the aspect of a branch. In a diagram from 'Quain's Anatomy,' the author shows that the angles which the right and left divisions of the aorta form with the abdominal aorta are respectively twenty-one and thirty-five degrees, showing that the right follows a course fourteen degrees nearer the direction of the aorta than the left does, and consequently, that it receives a more direct and stronger current of blood.

The author refers to eleven preparations in the Anatomical Museums of Edinburgh which prove this point. Now, the cord being fixed at one end by its attachment to the placenta, cannot yield by twisting to the pulsating force conveyed through the hypogastric arteries; but the fœtus floating freely in a fluid of its own specific gravity, readily gives way to the recoil acting on its pelvis; and from the position of the vessels at the umbilicus, the vein will represent the pivot on which it will move, whilst the right artery, having the greatest power of recoil, will determine the direction of the rotatory movement, that is, from right to left.

Mr. Simpson next applies his theory to the elucidation of the mode of production of convolution of the cord round the neck of the child. This is found almost always in one direction—namely, passing from the umbilicus over the right shoulder of the child, across the nape, then forward by the left side of the neck, and so on, according to the number of convolutions. This is also due to the unceasing pulsation of the fœtal heart continuing the rotation of the fœtus, so that its head passes into a loop of a long funis. If the funis be unusually long, the fœtus, at an early period of gestation, may pass completely through the loops, and thus form true knots upon the funis.

2. Professor Kussmaul describes a preparation of tubal gestation in the eighth or tenth week, in which the left Fallopian tube is developed into a fruit-sac at the point of its entry into the uterus, whilst the left ovary contains no corpus luteum, there being, however, two corpora lutea in the right ovary. The tubes are perfectly permeable down to the point where the fruit-sac is situated in the left tube, where a thick bunch of chorial villi stops the way. This being removed, the left tube is also free. Kussmaul concludes that the ovum developed in the left tube had passed down the right tube, and across the cavity of the uterus; and cites the following instances as similar to this one: 1. The cases of transmigration of the ova from the ovary of one side into the uterine horn of the opposite side in animals with uterus bicornis (Bischoff). 2. The case of transmigration of the human ovum from the ovary of one side into the rudimentarily-developed uterine horn of the opposite side (Scanzoni). 3. The case observed by Drejer and Eschricht at Copenhagen. 4. The observations of the development of the placenta on the one side of the uterus, whilst the corpora lutea were found on the opposite side. 5. The case of Oldham and Wharton Jones, in which the ovum apparently passed directly from the ovary into the tube of the other side, which had become adherent to it, developing itself in the uterine walls, and in which death followed the bursting of the sac.

The violent uterine colics and general spasmodic attacks which women in these cases suffer during menstruation, deserve attention. In the present case, according to Kussmaul's view, the transmigration of the ovum took place through the uterine cramp and antiperistaltic motion of the uterine end of the tube.

IV. Labour.

1. *Contribution to the Knowledge of Gestation outside the Uterus.* By Professor Hecker. (Monatschr. f. Geburtsk., Feb. 1859.)
2. *Case of Extra-uterine Pregnancy complicated with general Anasarca and Ossification of the Uterus.* By R. W. Hardee, M.D. (North Amer. Med. Chir. Rev., May, 1859.)
3. *Case of Tubal Pregnancy, with Rupture and Death.* By G. G. Steele, M.D. (North Amer. Med. Chir. Rev., May, 1859.)
4. *A Case of Placenta Prævia Succenturiala.* By Dr. Kueneke. (Monatsch. f. Geburtsk., May, 1859.)
5. *Laceration of the Right Sacro-iliac Synchondrosis during Labour.* By Scanzoni. (Allgem. Wiener Med. Zeitz., No. 8, 1859; and Schmidt's Jahrb., No. 7, 1859.)
6. *On Uterine Catheterisation, with Catgut for the Induction of Premature Labour.* By C. Braun. (Monatsschr. f. Geburtsk., May, 1859.)
7. *A Case of Spontaneous Rupture of the Uterus.* By Kapler. (Monatschr. f. Geburtsk., *April*, 1859.)

1. Professor Hecker makes a valuable contribution to the knowledge of extra-uterine gestation. He relates in the first place a case which he considers to have been one of *ovarian gestation*—a well-made woman, aged twenty-eight, who had borne a first child three years before. From that time she had menstruated regularly until recently, when conception was suspected. Now often on rising from bed she experienced a peculiar syncopal feeling which impelled her to lie down again. Then retention of urine appeared, and on vaginal examination, the os uteri was found pressed against the pubes, and the post-uterine region was filled by a firm elastic swelling resembling in size the uterus in the third month of pregnancy. This was taken for the retroverted uterus. Great efforts were made to effect reposition; during these collapse took place, and death in half-an-hour.

The autopsy revealed from four to five pounds of blood in the abdominal cavity; the uterus not gravid, much inclined forward, enlarged, its inner surface plainly lined with a decidua; behind it was a cyst, which without regard to its relations was cut out, leaving the uterus *in situ*. The cyst was of the size of the head of a child two years old; its walls were thin, and it bore a great resemblance to a dropsical ovarium. In one spot it was rent open; it contained a well-formed male fœtus, whose development corresponded to a pregnancy of eighteen or twenty weeks. Nothing could be detected in the corresponding Fallopian tubes, but at the lower part of the cyst was a swelling that was clearly recognised as the ovary. The idea of an after-union of the cyst with the ovary by adhesions was excluded by the most searching examination; they were evidently continuous and one. (There can be little doubt that the pregnant ovarian cyst was ruptured by the force used in attempting to reduce the supposed retroverted uterus.)

Professor Hecker then prosecutes an elaborate statistical inquiry into the various points connected with extra-uterine pregnancy. His analysis shows that frequently either a long pause in fertility precedes tubal gestation, or there has been previous sterility. That this sterility and the tubal gestation are co-effects of the same cause is proved by dissections.

The lapse of time from the first appearance of symptoms of illness to death was in two-thirds of the cases (48) within twenty-four hours, and in more than half of these in twelve hours.

Of sixty-four dissections, in 37 cases the gestation was found in the left tube, and in 27 in the right.

Of 31 cases, at the time of rupture of the tube, the fœtus was five weeks old in 1 case, six weeks in 5 cases, six to seven in 4 cases, six to eight weeks in 5 cases, beyond three months in 9 cases, beyond four months in 6 cases. Reckoning from the cessation of menstruation, the gestation had lasted from four to six weeks in 6 cases, six to eight weeks in 5 cases, three months in 2 cases, four months in 1case, five months in 1 case. Thus in far the greater number of cases the fatal catastrophe happens within the first eight weeks.

The Professor believes a common cause of tubal gestation to be inflammatory adhesions impeding the free course and connexion of the tubes with the ovaries. He supports this opinion by eight dissections, in which adhesions, the result of partial peritonitis, were formed, by the fact of the frequent sterility antecedent to tubal gestation, by the well-known sterility of prostitutes, which follows upon colic—*the colica scortorum.*

Attendant changes of the uterus are frequent. Thus of 40 cases there was marked enlargement of the uterus in 24. The development of a decidua is mentioned in 25 instances.

I. A case of tubal gestation is then cited, which we reproduce. A woman aged thirty-three had borne five children, and in the course of her sixth pregnancy, suddenly died under symptoms of internal rupture. In her abdomen was found a fœtus of four to five months, which had escaped from a rent in the right tube. The uterus was five inches and a half long, three broad, provided with decidua and gelatinous plug. The right Fallopian tube which had carried the fœtus was not, as is usual, united to the uterus between the fundus and body, but between the body and cervix; and there was this remarkable circumstance, that the corpus luteum was found not in the right, but in the left ovary.

II. *Interstitial Gestation.*—The total number of known cases of development of the ovum within the uterine portion of the Fallopian tube is 26. In all the course was fatal, but the probability of a more protracted duration is greater than in ordinary tubal gestation. The time of survival after setting in of fatal symptoms is twenty-six hours in 16 cases. In the majority of cases here also the gestation was on the left side. The duration of gestation was generally less than three months. Two cases in which the duration exceeded this time are cited. In the majority of cases it appears that the cavity formed in the walls of the uterus for the ovum is shut off by a distinct septum from that of the uterus proper.

III. *Abdominal Gestation.*—Professor Hecker has collected 132 instances in which the ovum had attached itself to some spot in the abdomen. In 90 out of 105 cases the patients were pluriparous. Of the 132 cases, 76 ended in recovery, and 56 in death. Thus the prognosis of abdominal gestation is more favourable than might be supposed. The fœtus may preserve its life for a long period. In Schmitt's case, the abdominal gestation lasted three years, and the child was, on the death of the mother, through the Cæsarean section, taken out asphyxiated but living; in the case of Grossi, the movements of the child were felt for twenty-three months.

A remarkable physiological phenomenon is the frequent appearance of labour-pains when the abdominal gestation has lasted nine months. That these pains have their seat in the uterus is probable from the development this organ mostly, but not always assumes; but it is more probable that these pains arise in the abnormal sac which holds the fœtus. Frequently it is found that the walls of this sac are partly composed of organic muscular fibres. Recovery took place after the expulsion of the fœtus in 28 cases, through stony conversion in 17 cases, after expulsion of the fœtus through the anterior abdominal wall in 15 cases, after abdominal sections in 11 cases, after vaginal incisions in three cases, and in two cases by modes not well determined.

The termination of abdominal gestation by discharge of the fœtus through the rectum is frequent and mostly a very tedious process. The elimination of the fœtus through the abdominal wall is generally a still slower process; for in 15 cases there are 7 in which the women went through one or more intra-uterine pregnancies before being disburdened of their extra-uterine child. But when the process is begun, its course is shorter, and admits of being assisted by art, as by dilating the perforated opening and extraction of the fœtus.

The following is a summary of the 56 fatal cases: death followed hectic in 18, peritonitis in 12, rupture and hæmorrhage in 7, fæcal vomiting in 2, dropsy in 1 case. It followed operative measures in 12 cases—namely, Cæsarean section 5 times, puncture of the sac and cauterization 4 times, section through vagina in 2 cases. In 5 cases the mode is undetermined.

Professor Hecker then relates the two following cases which came under his own observation:—

1st. A woman who had borne one child at the age of eighteen, began to complain eighteen years later of nausea, want of appetite, and a sense of weight and fulness in the abdomen; menstruation, however, being regular. Two months later than this, December, 1856, her illness became aggravated, and the abdomen enlarged, being painful on moving. Examined on the 17th of January following, she was excessively emaciated and in a hectic condition; the abdomen was so painful, that scarcely could the slightest touch be borne. It was ascertained that a hard body, of irregular form, was present in the right side, feeling like a fœtus; the uterus appeared to contain nothing, and a smooth elastic body was felt behind it, which could be pushed upwards. The diagnosis of extra-uterine gestation was confirmed on the 9th of June, before which time, movements of the fœtus were perceived by the patient and by others; on laying the hands on the abdomen, the different limbs of the child could be made out. The movements had now ceased, and it being concluded that the child was dead, the Cæsarean section was set aside. About Christmas, 1857, pains in the abdomen returned with hectic. In the night of the 8th of March, 1858, suddenly a strong effort at defæcation occurred, followed by discharge of half-a-pailful of watery yellow fluid, without admixture with fæces. This discharge, in all probability, of liquor amnii, caused a considerable collapse of the before distended abdomen. Great prostration attended; and an abscess opened below the navel on the 26th of March, through which came two cranial bones, and afterwards the rest of the head and the whole child in a putrid state. The woman died in two hours later. No inspection permitted.

2nd. A woman, aged thirty-eight, who had borne three children, was admitted into the Lying-in Hospital of the Berlin Charité on the 21st of March, 1857. She had believed herself pregnant since October, 1856, and complained of much pain in the abdomen. The abdomen was enlarged as in a seven months' pregnancy, very tender to the touch, and so evenly distended that nothing distinctly could be traced, but movements of the child were sensible both to the eye and to the touch. The fœtal heart could not be heard, but a very loud vascular rush was heard to the left of the navel. There was colostrum in the breast. Internal examination was so painful that it had to be carried out under chloroform. The os uteri was close behind the pubes, open, and the fingers struck upon a fatty mass feeling like placenta, which gave a carcass-like smell. The posterior vaginal roof was deeply depressed into the pelvis by a round immovable body like a child's head; this, when examined by the rectum, appeared to spring from the sacrum. On the 25th of March the fœtal movements ceased, on the 26th peritonitis suddenly set in, and death followed on the 27th. Immediately afterwards, a dead female child, thirteen inches long, was removed by abdominal incision. The autopsy was performed by Virchow. It revealed recent and universal peritonitis; the extra-uterine sac reached to the transverse colon, was united to the anterior abdominal wall, but elsewhere free. The uterus was much enlarged.

2. Dr. Hardee relates an interesting case of tubal gestation. He was called to a negro woman who had general anasarca. On examining the abdomen, he felt a large tumour resting upon the left side; the uterus presented the sensation of a hard bony mass; no os tincæ could be felt. It was reported that the tumour had been growing for fifteen years. The dropsy increased rapidly, so that repeated tapping became necessary before her death. On laying open the abdomen, the uterus and a fœtal head, larger than at term, were brought to view. The head resting just before the heart, and on the left side of the body; it was firmly attached to the uterus and intestines. After moving the head, a decayed mass was seen, but what it was could not be determined. All the bones of the fœtal head were present, with the exception of the superior and inferior maxillaries. The uterus was about ten inches long, about four inches wide, and four thick, forming one hard bony mass, weighing six or eight pounds.

3. Dr. Steele's case of tubal pregnancy is highly interesting both in a diagnostic and pathological point of view. He was called to a servant woman, aged about twenty-six, who had married a second time—having had a child some years before—two months back. Two

weeks prior to her death she had missed her courses for the first time, and suffered no pain up to the night of the 17th (month ?), when she was taken with severe pain of the abdomen, which was supposed to be bilious colic. The next day she said she was better; but Dr. Steele found the pulse small, quick, and feeble; she was still complaining of a pain of a spasmodic character over the whole abdomen. She died suddenly about an hour after. On opening the cavity of the abdomen, there was found effused about a gallon and a half of blood; that in the pelvic cavity was coagulated; in removing this, an embryo of about six weeks was found, lying near a rupture in the middle of the Fallopian tube. Dr. Steele conjectures, that this accident may happen oftener than is supposed. Unless hæmorrhage be severe, recovery might take place without the occurrence of very formidable symptoms, what had passed being unsuspected.

4. Dr. Küneke relates at great length a case of placenta prævia succenturiata. The following are the important facts. A woman, aged twenty-three, said to be pregnant for the first time, was admitted into the Lying-in Hospital of Göttingen on June 4th, 1858. Whilst engaged in field-labour she had been suddenly seized with a profuse flooding, attended with weak labour pains. Forty-five minutes after this flooding she was in the hospital. The os uteri was partially dilated. Auscultation revealed the sounds of the fœtal heart. The child was born alive by means of the natural efforts. The after-birth was removed without difficulty in ten minutes. The placenta consisted of two perfectly distinct parts, the cotyledons being planted in two points of the superficies of the chorion, separated by a space of free chorion between. There was no vascular connexion between the two portions of placenta. The funis was inserted into the edge of the larger oval-shaped portion of placenta; whilst distinct vessels proceeded from the root of the cord to the placenta succenturiata. Without doubt, says Dr. Küneke, this accessory cotyledon had lain over the os uteri, and had given rise to the hæmorrhage.

5. Professor Scanzoni relates a case which he describes as one of laceration of the sacro-iliac synchondrosis during labour. A woman aged thirty-two, who had previously borne one child normally, suffered during her second labour with severe pains, accompanied by the most acute pain in the sacral region. The child was unusually large. After delivery, the patient felt much exhausted, and complained of violent pain in the region of the right hip, running along the back of the thigh to the knee. Morphia internally and locally applied gave no relief. She could not bear the slightest movement without the most acute suffering; and in the seat of pain a crepitation was perceived. On examination of this spot a very painful swelling, the size of the hand, long, flat, and somewhat red, was found in the region of the sacro-iliac synchondrosis. The most intense pain attended lateral pressure on the hip; and also internal examination, through disturbance of the sacral ligaments. Crepitation was felt and heard on moving the patient. The existence of a separation of the sacro-iliac synchondrosis through the long-sustained eccentric pressure caused by the large and hard head of the fœtus in the pelvic canal was undoubted. The antiphlogistic treatment adopted effected little alteration. On the fourteenth day after labour there appeared along Poupart's ligament a swelling the breadth of two fingers, hard, and which by fomentations and poultices was dissipated in a fortnight. Otherwise the state of the disease was the same. For the first time, in the middle of July, gentle locomotive attempts could be made, by resting the body on both arms on the right knee. At the end of July, violent pain returned in the region of the sacro-iliac synchondrosis. The swelling there exhibited fluctuation at the beginning of August; and on the 7th, an incision being made, a pound of thick pus escaped. By means of the probe, the rough bare bones were felt, without, however, the instrument penetrating into the joint. Much relief ensued; when, two days later, about two ounces of limpid albuminous fluid had been discharged, the opening healed, and the patient walked, although somewhat lamely.

6. Dr. Braun proposes another plan, in addition to the many known, for exciting premature labour. He objects to many methods the danger of too easily rupturing the membranes. He employs catgut-bougies of a foot long and two or three lines thick; he steeps the end in soft water to full softening, and passes it, well-oiled, into the uterus, by means of twisting motions, until a length of only one or two fingers' breadth remains in the vagina. Uterine contractions are set up in from six to twenty hours. The bougie never (?) injures the membranes; it may be removed shortly before the rupture of the membranes or the birth of the child. During 1857 or 1858 the author had twelve opportunities of employing this uterine catheterization. By its means eleven children were born alive, five dead; eight mothers recovered, and no one sank from any puerperal process. The modes of death were: one from pneumonia; one from miliary tuberculosis; and two from Bright's disease. In esti-

mating the risk of rupturing the membranes by this method, it is right to bear in mind the author's precautions in using very flexible catgut-bougies and softening them for use.

7. Dr. Kapler's case of rupture of the uterus. A woman in labour with her second child had passed the period of gestation favourably. She had, however, suffered from metritis after her first labour. The pains were good, but labour made no progress; she was therefore taken into the Lying-in Hospital. She was now collapsed; abdomen much distended. The conjugate diameter was shortened by half an inch. The delivery was effected with great difficulty by perforation and turning. The patient died fifty-one hours after delivery. About half a pound of blood was found in the abdominal cavity. The uterus was extremely pale and flaccid. One part had a harder consistency, and on being cut reminded one of the non-pregnant uterus; the cut edges retracted and became concave. The right wall of the cervix was in the form of a long irregular gaping rent. Below this rent was a second smaller one in the portio vaginalis. It is conjectured that the rupture in this case was due to the pressure exerted on parts which had undergone an inflammatory change in the first labour.

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